Preempting Gas Bans: Policy Retrenchment and Credit Allocation

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ABSTRACT

This paper studies the real and financial consequences of state-level laws that preempt future municipal bans on gas infrastructure. Preemption laws are more likely to pass in politically conservative states with weak climate-policy support, highlighting the ideological roots of climate policy retrenchment. Leveraging SBA loans and novel SME energy-use data in a triple difference-in-differences design, we find that preemption laws increase lending to gas-dependent firms through both larger loans and lower rates. Exploiting the guarantee fee discontinuity as additional identification, we show that strategic bunching relaxes for gas-intensive borrowers, consistent with reduced transition risk. We then document real effects, with employment gains concentrated in SME-intensive and highly gas-exposed sectors. Our results suggest climate preemption enables gas-intensive firms to delay the net-zero transition, underscoring how political economy shapes the effectiveness of transition finance.

Keywords: Preemption Laws, Climate Politics, Small Business Loans, Energy Transition.

JEL classifications: Q58, G21, L26, H70

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I Introduction

Climate risk has emerged as a defining concern for financial markets, reshaping capital allocation and long-run investment strategies (Krueger et al., 2020; Hong et al., 2020; Giglio et al., 2021). Yet the decades-long trend toward stricter environmental regulation is increasingly being reversed by a new wave of politically-driven retrenchment. Municipal bans on natural gas, designed to curb greenhouse gas emissions, represent one of the most visible fronts of local climate action in the U.S., and a growing target of state-level preemption laws that block their adoption, creating a sharp and politically-driven reversal of transition policy (Walker and Malmuth, 2024). We exploit loan-level data from the Small Business Administration (SBA) to trace how preemption laws propagate through credit markets. Although lenders are not directly targeted, preemption laws can shape credit supply by reducing credit risk, particularly for gas-reliant borrowers that would otherwise face costly retrofitting requirements. Consistent with a transition-risk channel, we show that preemption eases credit access for gas-dependent firms, enabling them to create and retain more jobs.

We study this channel in the U.S. market for small-business credit for at least three reasons. First, SMEs operate at the technological margin of the transition. Their capital is often long-lived and illiquid, and their financing is collateral- and cash-flow constrained. When policy uncertainty affects the expected useful life of installed capital, SMEs are likely to feel it first as lenders may respond quickly. Second, the U.S. is governed through a fragmented federal system. The legal doctrine of preemption allows higher levels of government to block a lower-level government from self-regulating. Since SMEs typically operate in a single state and lack the scale to relocate quickly, state politics determines whether local climate initiatives "stick".[1] Third, small businesses have been responsible for two-thirds of all new jobs added to the economy, showing the importance of SMEs in job creation but also their potential in sustaining the green transition.[2]

We assemble a new data set that links (i) the timing of state laws preempting municipal gas bans, (ii) local gas-ban ordinances, (iii) loan-level SBA originations from 2016–2024, and (iv) a

[1] Existing evidence suggests that large corporations engage in regulatory arbitrage by shifting their (high-emission) activities to areas with less climate risk Bartram et al. (2022); Li and Zhou (2017); Dai et al. (2021).

[2] For more details, see here: https://advocacy.sba.gov/wp-content/uploads/2022/04/Small-Business-Job-Creation-Fact-Sheet-Apr2022.pdf.

novel, granular measure of industry energy technology built from the U.S. Department of Energy's Industrial Training and Assessment Centers (ITAC). ITACs provide plant-level energy use by fuel type for SMEs across hundreds of six-digit NAICS industries. We use this data to construct an industry-level measure of gas dependence, a proxy for how exposed a firm's technology is to the risk that gas bans would force costly retrofits.

Our empirical design exploits three dimensions of variation. First, about half of states adopt gas-ban preemption laws at some point in either 2020, 2021, or 2023. Second, within a state, firms differ sharply in how much their production technology relies on gas. Third, SBA loan data allows us to observe credit outcomes at the loan level with rich controls for borrower, loan, and lender characteristics. We estimate a triple difference-in-differences (DDD) model that compares changes in credit outcomes for more gas-dependent industries relative to less gas-dependent industries within the same state and month, before versus after the passage of a preemption law, and relative to states that have not (yet) adopted such a law. Importantly, our tightest specifications include state-by-month fixed effects, absorbing any contemporaneous economic, demographic, or political conditions that might accompany the passage of a preemption bill.

We begin by documenting that the passage of gas-ban preemption laws is strongly associated with conservative political orientation: adoption is more likely in Republican-leaning states and where climate-policy support is weaker. In contrast, we do not find a systematic relation with economic or demographic conditions. Interestingly, we also find that a higher share of the population expressing concern about climate change is positively associated with the passage of preemption laws, likely reflecting anticipatory incentives. In fact, all climate preemption laws we observe have moved to prohibit gas bans *ahead* of potential municipal actions. While studying the determinants of preemption laws is interesting from a political economy angle, that does not represent a threat to our identification strategy as the data allows us to exploit heterogeneity in *within*-state policy exposure (Besley and Case, 2000).[3]

[3]When we exclude state-by-month fixed effects from the regressions, we always control for the forces that lead to preemption laws. Moreover, our results are robust to the exclusion of never-treated states from the analysis, suggesting that policy endogeneity does not affect the adequacy of control groups.

The loan-level analysis shows that credit reallocates toward gas-dependent borrowers following climate preemption. Loan amounts increase for SMEs in more natural-gas-intensive industries, and interest rates fall modestly, consistent with a decline in perceived transition risk. The magnitudes are economically meaningful: an SME at the 75th percentile of gas reliance receives loans that are about 3.6% larger after state-level preemption. We find no evidence that these patterns are driven by shifts in borrower composition or loan demand. Moreover, we find no corresponding increase in ex post default, suggesting that lenders are not simply "reaching for yield" but rather updating beliefs about the likelihood of costly regulatory disruption to borrowers' existing technology. Several placebo exercises reinforce this interpretation. For example, the credit response does not appear at bill introduction or at partial legislative passage, and it is not explained by lender-headquarters exposure, which helps distinguish a borrower-technology channel from a bank-regulatory-risk channel.

Next, we probe mechanisms consistent with a transition-risk channel. The lending response is similar to the baseline when we restrict the sample to out-of-state lenders, suggesting the effects are not driven by local relationship lending or by banks' direct regulatory exposure. The effects are substantially stronger for franchise borrowers and for young firms—settings where business models have limited flexibility to adapt and are more exposure to upfront infrastructure decisions, consistent with preemption reducing expected retrofit and disruption costs. In addition, the lending response is nonlinear in the share of gas dependence. The effect is concentrated among firms in the upper quartiles of gas dependence, indicating that lenders react most where exposure to gas bans is greatest. The proportional effect is consistent with a firm's exposure to transition risk. The intuition is that firms with minimal gas reliance would face relatively lower disruptions from gas bans and therefore have little to gain from the removal of those bans. Finally, using alternative energy exposures further sharpen identification, as the effects are insignificant for fuel oil or wood usage, supporting the channel specificity of gas dependence. The lending effect is opposite for electricity-intensive firms, pointing to a high degree of substitution between gas and electricity usage at the industry level.

To further corroborate the transition risk mechanism, we exploit the discrete fee structure of

the SBA 7(a) guarantee program. Guarantee fees increase discontinuously at the $1,000,000 loan size threshold, creating incentives for lenders to bunch loans just below the cutoff to minimize fee exposure while retaining the guarantee (Bachas et al., 2021). This bunching should be stronger for riskier borrowers, for whom the guarantee is more valuable. If preemption genuinely reduces perceived transition risk for gas-intensive SMEs, lenders should become less reliant on the guarantee for these borrowers, attenuating the incentive to bunch below the threshold. Consistent with this prediction, in a regression discontinuity difference-in-differences design that exploits the fee notch as an exogenous source of variation in lender incentives we document that below-threshold strategic bunching relaxes for gas-intensive SMEs following preemption. This evidence is consistent with lower default risk raising risk-adjusted return above notch, offsetting the jump in fixed guarantee fee.

Importantly, we also find meaningful real effects associated with the lending response to climate preemption laws. The data allows to isolate the effect of credit on employment by observing the number of jobs created by each SBA loan. Employing the same DDD design, we find SME employment supported by SBA loans rises in the subsamples where the credit response is strongest, particularly among franchise borrowers and the most gas-reliant firms. This pattern mirrors our credit supply results and supports the interpretation that reducing operational transition risk not only increases credit availability but also allow firms to sustain employment in sectors where retrofitting or technological substitution is costly.

To assess the external validity of loan-level employment effects, we complement the SBA-based analysis with administrative employment data from the BLS Quarterly Census of Employment and Wages (QCEW). Unlike the self-reported job measures in SBA applications, QCEW provides realized employment at the state–industry level. In line with our micro-level findings, we observe that total employment increases in highly gas-exposed industries following preemption laws, including SME-intensive and privately owned employers. These aggregate effects suggest that the documented credit expansion is not merely reflected in projected job counts within the SBA program, but translates into broader labor market outcomes.

Together, our findings contribute to the climate finance literature by shifting attention from how finance adapts to more climate policy to how finance adapts to less, or to the credible threat that climate ambition will be curtailed. The paper also contributes to the political economy of finance by highlighting that political conflict within a federal system can shape capital allocation through jurisdictional control over future regulation (Lambert and Volpin, 2018). More broadly, our results emphasize that "transition risk" is not only about carbon prices and emissions caps; it is also about whether political institutions allow local policy to bind.[4] The welfare implications are ambiguous and precisely for that reason important. If preemption reduces regulatory uncertainty, then easing financing constraints for gas-dependent SMEs may improve investment and employment. But if preemption mainly insulates incumbent technologies from local decarbonization efforts, then it can redirect government-backed credit toward carbon-intensive capital and away from cleaner substitutes, slowing adoption at the local level and potentially undermining the effectiveness of federal climate initiatives.

A Literature Review

The climate finance literature has largely examined how financial markets adapt to policies designed to combat climate change, such as regulatory actions (Ivanov et al., 2024), carbon taxes (Laeven and Popov, 2023), or exit strategies (Green and Vallee, 2025). There is evidence that, primarily after the Paris agreement, investors started pricing or reallocating capital in response to climate policy signals (Krueger et al., 2020; Degryse et al., 2023; Kacperczyk and Peydró, 2022). By contrast, our paper shifts the focus from asset prices and disclosure to credit supply and small business lending in response to policy retrenchment, a different but increasingly salient phenomenon. This retrenchment—often driven by industry lobbying, partisan polarization, or populist rhetoric—frequently takes the form of regulatory rollbacks, budget cuts to environmental agencies, or state-level preemption of subnational initiatives. Studying this policy misalignment offers new insights into the broader interaction between a country's climate ambition and its development, while extending the banking literature on the consequences of political misalignment (Dagostino et al., 2023; Kempf et al., 2023; Igan et al., 2025). While the broader "climate culture war" has attracted significant

[4] See De Haas (2025) for a survey of the literature on how banks respond to climate change.

attention (Hoffman, 2015; Hochachka et al., 2025), its implications for sustainable finance and the economy remain largely unexplored. Our paper contributes to this debate, providing new evidence on the finance–growth nexus in the context of the green economy (De Haas and Popov, 2023).

Given the recent growing political backlash against climate action, it is not surprising that there is only limited work on anti-climate policies. This nascent literature largely focuses on shocks to the *supply* side of financing such as restrictions imposed on institutional investors—documenting effects such as tilts toward brown firms (Tang et al., 2024; Rozhkova, 2024) and higher municipal borrowing costs after Texas Senate Bills 13 and 19 (Garrett and Ivanov, 2024). Our paper shifts the attention to a different class of policies which, unlike investor-focused measures, broadly apply to the demand (borrowers) and introduce a novel way of looking at the lending response to climate risk, stemming from states enacting policies that reduce transition risk.

This paper also contributes to a literature on credit frictions in small business lending. A large body of previous research show that financial constraints are more severe for firms that are smaller and younger, have lower credit scores, or have weak relationship with lenders (Berger and Udell, 1995). Lending decisions to these firms are particularly subject to uncertainty stemming from policy swing. Seminal works by Petersen and Rajan (1994) and Petersen and Rajan (1995) argue that small firms closely tied with institutional lenders face increases in the availability of credit. Related this issue, recent work shows how federal programs can affect the supply of SME credit and employment. For example, Brown and Earle (2017) document the SBA program affect the access to credit and has a large effect on the number of jobs. Howell (2017) shows that the volume of small business lending is highly responsive to loan guarantees. We contribute to the literature on credit frictions in small business lending by identifying a novel channel, technology-related transition risk, through which climate policy uncertainty can constrain credit access. Using the staggered adoption of state-level preemption laws as a natural experiment, we show that reducing regulatory uncertainty around gas infrastructure improves loan terms for gas-intensive firms, highlighting how policy signals shape credit supply even in partially guaranteed lending markets.

II Institutional Background

In the absence of a comprehensive federal climate legislation, U.S. cities and counties have taken the lead in pursuing local decarbonization policies, as local governments are the level of government closest to their communities. Among the most prominent and politically salient measures in recent years are municipal natural gas bans. The primary objective of gas ban ordinance is to accelerate electrification, reduce fossil fuel dependency, and support long-term climate targets, primarily by restricting natural gas in new buildings or incentivizing removal from old ones.[5] Potential health harms of indoor gas usage are well documented (Kashtan et al., 2023; Anenberg et al., 2022; Lin et al., 2013). Therefore, gas bans can enhance indoor air quality in buildings by lowering emissions of nitrous oxide and methane. Indeed, public backing for bans on gas is strongly shaped by perceptions of their health benefits (McLean et al., 2024).

The details of gas restrictions vary by local governments. California is leading with dozens of such municipalities. Berkeley in California was the first city in the US to pass an ordinance banning natural gas hookups in new construction in 2019. This policy, aimed at reducing 37% of city emissions caused by gas, served as a model for over 40 other California cities. Brookline in Massachusetts passed the first ban outside California, restricting gas hookups in new buildings and major renovations in 2019. Recently, New York became the first state to implement statewide gas restrictions that impact local governments across the state.[6] Most municipal gas bans in the U.S. are written to target natural gas specifically, but the legal language and scope can vary. In some cases, they also apply to other gaseous fuels, such as propane (LPG) or synthetic gas.

[5]These bans are designed to lower the greenhouse gas and other pollutants produced by appliances that utilize gas in buildings. Natural gas primarily consists of methane, carbon dioxide, and nitrogen dioxide, a gas known to trigger or worsen respiratory issues. By minimizing the use of gas in buildings, these restrictions are anticipated to lead to a reduction in greenhouse gas emissions, as gas use in buildings accounts for nearly a third of all US planet-heating pollution (https://www.c2es.org/document/decarbonizing-u-s-buildings/).

[6]Following the Building Decarbonization Coalition's policy tracker (https://buildingdecarb.org/zeb-ordinances), we count 75 individual gas ban ("All-Electric Requirement" under Policy Scope) policies enacted at some point between 2019 and 2025.

A Climate Preemption Laws

Gas ban critics argue that gas ban ordinances are contributing to a trend of increasing energy costs, which may worsen issues related to energy poverty. Estimates from industry reports suggest that a natural gas ban could cost every household as much as $26,000.[7] In this context, gas ban policies often collide with the reality of the preservation of "energy choice", creating a patchwork of regulations.[8] The "energy choice" is argued to be a condition for maintaining access to affordable and reliable energy. Unsurprisingly, local governments' actions are countered by state-level pushback, facing legal challenges from industry and federal lawsuits over federal preemption.[9]

The countermovement is reflected into the introduction of climate preemption laws, the focus of this paper. Climate preemption laws are legislative efforts at the state level to restrict local governments from implementing their own climate policies like banning gas or setting stricter emissions rules by asserting state authority. These laws block local governments from enacting measures that reduce greenhouse gas emissions, adapt to climate change, restrict oil and gas development, or support renewable energy development. Climate preemption laws typically prohibit local measures that (1) ban gas hookups, (2) restrict appliances that use fossil fuels, or (3) both.[10] Climate preemption laws take various forms from state prohibitions on natural gas bans to climate-related governance against local governments. Laws preempting local building electrification ordinances now exist in over half of U.S. states.[11] By 2023, 26 states have passed preemption legislation related to municipal gas bans (as shown in Table AII).

In essence, climate preemption marked by state laws bar local efforts to address issues related to reducing fossil fuel dependency and transitioning to renewable or low-carbon types of energy.

[7] For more details see https://www.powermag.com/natural-gas-bans.

[8] Critics come primarily from home construction associations. For example, see www.nahb.org or https://landscapearchitect.com.

[9] See Los Angeles Times on January 16, 2026: "Trump administration sues two California cities over natural gas bans"

[10] For more details, see www.spglobal.com/marketintelligence.

[11] For example, Arizona introduced first the climate preemption law (HB 2686) in 2020. Texas passed the law (HB 2127), as known as "Death Star" law, to preempt local regulations on energy in 2021. At the same year, Florida passed the law (HB 919), preempting local governments from prohibiting the provision of gas service to new buildings. Ohio passed the preemption law that stops local governments from banning gas or propane in new construction.

The implementation of preemption laws tend to be partisan. These laws often tend to target what can be characterized as progressive local laws and create a regulatory vacuum by restricting local regulation.[12] As of 2024, climate preemption laws were anticipatory, enacted before any local gas bans had been proposed—often under pressure from real estate developers, fossil fuel utilities, and industry groups. As a result, the legal landscape governing gas infrastructure in the U.S. has become highly fragmented, with sharp policy divergences between state and local levels, and across states themselves.

This institutional conflict over gas bans not only shapes regulatory outcomes, but also introduces legal and operational risk for firms, particularly those whose operations or products depend heavily on gas infrastructure. It is in this policy context that we examine how state preemption laws affect the supply of credit to small businesses, with a focus on firms in gas-intensive industries that would have faced elevated transition risk from potential gas ban mandates.

III Data Sources and Summary Statistics

We collect data from various sources. Regarding preemption laws, we follow Walker and Malmuth (2024), which cover the period from early 2020 to late 2023 and provide detailed information on bill passage. Figure I shows the timing of the passage of state-level laws preempting local natural gas bans.

Figure I here.

Preemption law passage is based on the date in which the preemption bill passed through the legislative process in both House and Senate. Four bills passed in Arizona, Oklahoma, Louisiana, and Tennessee in 2020. 16 states passed preemption laws in 2021. In 2023, six more states passed the laws. In addition, states marked with dots represent those where local governments, such as cities or counties, have implemented gas bans, yet no climate preemption laws have been proposed.

[12] While many of the state laws operate through the mechanisms of preemption, not all preemption laws fall neatly within climate categories. Climate preemption can be understood as a substantive category of preemption, defined by its effect on local climate action.

A State-level analysis

To study the determinants of climate preemption law adoption, we merge in state-level data from various sources. The main economic and demographic variables include *Population*, *Bachelor's degree or higher*, *Real income per capita*, *Unemployment rate* (in %), and *Tax revenue*. Moreover, we include financial characteristics including *Banking sector depth*, defined as the ratio of bank branches to nominal GDP, to capture banking sector development. Regarding the political variables, we construct *State government trifecta*, which is defined as indicators equal to one if one political party holds the governorship and majorities in both chambers of the state legislature, whereas *Republican-leaning state* is the fraction of the three key state government institutions controlled by Republicans—governor, state senate, and state house. Both variables are constructed using Ballotpedia's party-control classifications. Finally, attitudes toward climate change are from the Yale Program on Climate Change Communication (YPCCC) surveys (Howe et al., 2015; Marlon et al., 2022), including *Climate concerns* and *CO2 regulation support*.

Table I presents the summary statistics for the state-level variables used in the analysis of preemption law adoption. The dataset includes 50 U.S. states observed over the period 2016-2024, with variables capturing political control, demographic characteristics, and public attitudes toward climate policy.

Table I here.

In our sample, on average a state has a *Population* of 8.273 million. 33% of the population has a *Bachelor's degree or higher* education. The mean of *Real income per capita* is 10.912. Finally, 60% of states are somewhat concerned about climate change, while the average share of the population supporting carbon regulation is around 73%, with considerable cross-state variation.

B Loan-level analysis

We rely on the U.S. Small Business Administration (SBA) loan guarantee programs for the lending information. The SBA loan guarantee programs are designed to expand access to credit for young

and small firms that may otherwise be unable to borrow through conventional lending markets.[13] Participating lenders retain primary responsibility for loan underwriting and, in many cases, operate under delegated authority that allows them to assess applications, make lending decisions, set interest rates, and disburse funds without prior approval from the SBA. Because lenders bear a portion of any loan losses proportional to the guarantee share, they remain exposed to borrower risk, which incentivizes careful screening and risk-based pricing within the interest rate limits established by the SBA (DeYoung et al., 2008). Therefore, the characteristics of SBA loans provide a suitable setting for testing the effects of climate preemption laws. As SBA loans assist eligible small businesses, including startups and riskier companies that struggle to secure credit from traditional funding sources, SBA loans are more likely to experience an effect of the preemption law compared to other conventional business loan products by commercial banks.[14]

We obtain SBA loan-level data from Small Business Administration 7(a) dataset, which is accessible under the Freedom of Information Act (FOIA). This dataset provides comprehensive loan-level details including loan amounts, interest rates, and maturities. The dataset also contains borrower details, such operational locations, business ages, but also lender identity. The sample period covers January 2016-December 2024. Following Nguyen et al. (2025), we exclude Financial companies (NAICS code: 52), borrowers located in the states of Guam (GU), Marshall Islands (MH), Micronesia (FM), Virgin Islands (VI), loans that have a canceled status, and those with missing lender identity.

One key contribution of our paper is that we measure the energy intensity of business sectors at granular level. To capture borrower exposure to climate preemption laws, we leverage underutilized energy usage data from the U.S. Department of Energy's Industrial Training and Assessment Centers (ITACs). ITACs provide no-cost technical assessments conducted by university based teams of engineering students and faculty. Assessments are anonymized and collected in a database containing detailed information on energy quantities and prices from more than twenty

[13] The SBA is an independent federal government agency created in 1953 to provide assistance to small businesses. In this study, we focus on the SBA 7(a) loan program. The SBA 7(a) program is the largest small-business lending program in the US. In 2024, the 7(a) program supported 70,242 loans with a total value of $31.1 billion (Nguyen et al., 2025).

[14] Essentially, the SBA loans ensure the flow of credit to financially marginal small businesses.

thousand plant-level assessments across more than 550 distinct 6-digit industries. ITACs data is particularly suitable for our study since it specifically targets SMEs in the US.[15] We exploit the energy-type breakdown (natural gas, electricity, fuel oil, wood, LPG, and other gas) to measure borrowers' exposure to a state preemption law, defined as follows:

$$GasDependence_j = \frac{\sum_{i=1}^{n} Gas_{i,j}}{\sum_{i=1}^{n} TotalEnergy_{i,j}} \tag{1}$$

where *Gas* is the sum of natural gas, liquefied petroleum gas (LPG), and other gas (e.g., propane, butane, or other specialty gases), used by all ITAC-assessed firms in industry j. The gas level is scaled by total energy, defined as the sum of all energy sources (i.e., electricity, fuel oil, coal, wood, paper) used by ITAC-assessed SMEs that operate in the same sector j.[16] The time-invariant variable $GasDependence_j$ is built on all gas types recorded in ITAC assessments, thus it intends to capture the exposure of borrowers to gas-ban preemption laws comprehensively.[17]

Since our measure of gas dependence is defined at very granular level (6-digit NAICS),[18] we assume SMEs within an industry employ relatively homogeneous production technologies. This is motivated by evidence presented in Appendix Figure AI, which reports the R^2 from separate regressions of oue natural gas intensity measure at plant-level on individual groups of controls, using the subsample of plants covered by the ITACs data. Industry fixed effects at the 6-digit NAICS level explain a larger share of variation than state, year, and assessment center fixed effects combined, and about 75% of the variation explained in the full model, supporting the use of industry as the primary dimension of aggregation. As Figure AII illustrates, there is substantial cross-industry heterogeneity in energy use. With granular data on SME energy consumption largely unavailable, our measure of gas exposure provides a novel and essential foundation for the analysis.

[15] There are specific eligibility requirements, such as reporting annual revenue must be lower than \$250M, and yearly energy bills between \$100,000 and \$3,500,000.

[16] All energy sources are reported in Million British Thermal Units (MMBtu), except for electricity which is reported in kilowatthour (kWh). Therefore, we multiply electricity levels by 0.003412 to convert electricity into MMBtu.

[17] Most municipal gas bans in the U.S. are written to target natural gas specifically, but the legal language and scope can vary. In some cases, they also apply to other gaseous fuels, such as propane (LPG) or synthetic gas.

[18] NAICS is the standard used by Federal statistical agencies in classifying business establishments for the purpose of collecting, analyzing, and publishing statistical data related to the U.S. business economy.

After merging SBA loan data, ITAC data, and state-level variables, we present the loan-level summary statistics in Table II:

Table II here.

Our main sample contains a total of 141,817 loans. To avoid the impact of outliers, all continuous variables are winsorized at 1% on both sides of the distribution. In our sample, the mean of *log(Loan Amount)* is 12.307 and the standard deviation is 1.554. SBA loans are structured as term loans, featuring fixed monthly payments that cover both principal and interest. The interest can be either fixed or variable. The average *interest rate* in our sample is 7.546%, which is in line with other SME loan settings. Loans come with a set maturity date. On average, loan term is 144 months, approximately 12 years. An SBA loan enters default when a borrower repeatedly fails to meet the legal terms of their loan agreement. The average *Default* rate is rather high, measuring 3.4%, reflecting the relatively high credit risk of the borrowers. SBA loans, including the 7(a) and 504 programs, typically require business assets as collateral. Depending on the loan agreement, lenders may have the right to seize and liquidate assets such as equipment, inventory, vehicles, real estate, or business-owned property. Unfortunately, we cannot observe the type of collateral, but 80% of SBA loans in our sample have a collateral. The maximum guaranty offered by the SBA varies depending on the type of loan and can reach 85-90% of the total approved amount. The average *SBA Contribution* in our sample is 67.8%. Finally, firms are more reliant on gas than on fuel oil for their facilities, as the mean of *Industry Gas Dependence* is 25% larger than the mean of *Industry Fuel Oil Dependence*. Approximately, 30% of SBA loans are from *Brown Industries*, as defined by SBA,[19] which does not perfectly overlap with our measure of gas share, suggesting that the two variables capture distinct aspects of a firm.

IV Results

In this section, we first examine the determinants of states' adoption of preemption laws. We then present our main results on their effects on the supply and pricing of bank credit to SMEs using

[19] Appendix 6 of the SBA SOP 50 10 8 provides a list of NAICS codes that are defined as Environmentally Sensitive Industries.

loan-level data and within-state variation in gas dependence. Subsequently, we run a series of tests to shed light on the channels. We close the section by examining the employment consequences for SMEs.

A Determinants of Preemption Laws

We begin by exploring the factors that predict state-level climate preemption laws, which prohibit municipalities from enacting natural gas bans. We test the predictive power of demographic, financial, and political factors in a state-year panel dataset using a linear probability model of the following form:

$$PreemptionLaw_{s,t} = \alpha_s + \alpha_t + \beta X_{s,t} + \varepsilon_{s,t}, \tag{2}$$

where $PreemptionLaw_{s,t}$ is an indicator taking value 1 after a preemption bill becomes law after it passed the approval by the second chamber of the state's legislature, as reported by Walker and Malmuth (2024). The vector $X_{s,t}$ includes annual economic, political, or demographic factors of a state that may potentially drive the passage of preemption laws. Our economic and demographic variables include (log) population, the percentage of population having a Bachelor's degree or higher education, real income per capita (in log), and unemployment rate. As a measure of banking sector depth, we include the total FDIC-insured deposits as a fraction of nominal state GDP, while for the fiscal capacity of the state the annual (log) tax revenue. Finally, the (climate) political variables are indicators for a single-party government, and one for republican-leaning states, and the percentage of people in a state who are worried about global warming or who support regulating carbon dioxide as a pollutant. The model is saturated with state and year fixed effects which are intended to capture time-invariant state characteristics and common time trends, respectively.

The sample spans the period 2016-2024 and includes all states except for the District of Columbia, where party control is institutionally different from the rest. Moreover, since the state of Nebraska is unicameral and officially nonpartisan, Nebraska does not enter specifications where political variables are used as predictors. Standard errors are clustered at state level. Table III presents the results of this predictive exercise.

Table III here.

The specification in column 1 shows that preemption is more likely to pass in states with lower educational attainment and unemployment, and higher wealth. The development of the banking sector and state fiscal capacity are not statistically associated with the adoption of gas ban preemption laws. Next, in column 2 we supplement the specification with political factors. Political orientation emerges as a strong predictor; in line with Walker and Malmuth (2024), preemption is more likely in Republican-leaning states. Consistent with political economy explanations, support for CO2 regulation is negatively associated with adoption. The coefficient on broader climate concern is positive, possibly capturing political incentives to incorporate the likelihood of gas bans in the future.

Finally, the last two columns include state and year fixed effects. Interestingly, economic factors lose statistical significance, whereas political factors remain strong predictors. Republican-leaning political majorities are more likely to adopt these laws. Instead, states with higher climate concerns or less supportive of CO2 regulation have a significantly higher probability of adopting preemption. In column 4, these estimates are statistically significant at either 5% or 1% level.

While this predictive exercise confirms our prior that these policies are endogenous, understanding their correlates helps contextualize our identification strategy. Importantly, the main analyses we present in the next sections rely on a difference-in-difference-in-differences (DDD) design which accounts for this selection by controlling for state-by-month fixed effects and leveraging within-state variation in firm exposure to a preemption law.

B Credit Supply

We exploit cross-state variation in adoption timing and within-state heterogeneity in exposure across industries to examine whether gas-ban preemption laws affect credit supply. Our triple difference-in-differences (DDD) design allows us to compare changes in lending to more gas-dependent SMEs after a state adopts a preemption law, relative to less gas-dependent SMEs in the same state and to SMEs in states that have not yet adopted such a law. The regression specification is as

follows:

$$Credit_{i,j,l,s,t} = \alpha_l + \alpha_j + \alpha_{s,t} + \beta PreemptionLaw_{s,t} + \gamma PreemptionLaw_{s,t} \times GasDependence_j + X_{s,t} + X_i + \varepsilon_{i,j,l,s,t}, \quad (3)$$

where the dependent variable is the loan amount (in log) of a loan originated by lender l in month-year t to a borrower i that belongs to 6-digit-NAICS industry j and is located in state s. In secondary analyses, we also use interest rate, default rate, or the log of jobs supported as dependent variables. The $PreemptionLaw$ dummy variable set to 1 if and after a preemption bill is passed by the second chamber of the state s's legislature. The main coefficient of interest is γ which identifies the differential credit supply effect of preemption laws on firms highly dependent on gas usage vis-a-vis those whose facilities use less gas.

The specification also includes time-varying state characteristics $X_{s,t}$ that appeared to be significant drivers of the law passage in Section A (Republican-leaning state, climate concerns, and CO2 regulation support), as well as a battery of loan characteristics X_i, such as dummy variables for the business type (corporation, individual, or partnership), a dummy variable for the use of collateral, the SBA contribution as a fraction of the loan amount, a dummy variable for the loan being sold in the secondary market, and the loan maturity in months. All specifications control for SBA disbursement method $\times$ subprogram fixed effects, as well as for lender l and 6-digit NAICS industry j fixed effects. Moreover, state s and month-year t fixed effects are included, which capture time-invariant differences in state-level credit supply as well as credit-supply trends common across states, respectively. In the most stringent specification, we include also state-times-month fixed effects, which net out all time-varying factors of a state. We cluster our standard errors at the state (policy) level.

Table IV here

We examine whether credit supply to SMEs changes following climate preemption laws, and how this change depends on the business exposure to the law as proxied by the share of gas use. Table IV presents the OLS estimates of the regressions specified in Model 3. The difference-in-difference

coefficient in column 1 is negative and significant, suggesting that preemption law may negatively affect overall credit supply. However, the endogenous nature of the policy, and varying degree of business exposure to treatment are likely to bias this result. To achieve identification, the main specification is presented starting from column 2. We exploit the heterogeneous business exposure to gas ban preemption laws by leveraging the average energy usage of an industry. In particular, our prediction is that larger usage of gas in the facilities is associated with a higher exposure of the firm to a gas ban preemption law. Thus, lenders should take into consideration the effects of preemption law when lending to firms in gas-intensive sectors. Column 2 shows that following a preemption law, loan amounts increase significantly for firms highly dependent on gas. The coefficient can be interpreted as the differential credit supply effect on gas-intensive firms following a preemption law.

A potential threat to identification is that law passage correlates with macro economic conditions (e.g., inflation, monetary policy, etc.). Therefore, in column 3 we control for month fixed effects which absorb all macro shocks common to all states. The point estimate on the interaction term is still positive and significant. Next, we add industry fixed effects to address structural industry differences that may drive the results. The estimate in column 4 is still positive and remain very similar to the coefficients in previous specifications. Finally, in column 5 we interact state and month fixed effects. The coefficient on the interaction between Preemption Law and Gas Dependence in column 5 is 0.057, significant at the 5% level.[20] This corresponds to a 5.7% increase in average loan amount per one-unit increase in gas reliance. For a firm at the 75th percentile of gas intensity (0.64), this implies an increase in log loan size of approximately 0.036, or 3.6%—a meaningful shift in credit allocation. Overall, these findings are consistent with a reduction in perceived transition risk for firms that rely on natural gas when legal threats to gas infrastructure are lifted.[21]

[20]Specification in column 5 is our preferred specification for its reliance on cross-industry slopes within each state-month. In fact, the inclusion of state-by-month fixed effects effectively makes identification specific to *within-state-month* reallocation across industries along the gas-dependence gradient. In Appendix AIV, we show the main result survives specifications that force identification *within state-industry* over time, while flexibly absorbing common industry-time shocks.

[21]Because preemption laws were adopted at different times across states, one concern is that our results may be driven by a specific cohort of adopters. To evaluate this possibility, we implement a leave-one-event-out (LOEO) analysis, sequentially excluding states that passed preemption at a given date and re-estimating the baseline specification. As shown in figure AIII in the Appendix, the interaction coefficient of interest remains stable in magnitude and significance across all iterations. This exercise suggests that the documented credit supply response is not attributable to any single wave of policy adoption. Also, it alleviates concerns that staggered timing, weighting distortions, or

In a DDD design, the identifying assumption is the difference between high- and low-exposure firms would have evolved similarly over time in treated and untreated states, absent the law. We assess the identifying assumption using an event-study version of our triple-difference specification. To examine the dynamic effects of the policy, we estimate event-study specifications using the Borusyak et al. (2024)'s imputation estimator, which is robust to staggered treatment adoption and heterogeneous treatment effects. Also, because loan-level borrower panels are extremely sparse, we aggregate observations to the bank–state–month level to create a dense panel with full support. Banks operate continuously across states and months and therefore provide a natural unit for studying policy-induced changes in lending behavior over time. This aggregation yields a balanced panel structure required by modern staggered-adoption difference-in-differences estimators, while preserving the relevant variation in borrower-state exposure to the policy.

We classify firms into exposure groups based on their sensitivity to the policy. Specifically, firms above the median value of the industry exposure distribution are assigned to the high-exposure group, while firms below the median are assigned to the low-exposure group. This median-based split maximizes contrast between firms that are plausibly most affected by the policy and those that are least affected, while avoiding arbitrary cutoff choices and minimizing contamination from firms with intermediate exposure. We estimate the event path separately for high- and low-exposure firms and plot the difference between the two, effectively implementing a triple-difference event study. The same set of borrower controls and fixed effects of the baseline model are included.

Figure II here

The resulting event plot shows no systematic differences between high- and low-exposure firms in the pre-treatment period, supporting the parallel trends assumption underlying our identification strategy. Following treatment, credit volumes for high-exposure firms diverge sharply from those of low-exposure firms, with effects emerging during the month of policy adoption and persisting for at least eigth months thereafter. This pattern is consistent with the policy having a credit reallocation impact that depends on the policy exposure of borrowers.

cohort-specific dynamics drive the results.

C Robustness Checks

Table V provides robustness checks to support our preferred specification as reported in column 5 of Table IV.

Table V here.

In Panel A, we test several placebo designs, including re-timing treatment at previous dates, assigning treatment incidence based on the state of the lender's headquarter, or testing an alternative measure of exposure. As described in Section B, our main analysis identifies the treatment timing to the month in which the bill is approved by the second chamber (Senate), that is when the proposal becomes law. Instead, the *Preemption Law* variable in columns 1 and 2 of Table V moves treatment date to the bill introduction date, or to the time the law is approved by only the first legislative chamber, respectively. In the data we observe that it can take up to 5 months from bill introduction to a successful passage at the second chamber. Moreover, there are states (e.g., VA or MI) where a bill that is first introduced eventually fails to pass the first or second chamber, in which case the bill does not become a law. We expect lenders to pay less systematic attention to those preliminary events. Keeping the same controls and fixed effects structure of our baseline specification, both estimates are statistically insignificant. This suggests that there are no anticipatory effects in the lending response to a gas ban preemption law.

If preemption laws reduce policy and legal risk for the bank, the credit supply reaction we document should be driven by a heightened perception of changing climate regulations going forward. On the contrary, if preemption laws operate through a technology risk that forces the firm altering their facilities, the lending reaction should be driven by a reduced transition risk for the borrower. In column 3 we run a test that is aimed at separating these two channels. While the timing of state preemption laws is unchanged with respect to the baseline, treatment incidence is assigned based on the state where the bank is headquartered rather than where the borrower is located. The coefficient on the interaction between Preemption law and gas dependence is positive, statistically insignificant. Also, it is substantially lower (about one twentieth) than the baseline. Therefore,

this evidence suggests that lending effects are more likely to be driven by a borrower technology risk channel, rather than regulatory risk for banks. Gas-intensive borrowers often rely on fixed, long-lived capital assets (e.g., gas stoves, water heaters, HVAC systems) that are costly to retrofit or replace. Preventing municipalities from enforcing such bans allow those firms to continue using existing gas-based equipment, signaling to lenders that the firm's current technology is less exposed to regulatory disruption.

Finally, we test the accuracy of our gas dependence variable, that is intended to measure the firm exposure to a preemption law. We augment the model with the interaction between preemption law and an alternative proxy of high-polluting firms. We assign firms in "brown" sectors based on the list of environmentally sensitive industries produced by the SBA in its Lender and Development Company Loan Programs.[22] We interact *Preemption Law* with *Gas Dependence* as well as with the *Brown* variable in the same regression to see which coefficient reflects the lending effect the most. In column 4, the coefficient on the interaction between *Preemption Law* and our measure of *Gas Dependence* is positive and statistically significant, while the one on *Brown* industry is insignificant. This confirms that climate preemption targets specific type of firms based on the energy type they use, and does not generally improve the reputation of brown firms.[23]

Figure III here

The United States is by design, if not always in practice, a federal republic of "dual sovereigns" with the state and federal governments remaining independently competent to legislate in their respective spheres. Federal preemption of state law takes a variety of forms. As additional placebo tests, in Figure III we show that the DDD coefficients on firm gas usage interacted with state-level preemption in *non-gas* ban domains are overall not statistically significant.[24] We source the exact

[22]For the full list of environmentally sensitive industries, please see here https://www.partneresi.com/resources/references/standards-regulations/sba-sop-naics-codes/.

[23]To further test the validity of our gas dependence variable, we verify in table AIII that the results are robust to restricting the computation of gas dependence to assessments conducted after 2010 and after 2015, ensuring that the measure reflects relatively recent energy use patterns. They are also robust to dropping NAICS codes with fewer than 5 assessments — the median number of assessments per sector in our sample — which addresses concerns about sectors where the average may be imprecisely estimated due to limited coverage.

[24]One exception is the coefficient on election policies (10th coefficient), which appears weakly significant, potentially driven by the correlation with the timing of climate preemption law.

timing of the state law passage from the LawAtlas.org project maintained by the Center for Public Health Law Research (CPHLR) at Temple University's Beasley School of Law in Philadelphia. The ten domains of alternative preemption laws are related to the social determinants of health: (1) Ban the Box, (2) firearms, (3) mandatory inclusionary zoning, (4) municipal broadband, (5) mandatory paid leave, (6) rent control, (7) transgender rights, (8) local law enforcement budgets, (9) race and racism in school curriculum, and (10) election policies. All in all, none of these alternative treatment timing, assignment, or exposure definitions yield significant results, reinforcing the idea that the lending effect identifies a reduction in transition risk through a technology channel, rather than regulatory uncertainty or legal risk.

In Panel B of Table V, we test the main mechanism in subsamples. First, in column 1 we exclude from the control groups borrowers located in states where counties or cities ban the use of natural gas. Although Figure I shows that there is no overlap in terms of gas bans and preemption laws, a valid concern is that credit supply is affected in the subset of control states that experience local gas bans.[25] After excluding gas ban states from the control group, the DDD coefficient in column 1 of Panel B in Table V is still positive, statistical significant, and similar in magnitude to the baseline coefficient.[26]

Next, column 2 excludes preemption laws that passed in 2020 from the list of treatments. In 2020, there was the Covid pandemic which may confound the results since various public policies were introduced to support SMEs. If anything, the coefficient increases in magnitude. The point estimate is 0.068 and statistically significant at 1% level. In column 3, we exclude counties near state borders to address spatial policy spillovers that may contaminate our estimates. In fact, a city near the border may be economically influenced by a neighboring state's climate or energy policies. Also, businesses in border counties may operate in cross-state labor or utility markets, making them less responsive to within-state-only regulation like preemption laws. We find a coefficient of

[25] In Table AI in Appendix we analyze the effects of actual local gas bans (i.e., cities/counties implementing electrification rules). Results are mixed, weakly suggesting that following a municipal gas ban lenders reduce credit to gas dependent firms. However, given the smaller sample and local variation in enforcement, we interpret these results cautiously.

[26] Excluding all never treated states from the analysis produces qualitatively identical results (not reported for brevity).

0.076, significant at 1% level. Therefore, excluding counties near state borders preserves and even strengthens the significance of the credit supply effect.

The observed credit supply effects might reflect broader political ideology or a pro-business regulatory environment—not the specific effect of preemption laws removing gas-related regulatory risk. To address this concern, we exclude states that are deeply Republican states. Using the Shor-McCarty state legislator ideology data (Shor, 2020), we exclude states where both ideology scores of the Republicans in the Senate and House of Representative at the moment of preemption law passage were higher than the respective median, states where climate preemption might be "anticipated" and already priced in. As expected, we find that the DDD coefficient is positive and statistically significance, and even strengthened in magnitude.

Our identification strategy relies on the assumption that demand does not shift following a gas ban preemption law. For instance, prior evidence indicates that local house-price appreciation gives entrepreneurs more valuable collateral allowing them to use more debt (e.g., Schmalz et al. (2017)). If natural gas bans affected house prices, the climate preemption could improve the value of collateral for entrepreneurs and SMEs, and in turn credit conditions. To rule out credit demand effects, Table VI investigates whether borrower-side characteristics change following preemption laws.

Table VI here.

We find no significant effects of preemption laws on the legal form of the borrower (corporation, individual or partnership), on the use of collateral, on secondary market sale, on the SBA contribution share, or loan maturity. Even though all our specifications include borrower and loan characteristics, the null results reported in Table VI support the interpretation that our findings reflect changes in credit supply, not shifts in borrower composition or loan demand.[27]

[27]To formally rule out the collateral channel (e.g., Schmalz et al. (2017)), we regress the Federal Housing Finance Agency (FHFA) house price index, seasonally adjusted, at state-quarter level on the preemption law dummy, state and quarter fixed effects, as well as state-level time-varying controls (same as those in Table III). We plot the event-study coefficients in Figure AIV. The DiD coefficients are insignificant post-adoption (t=0), suggesting that the adoption of climate preemption laws has no effect on house prices.

D Transition-Risk Channel

To shed light on the underlying mechanisms driving the credit supply response to gas ban preemption laws, Table VII explores a series of heterogeneity analyses and further subsample tests.

Table VII here

First, similarly to the exercise in column 3 of panel A in Table V, we isolate the effect of the climate preemption law on borrowers by focusing on loans made only by out-of-state lenders. In fact, the lending effect may be confounded by the fact that preemption laws affect lenders directly, e.g., through a regulatory risk channel. In column 1 we exclude local lenders, and we find a similar estimate to the baseline, in terms of both statistical significance and economic magnitude. This suggests that the effects are not confined to relationship lending or local informational frictions.

Results become even stronger when we focus on franchise borrowers (column 2). The effect size nearly doubles to 0.133 in that subsample, suggesting that preemption law matters more for firms with franchise-level infrastructure. This may support the interpretation that gas ban preemption laws reduce technology risk, particularly for businesses that face strict infrastructure and equipment standards. Because franchisees have limited flexibility to adapt their operations to changing local energy regulations, lenders may view them as especially vulnerable to gas bans. Preemption reduces this exposure, leading to improved credit access.

Next, in column 3 we focus on young firms, defined in SBA as startups, for which the loan funds will be used with the purpose of opening the business. Such firms lack established cash flows or collateral, so any perceived increase in risk (e.g., future retrofitting costs or operational disruptions due to local gas bans) can significantly tighten credit. Using the same specification as the baseline, we find a significant credit supply response to preemption laws for this type of firms. The economic magnitude of the coefficient doubles. Therefore, this suggests that removing that risk via preemption unlock more credit growth for young firms.

Column 4 tests for nonlinear effects by interacting treatment with gas dependence quartiles. We find that the coefficients increase with the firm exposure to the law. The effect is concentrated in

the upper two quartiles of gas dependence, indicating that the credit response intensifies with firms' reliance on gas infrastructure. This evidence is in line with transition risk theory. While firms with moderate gas reliance may have more flexibility in adapting operations, firms in the top quartiles of gas intensity may face larger upfront capital costs if forced to electrify, greater risk of stranded assets, and higher policy sensitivity due to fixed infrastructure. Therefore, this non-linearity offers indirect evidence that lenders are pricing policy risk related to infrastructure misalignment, not just responding to general political environment or macroeconomic changes.

Finally, to test whether our results are truly driven by gas reliance, we replace the gas dependence variable with alternative energy dependencies. Almost full breakdown of energy sources are included in the same regression except for natural gas, as electricity and natural gas shares are strongly negatively correlated (–0.92). The results show that Preemption Law × electricity share is negative (–0.076) and highly significant, consistent with a substitution relationship between electricity and gas. Instead, interactions with wood, and especially with fuel oil, LPG, and other gas are insignificant, indicating that preemption laws specifically increase credit for gas-reliant firms rather than broadly affecting all firms with polluting technologies. These results strengthen our interpretation that credit supply effects are specific to natural gas.

E Pricing and Risk

Table VIII assesses the effects of preemption laws on credit supply along different margins than loan amount. We study loan pricing, and lender risk-taking for which ex-post loan default is used as a proxy.

Table VIII here

In the first two columns, the dependent variable is the interest rate on the loan. In column 1 we find that the DDD coefficient is negative and significant at the 10% level. To control for state-specific time trends, we add state × month fixed effects in column 2. The coefficient is still negative, and statisitcal significance improves to the 5% level. Following a gas ban preemption law, interest rates fall by approximately 3 basis points for an SME using an average share of natural

gas for their operations (0.387). This result is consistent with improved creditworthiness as the regulatory outlook stabilizes.[28]

Next, we replace the dependent variable with ex-post loan default in the last two columns of the table. The coefficients on the interaction between preemption law and gas dependence are statistically insignificant. Therefore, there is no effect on default rates, suggesting that the expansion in credit is not accompanied by excessive risk-taking. The combination of higher loan amounts and lower interest rates reinforces the interpretation that lenders positively perceive climate preemption as reducing operational uncertainty. This evidence complements the loan amount findings and indicates that lenders perceive lower transition risk after preemption, offering cheaper credit without degrading portfolio quality.

F SBA Fee Thresholds and Strategic Loan Structuring

To corroborate the mechanism of a reduction in lenders' assessed transition risk for gas-intensive SMEs, we complement the main credit supply results with an additional test that exploits the discrete fee schedule embedded in the SBA 7(a) guarantee program. SBA guarantee fees are assessed as a percentage of the guaranteed portion of the loan and increase discontinuously at fixed dollar thresholds. Bachas et al. (2021) document significant bunching in the SBA loan size distribution just below the threshold at which the guarantee portion increase and fees decrease, estimating a highly elastic lending supply response to loan guarantees between 2008 and 2017. We focus specifically on the $1,000,000 threshold, above which the guarantee fee rises sharply. A key institutional advantage of this cutoff for our purposes is that it remained operative throughout our sample period. In fact, while the SBA suspended guarantee fees on loans below $1,000,000 during the COVID-19 relief period, the fee discontinuity at the $1,000,000 threshold was not affected by these waivers, ensuring that the notch is present and economically relevant across the full sample. We argue that the incentive to bunch below this threshold are decreasing in the perceived creditworthiness of the borrower: a lender that views a borrower as low-risk has less need for the

[28] SBA rates are constrained by program caps that may depend on loan size. In principle, the pricing effect could reflect cap-tier mechanics rather than risk repricing. We address this issue in two ways: first, all our specifications include SBA subprogram (by disbursement method) fixed effects; second, the results of the pricing regressions are robust to the inclusion of loan size as a control variable.

guarantee and is therefore more willing to absorb the higher fee cost or lower guarantee portion of a loan above $1,000,000.[29]

We exploit this logic to construct a complementary test of whether state-level preemption of natural gas bans reduces lenders' assessed transition risk for gas-intensive SMEs in a Regression Discontinuity Difference-in-Difference-in-Differences (RD-DDD):

$$1(A_{i,j,l,s,t}) = \alpha_l + \alpha_j + \alpha_{s,t} + f(G_{i,j,l,s,t} - T) + \beta PreemptionLaw_{s,t} + \gamma PreemptionLaw_{s,t} \times GasDependence_j + X_{s,t} + X_i + \varepsilon_{i,j,l,s,t}, \quad (4)$$

where $1(A_{i,j,l,s,t})$ is an indicator equal to one if the SBA guarantee amount (running variable) falls strictly below $T = \$1,000,000$. For each loan, we control for the distance, and its quadratic term, between the SBA guarantee amount G and the threshold T, as well as all loan controls included in the main analysis. Also, the set of fixed effects remain the same. We restrict the estimation sample to loans falling within a symmetric, narrow bandwidth ϵ around T, so that observations just below and just above the cutoff are comparable in all respects except for the discrete jump in guarantee fees at the notch. To avoid mechanical classification errors or spurious density shifts exactly at cutoff, we drop observations that are in the inner 2% around the cutoff.

Figure IV here

Figure IV reports the main coefficient of interest along with 95% confidence intervals. The full regression output is included in Table AV. The triple interaction coefficient γ is negative and statistically significant across specifications with varying bandwidths. Statistical significance is lower the larger the bandwidth, but t-statistics are always above customary levels. The negative coefficient indicates that gas-intensive SMEs in preemption states are less (more) likely to receive loans below (above) the $1,000,000 fee threshold following the enactment of a preemption law, relative to both non-gas-intensive firms and firms in the same state. This finding is consistent with the transition risk channel. After preemption, the reduction in perceived transition risk lowers

[29] SBA fees are 3.5% of the guaranteed portion up to $1M, plus 3.75% of the guaranteed portion over $1M.

the marginal value of the SBA guarantee, making lenders more willing to cross the threshold and absorb the higher fee in exchange for a larger guaranteed loan.

We implement placebo threshold tests by re-estimating the specification at fictitious cutoffs of $900,000 or $1,100,000 where no fee discontinuity exists. Results are presented in Figure IV as well as in Panel B and C of Table AV. The insignificant triple interaction terms at placebo thresholds for all bandwidths confirm that the result is anchored to the fee structure rather than to confounding differential trends in loan size for gas-intensive SMEs in preemption states.

G Real Effects

We conclude the analysis by exploring whether the shift in credit supply following preemption laws translates into real effects focusing on borrower employment. The challenge for the identification is that preemption laws may have a direct effect on SME employment, an effect that is separated from the credit response. To mitigate this concern, we take advantage of the SBA programs that require SMEs to report the number of jobs supported or retained due to SBA financing. In Table IX, we regress the log of reported jobs supported, defined as the sum of jobs created and retained as reported on the SBA loan application, on the interaction between preemption and gas dependence. We adopt the same empirical framework as our baseline loan-level regressions.

Table IX here

While results are statistically insignificant in the full sample (column 1) and among young firms (column 3), we find significant and economically meaningful effects among franchise borrowers and firms in the top quartile of gas reliance. These patterns are again consistent with a transition risk interpretation, in which the most operationally constrained and gas-dependent firms benefit most from regulatory relief, allowing them to sustain or expand employment.

Although the SBA job measure has the advantage of attaching jobs supported to a specific loan, we interpret these findings with caution. Job creation and retention figures are self-reported by lenders at the time of loan origination, and the SBA does not independently verify or audit these estimates. The resulting noise may attenuate statistical power, particularly in smaller or newer

firms. Nevertheless, the fact that employment effects emerge in the same subsamples where we observe the strongest credit supply response offers suggestive evidence that the preemption laws may have tangible implications for real economic activity in gas-reliant sectors.

Because the *JobsSupported* variable in SBA data is self-reported and not independently verified, we complement our analysis using 2018-2024 administrative employment data from the Bureau of Labor Statistics (BLS) Quarterly Census of Employment and Wages (QCEW). We report the results from DDD regressions in Table X, where the log of annual total employment at the state–industry (6-digit) level is regressed on the interaction between preemption laws and sector gas dependence. In the first column, the non-interacted coefficient is negative and insignificant. In columns 2 and 3, we add the interaction term and state-year FE, and we find that employment levels in gas-intensive sectors increase following preemption. This provides stronger evidence that the relaxation of transition-related operational risk translates into realized labor demand, and are consistent with an intensive-margin expansion of incumbent firms rather than increased entry.

Next, we test the effect of preemption laws on employment levels focusing on specific settings in which credit frictions bind. In column 4, we multiply our QCEW-based employment variable with the weight of SME employment within an industry-state using 2022 data from the Statistics of U.S. Businesses (SUSB) by the Census Bureau.[30] We find the real effects persist in SME-intensive settings, strengthening the financial channel. Finally, the sample column 5 consists of private firms only, that is excluding local, State, or Federal Governmental establishments. We find the results persist also in this subsample. Overall, this evidence connects finance to real economic outcomes, and shows political climate retrenchment has measurable labor market effects.

V Conclusion

This paper examines the impact of state-level climate preemption laws on credit access for small businesses. Climate preemption laws prevent local governments from enacting their own climate policies, particularly those that would restrict fossil fuels or mandate renewable energy, hindering

[30] Following the SBA definition of small businesses, we focus on private independent employers with fewer than 500 employees.

local climate action. The conflict regarding natural gas prohibition in the US intensified, as more than half of the states adopted preemption statutes to stop local authorities from limiting access to natural gas. Typically, these laws prevent municipalities from prohibiting gas connections or appliances. We document how climate preemption laws reshape credit allocation between gas-intensive and clean energy firms, revealing how top-down political interventions can disrupt local de-carbonization efforts and private investment patterns.

By employing matched SBA loan-level data alongside industry-specific energy consumption in a difference-in-difference-in-differences framework, we show that preemption laws enhance credit availability—reflected in both loan amounts and interest rates—for small and medium-sized enterprises that heavily rely on natural gas. These effects are driven by higher loan volumes and modest reductions in interest rates, with no corresponding increase in loan default risk. The response is strongest among franchise firms, startups, and firms in the top quartiles of gas intensity, consistent with a technology-based transition risk channel: lenders perceive preemption as reducing the regulatory uncertainty associated with existing gas-based infrastructure.

We also find preemption laws affect real outcomes through small-business credit markets. Employment in gas-intensive sectors increases following preemption laws, especially in SME-intensive industries and for private firms. In line with prior research showing that labor market exposure shapes political preferences, policies that expand employment in fossil-dependent industries may reinforce political support for climate retrenchment, potentially generating feedback loops that stabilize anti-decarbonization coalitions. While we do not directly test voting behavior, our results highlight how subnational climate policy conflicts may influence not only capital allocation but also the political constituencies affected by the energy transition.

Overall, our paper results highlight that the success of transition financing is contingent upon the political dynamics that influence regional climate policies. The consequences of state-level climate preemption shed light on businesses, policymakers, and society by highlighting that reducing transition risk may impact SME access to credit in already green sectors. For businesses, this translates into altered incentives to innovate. Our results reveal that political backlash against

local environmental governance creates risks not only for climate transition and adaptation but also for business dynamism, underscoring the need for coordinated climate policy across federal, state, and local levels (Hale, 2016).

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Figures

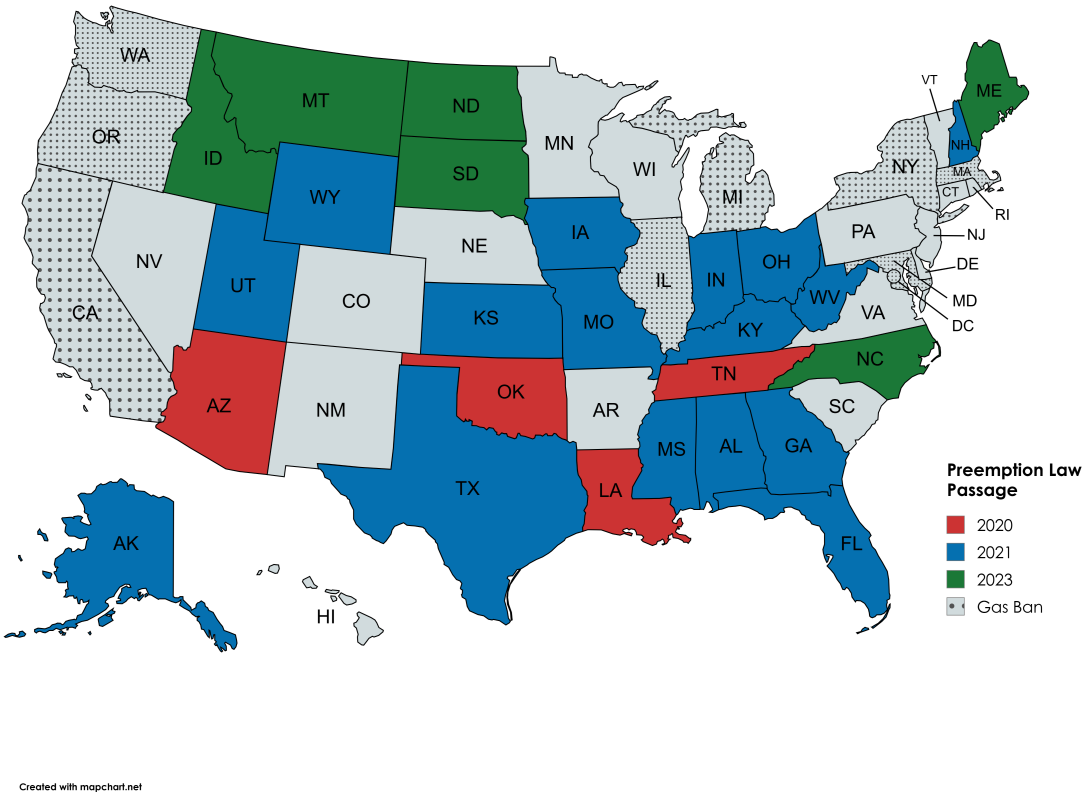


Figure I: This map shows the timing of the passage of state-level laws preempting local gas bans. Preemption law passage is based on the year in which the preemption bill has successfully gone through the legislative process in both House and Senate. Source: Walker and Malmuth (2024). Instead, gas bans denote states where at least one city or county passes an all-electric requirement policy between 2019 and 2024. Source: Building Decarbonization Coalition.

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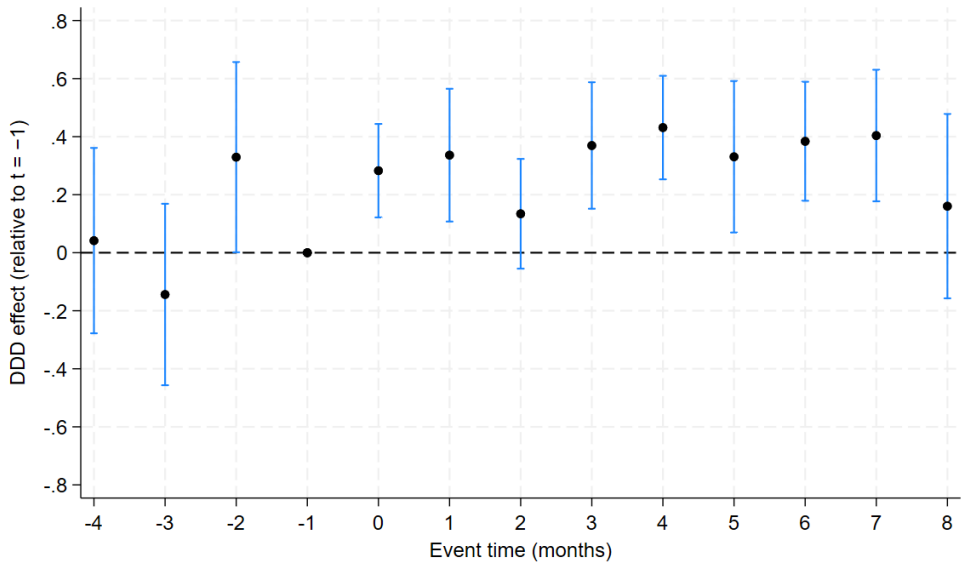


Figure II: This figure plots dynamic treatment effects around policy adoption, estimated using the Borusyak et al. (2024)'s estimator. Firms above the median gas dependence are classified as high exposure, while firms below the median are classified as low exposure. The plotted series shows the difference between high- and low-exposure firms (a triple-difference specification). Coefficients are normalized relative to event time 1 (the month prior to adoption). Vertical bars represent 95% confidence intervals based on standard errors clustered at the state level.

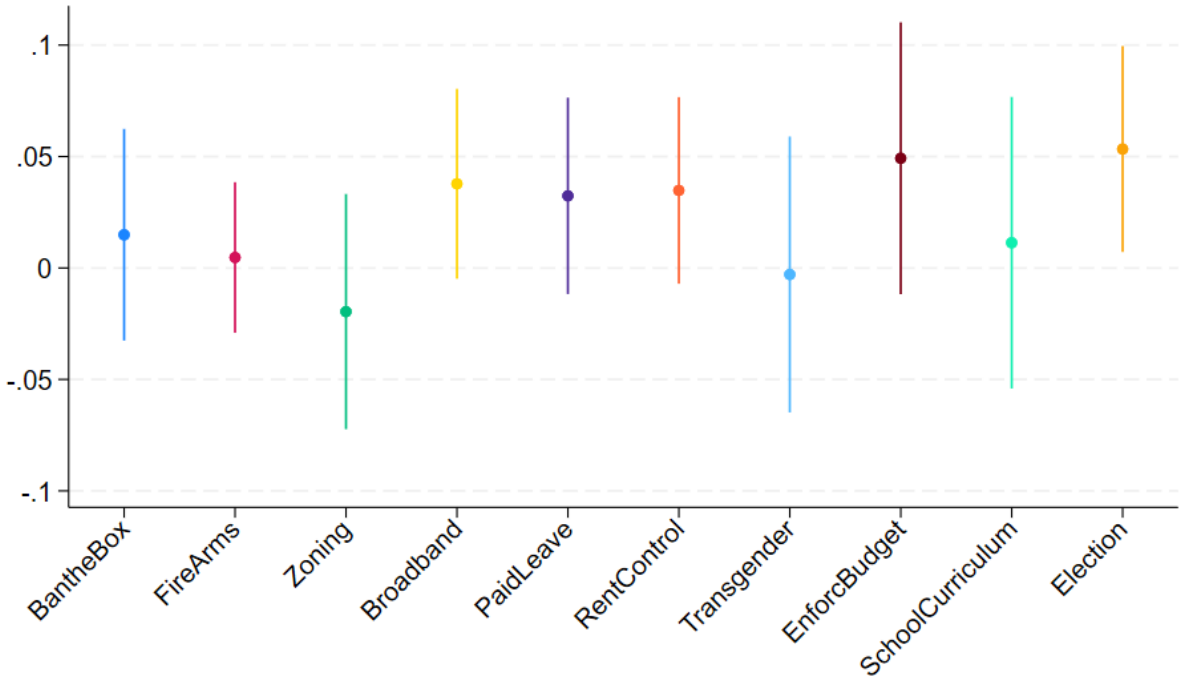


Figure III: This chart displays the coefficients and confidence intervals of the triple interactions between firm gas exposure and various preemption laws ($PreemptionLaw \times GasDependence$ in model 3). Preemption law timing information is sourced from the Center for Public Health Law Research (CPHLR) at Temple University's Beasley School of Law in Philadelphia. Laws cover the period from August 1, 2019, to December 31, 2024. We build interactions based on ten domains that are related to the social determinants of health: (1) Ban the Box, (2) firearms, (3) mandatory inclusionary zoning, (4) municipal broadband, (5) mandatory paid leave, (6) rent control, (7) transgender rights, (8) local law enforcement budgets, (9) race and racism in school curriculum, and (10) election policies.

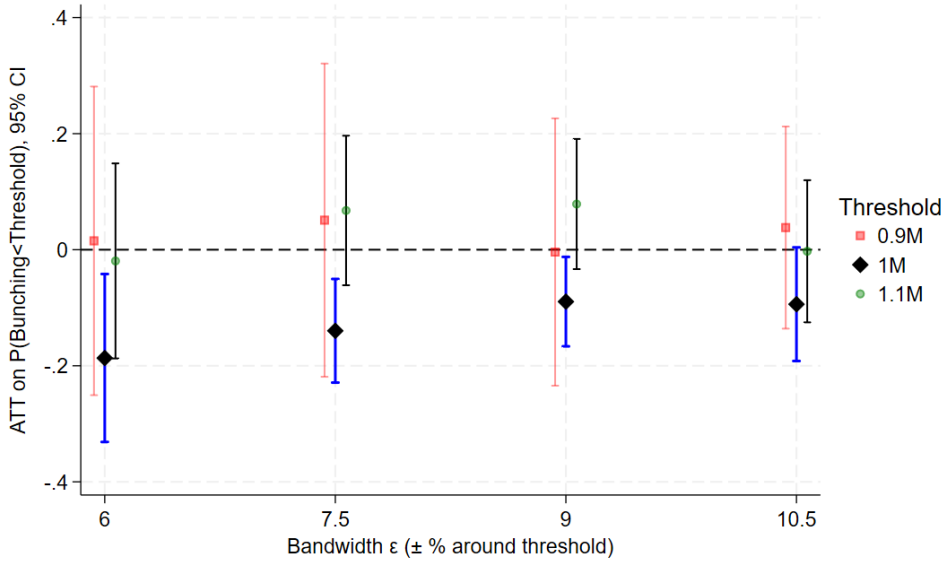


Figure IV: The figure plots the estimated DDD coefficient on $Preemption_{s,t} \times GasDependence_j$ from regressions around the \$1 million SBA 7(a) fee threshold, and placebo thresholds (\$900,000 and \$1,100,000) across alternative bandwidths (±6% to ±10.5%). Vertical bars represent 95% confidence intervals. Estimates are obtained from regressions including two polynomial controls for distance to the threshold, and fixed effects as in the baseline specification (column 5 of Table IV). Detailed coefficients reported in table AV.

Tables

Table I: State-level Summary Statistics

This table presents summary statistics for all variables used in Table III. The sample includes all US states and spans the period 2016-2024. For a full description of the variables, see Table AVI. Data sources: Walker and Malmuth (2024) (WM2024), Census Bureau (Census), Bureau of Economic Analysis (BEA), Bureau of Labor Statistics (BLS), Federal Deposit Insurance Corporation (FDIC), Ballotpedia, Yale Program on Climate Change Communication (YPCCC).

	Source	N	Mean	SD	Min	Max
Preemption Law	WM2024	459	0.209	0.407	0.000	1.000
Population	Census	459	8.273	1.034	6.359	10.585
Bachelor's degree or higher	BEA	459	33.780	6.864	20.200	65.900
Real income per capita	BLS	459	10.912	0.124	10.622	11.324
Unemployment rate	BLS	459	4.314	1.601	1.800	13.700
Bank sector depth	FDIC & BEA	459	0.856	1.654	0.216	13.945
Tax revenue	Census	459	8.161	1.019	5.583	11.150
State government trifecta	Ballotpedia	441	0.707	0.455	0.000	1.000
Republican-leaning state	Ballotpedia	441	0.605	0.418	0.000	1.000
Climate concerns	YPCCC	459	60.385	6.592	41.760	78.820
CO2 regulation support	YPCCC	459	73.275	3.998	61.760	83.700

Table II: Loan-level Summary Statistics

This table presents summary statistics of all variables for the loan-level regression analyses. The sample period spans the period January 2016-December 2024. For a full description of the variables, see Table AVI. Data sources: Small Business Administration (SBA), Industrial Training and Assessment Centers (ITAC), Ballotpedia, Yale Program on Climate Change Communication (YPCCC).

	Source	N	Mean	SD	Min	Max
log(Loan Amount)	SBA	141,817	12.307	1.554	8.922	15.424
Interest Rate	SBA	141,816	7.546	2.417	3.53	14.5
Default (dummy)	SBA	141,817	0.034	0.182	0	1
Gas Dependence	ITAC	141,817	0.387	0.278	0	0.964
Electricity Dependence	ITAC	141,817	0.591	.285	0.0359	1
Fuel Oil Dependence	ITAC	141,817	0.013	0.088	0	0.832
Wood Dependence	ITAC	141,817	0.005	0.0370	0	0.304
LPG Dependence	ITAC	141,817	0.002	0.007	0	0.048
Other Gas Dependence	ITAC	141,817	0.001	0.008	0	0.061
Brown Industry (dummy)	SBA	141,817	0.306	0.461	0	1
Loan term (in months)	SBA	141,817	144.080	82.536	12	309
SBA Contribution	SBA	141,817	0.678	.145	0.5	0.9
Corporation (dummy)	SBA	141,817	0.934	0.247	0	1
Collateral (dummy)	SBA	141,817	0.801	0.399	0	1
Sold Secondary Market (dummy)	SBA	141,817	0.349	0.477	0	1
Franchise (dummy)	SBA	141,817	0.134	0.341	0	1
Young (dummy)	SBA	141,817	0.137	0.344	0	1
Republican-leaning state	Ballotpedia	141,817	0.540	0.498	0	1
Climate concerns	YPCCC	141,817	62.751	5.796	41.76	73.63
CO2 regulation support	YPCCC	141,817	74.693	3.400	61.76	81.7

Table III: Determinants of the Adoption of State Climate Preemption Laws

This table examines whether a state's economic, political, or demographic conditions predict the adoption of a climate preemption law. The dependent variable is an indicator equal to one (*Preemption law*) if a state s has adopted a climate preemption law in year t. For a full description of the variables, see Table AVI. The sample includes all states and spans the period 2016-2024, but does not include the District of Columbia (all columns) and the state of Nebraska (columns 2 and 4). Nebraska is unicameral and officially nonpartisan, so political conditions cannot map cleanly. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Population	0.125	0.050	1.612	1.305
	(0.080)	(0.100)	(1.676)	(1.532)
Bachelor's degree or higher	-0.022***	0.001	-0.033	-0.015
	(0.006)	(0.008)	(0.032)	(0.040)
Real income per capita	1.422***	0.432*	0.808	0.331
	(0.301)	(0.236)	(1.073)	(1.000)
Unemployment rate	-0.050***	-0.039***	-0.046**	-0.029
	(0.012)	(0.011)	(0.023)	(0.023)
Bank sector depth	-0.025**	-0.018	0.074	0.096
	(0.012)	(0.011)	(0.074)	(0.062)
Tax revenue	-0.111	-0.026	-0.420	-0.256
	(0.081)	(0.105)	(0.360)	(0.332)
State government trifecta		-0.011		-0.064
		(0.053)		(0.051)
Republican-leaning state		0.315***		0.378***
		(0.087)		(0.094)
Climate concerns		0.026***		0.026**
		(0.007)		(0.012)
CO2 regulation support		-0.065***		-0.086***
		(0.009)		(0.017)
Observations	459	441	459	441
R-squared	0.142	0.384	0.603	0.662
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
Cluster SE	State	State	State	State

Table IV: Climate Preemption Laws and Loan Amount

This table presents the OLS estimates of the Difference-in-Difference (DiD) regression of SME lending on preemption laws using loan-level data from Small Business Administration (SBA) programs. The sample spans the period January 2016-December 2024. The dependent variable is the log of loan amount. State controls include a Republican-leaning state dummy, population concerns about climate, and CO2 regulation support, whereas loan controls a dummy indicating a corporation as the borrower, a dummy for the use of collateral, the share of SBA contribution in the loan amount, a dummy for the loan being sold in the secondary market, and loan maturity in months. For variables definition, please see Table AVI. The set of fixed effects is at disbursement method × SBA subprogram, bank, state, and month or state-month level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dep.Variable: log(LoanAmount)	(1)	(2)	(3)	(4)	(5)
Preemption Law	-0.097***	-0.119***	-0.056***	-0.043*	
	(0.027)	(0.031)	(0.021)	(0.022)	
Preemption Law × Gas Dependence		0.054**	0.067***	0.052**	0.057**
		(0.026)	(0.022)	(0.023)	(0.022)
Industry Gas Dependence		0.111***	0.106***		
		(0.014)	(0.012)		
Republican-leaning state	0.171***	0.172***	0.067**	0.070***	
	(0.051)	(0.051)	(0.025)	(0.024)	
Climate concerns	0.031***	0.031***	-0.003	-0.003	
	(0.003)	(0.003)	(0.004)	(0.004)	
CO2 regulation support	-0.058***	-0.058***	0.004	0.004	
	(0.004)	(0.004)	(0.005)	(0.005)	
Corporation	0.380***	0.377***	0.364***	0.334***	0.336***
	(0.020)	(0.021)	(0.019)	(0.017)	(0.017)
Collateral	0.594***	0.593***	0.578***	0.569***	0.578***
	(0.031)	(0.032)	(0.027)	(0.028)	(0.026)
SBA Contribution	-3.466***	-3.456***	-7.290***	-6.967***	-7.012***
	(0.163)	(0.163)	(0.261)	(0.256)	(0.258)
Sold Secondary Market	0.090**	0.091**	0.086**	0.064*	0.062*
	(0.039)	(0.039)	(0.036)	(0.036)	(0.037)
Loan Term	0.005***	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	141,817	141,817	141,817	141,806	141,634
Mean	12.308	12.308	12.308	12.308	12.308
R-squared	0.686	0.686	0.704	0.719	0.731
Bank FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	No
Month FE	No	No	Yes	Yes	No
Industry FE	No	No	No	Yes	Yes
State × Month FE	No	No	No	No	Yes

Table V: Robustness - Climate Preemption Laws and Loan Amount

This table presents the results of the robustness tests for the regression of SME lending on preemption laws departing from the baseline model (column 5 in Table IV). The dependent variable is the logarithm of the loan amount. In panel A, specifications in column 1 and 2 shift the treatment timing (preemption law) earlier to when the preemption bill is first introduced in either the House of Representatives or the Senate, and to when the bill is passed by the first chamber, respectively. In column 3, the treatment is assigned to states based on the bank's headquarter. In column 4, *Brown* is a dummy indicating those industries that appear in the SBA list of environmentally sensitive industries. In panel B, samples exclude states where either a city or county has introduced a gas ban (column 1), preemption laws passed during the 2020 Covid period (column 2), counties that are contiguous to a state border (column 3), and states with a high (¿25th percentile) republican ideology (column 4). For variables definition, please see Table AVI. Fixed effects are at disbursement method × SBA subprogram, bank, industry, and state-month level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Placebo Tests	(1)	(2)	(3)	(4)
Preemption Law × Gas Dependence	0.030	0.036	0.003	0.052**
	(0.023)	(0.023)	(0.017)	(0.023)
Preemption Law × Brown Industry				0.017
				(0.014)
Law	Introduced	1stChamber	2ndCh.-Bank's HQ	2ndChamber
Observations	141,825	141,825	141,825	141,825
Mean	12.308	12.308	12.308	12.308
R-squared	0.731	0.731	0.731	0.731
Loan Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes
Panel B: Subsample Analysis	(1)	(2)	(3)	(4)
Preemption Law × Gas Dependence	0.056**	0.068***	0.076***	0.085***
	(0.023)	(0.019)	(0.020)	(0.017)
Observations	88,701	135,077	116,728	98,528
Mean	12.355	12.293	12.282	12.210
R-square	0.730	0.731	0.731	0.723
Sample Excl.	GasBans	Covid (2020)	BorderCo	HighRepublican
Loan Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes

Table VI: Climate Preemption Laws and Loan Characteristics (Credit Demand)

This table presents the results of triple Difference-in-Difference regression of Preemption Laws on SME lending using loan-level data from SBA. The sample spans the period January 2016-December 2024. Loan controls a dummy indicating a corporation as the borrower, a dummy for the use of collateral, the share of SBA contribution in the loan amount, a dummy for the loan being sold in the secondary market, and loan maturity in months. For variables definition, please see Table AVI. Fixed effects are at disbursement method × SBA subprogram, bank, industry, and state-month level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1) Corporation	(2) Collateral	(3) SBA Contrib.	(4) Sold Sec.	(5) Term
Preemption Law × Gas	-0.005	-0.005	-0.001	-0.004	-2.712
	(0.006)	(0.007)	(0.001)	(0.007)	(1.815)
Observations	141,825	141,825	141,825	141,825	141,825
Mean	0.935	0.801	0.678	0.349	144.053
R-squared	0.128	0.451	0.942	0.708	0.554
Loan Controls	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes	Yes

Table VII: Climate Preemption Laws and Credit – Channels

This table presents the results of the triple Difference-in-Difference (DDD) regression of Preemption Laws on SME lending using loan-level data from Small Business Administration (SBA) programs. The sample spans the period January 2016-December 2024. Loan controls a dummy indicating a corporation as the borrower, a dummy for the use of collateral, the share of SBA contribution in the loan amount, a dummy for the loan being sold in the secondary market, and loan maturity in months. For variables definition, please see Table AVI. The sample in column 1 includes only out-of-state lenders. In columns 2 and 3, the samples include franchise firms and startup only, respectively. Instead, specifications in columns 4 and 5 are based on the full sample. Fixed effects are at disbursement method × SBA subprogram, bank, industry, and state-month level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dep.Variable: log(LoanAmount)	(1)	(2)	(3)	(4)	(5)
Preemption Law × Gas Dependence	0.054**	0.133***	0.105**		
	(0.023)	(0.039)	(0.052)		
Preemption Law × Gas Quartile2				0.029*	
				(0.016)	
Preemption Law × Gas Quartile3				0.042***	
				(0.015)	
Preemption Law × Gas Quartile4				0.054***	
				(0.015)	
Preemption Law × Electricity					-0.068***
					(0.022)
Preemption Law × Fuel Oil					0.102
					(0.095)
Preemption Law × Wood					0.240
					(0.178)
Preemption Law × LPG					0.543
					(0.954)
Preemption Law × Other Gas					-1.498
					(0.964)
Observations	90,864	17,768	16,085	141,825	141,825
Mean	12.297	13.256	12.504	12.308	12.308
R-squared	0.762	0.845	0.769	0.731	0.731
Sample	OutStateLend	Franchise	Young	All	All
Loan Controls	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes	Yes

Table VIII: Preemption Laws and Credit Supply Responses – Interest Rates and Default

This table presents the results of triple difference-in-difference regressions of SME loan interest rates, or default rates on Preemption Laws using loan-level data from SBA programs. The sample spans the period January 2016-December 2024. State controls include a Republican-leaning state dummy, population concerns about climate, and CO2 regulation support, whereas loan controls a dummy indicating a corporation as the borrower, a dummy for the use of collateral, the share of SBA contribution in the loan amount, a dummy for the loan being sold in the secondary market, and loan maturity in months. For variables definition, please see Table AVI. Fixed effects are at disbursement method × SBA subprogram, bank, industry, and state-month level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Dep. Variables:	Interest rate		Default	
Preemption Law	0.052		-0.007*	
	(0.053)		(0.004)	
Preemption Law × Gas Dependence	-0.077*	-0.084**	0.000	-0.000
	(0.042)	(0.041)	(0.003)	(0.003)
Observations	141,805	141,633	141,806	141,634
Mean	7.546	7.546	0.034	0.034
R-squared	0.807	0.817	0.104	0.136
State Controls	Yes	Yes	Yes	Yes
Loan Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	No
Month FE	Yes	No	Yes	No
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	No	Yes	No	Yes

Table IX: Preemption Laws and Jobs Supported (SBA)

This table presents the results of regressing jobs supported by a SBA loan on the interaction between preemption law and a firm's gas dependence. The sample spans the period January 2016-December 2024. Loan controls a dummy indicating a corporation as the borrower, a dummy for the use of collateral, the share of SBA contribution in the loan amount, a dummy for the loan being sold in the secondary market, and loan maturity in months. For variables definition, please see Table AVI. The samples in columns 2 and 3 contain only franchise firms, and start-ups, respectively. Standard errors are reported in parentheses and clustered at the state level. Fixed effects are at disbursement method × SBA subprogram, bank, industry, and state-month level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dep.Variable: log(Jobs Supported)	(1)	(2)	(3)	(4)
Preemption Law × Gas Dependence	0.023	0.131*	0.071	
	(0.031)	(0.068)	(0.077)	
Preemption Law × Gas Quartile2				0.022
				(0.023)
Preemption Law × Gas Quartile3				-0.026
				(0.026)
Preemption Law × Gas Quartile4				0.054**
				(0.021)
Observations	123,478	17,175	16,258	123,478
Mean	1.906	2.238	1.990	1.906
R-squared	0.279	0.488	0.496	0.279
Sample	All	Franchise	Young	All
Loan Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes

Table X: Preemption Laws and Total Employment (QCEW)

This table presents the results of regressing total employment (in log) on the interaction between preemption law and a firm's gas dependence. The sample spans the period 2018-2024. The outcome variable in column 4 is the product between employment and the weight of SME employment in a 6-digit industry within a state, whereas the sample in column 5 contains only private firms. Standard errors are reported in parentheses and clustered at the state level. Fixed effects are at industry, and state-year level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dep. Variable: log(Employment)	(1)	(2)	(3)	(4)	(5)
Preemption Law	-0.022	-0.123***			
	(0.022)	(0.038)			
Preemption Law × Gas Dependence		0.245***	0.245***	0.149**	0.166**
		(0.065)	(0.066)	(0.068)	(0.082)
Observations	125,941	125,941	125,941	106,065	105,919
Mean	6.369	6.369	6.369	5.859	6.530
R-squared	0.417	0.417	0.418	0.342	0.684
Employers	All	All	All	SMEs	Private
State FE	Yes	Yes	No	No	No
Year FE	Yes	Yes	No	No	No
Industry FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	No	No	Yes	Yes	Yes

Appendix

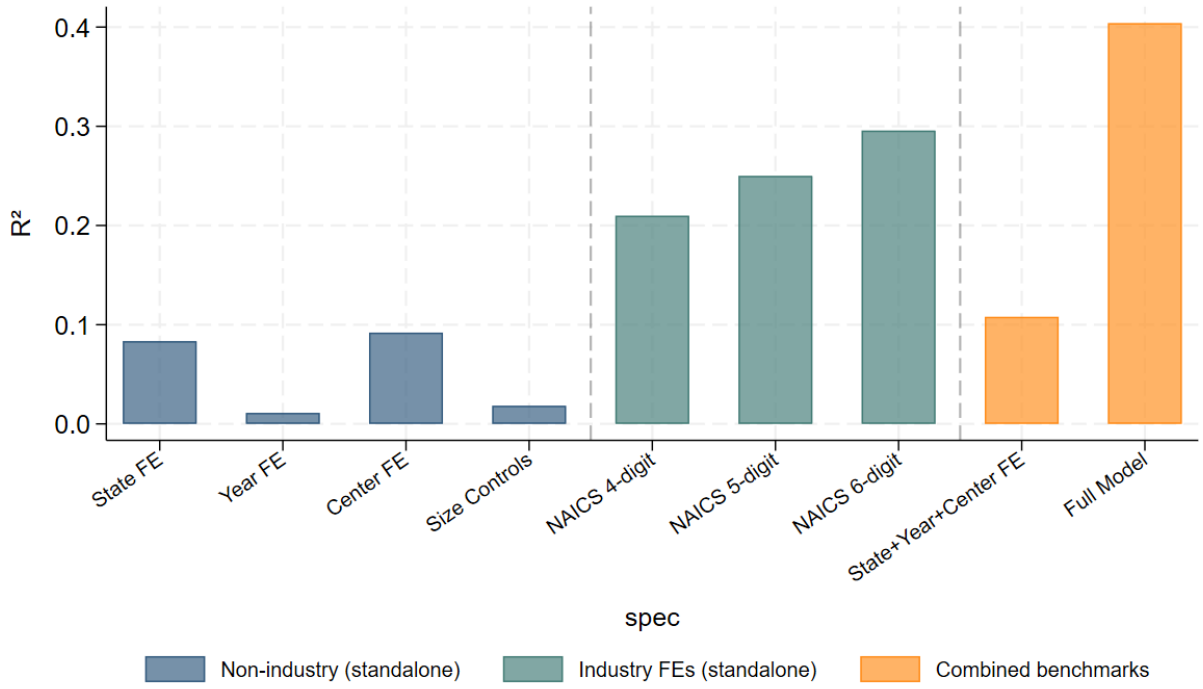


Figure AI: Using ITACs plant-level data, each bar reports the R^2 from a separate OLS regression of natural gas as a fraction of total energy use on a single group of controls: SME state fixed effects, assessment year fixed effects, assessment center fixed effects, plant and firm size controls, or NAICS industry fixed effects at the 4-, 5-, and 6-digit level. The two rightmost bars show the R^2 of the non-industry baseline (state, year, and center fixed effects jointly) and the full model (all non-industry fixed effects, size controls, and NAICS 6-digit fixed effects), included for reference.

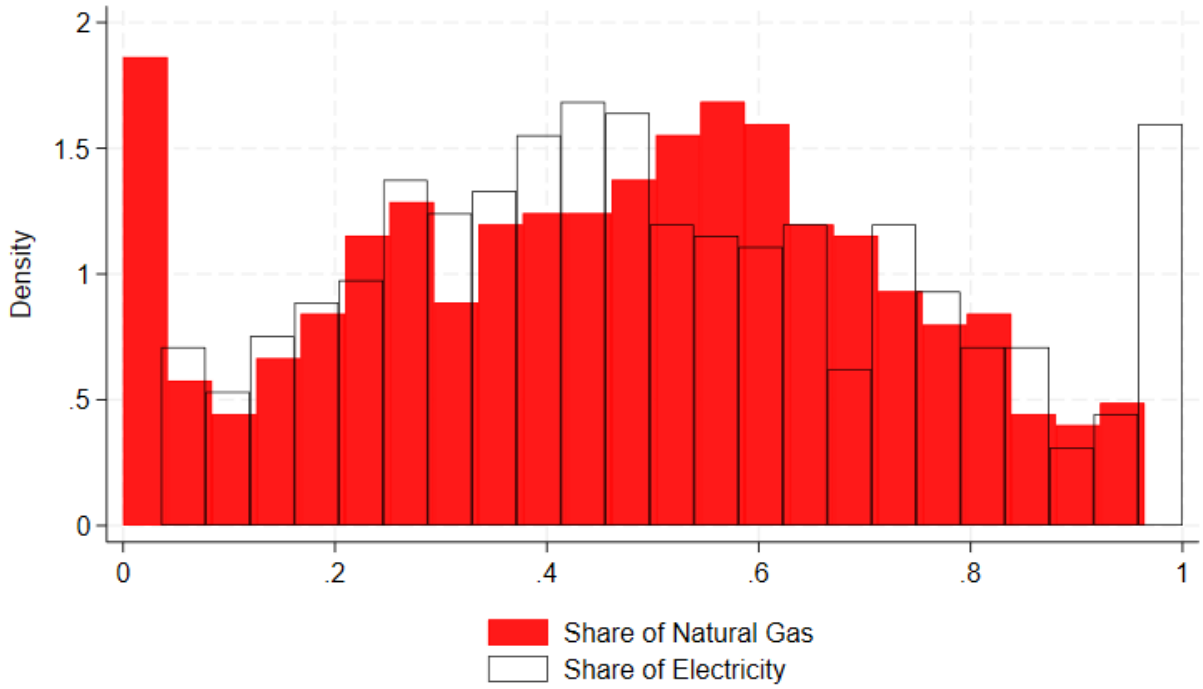


Figure AII: This chart shows the average shares of gas and electricity usages for each 6-digit NAICS sectors covered by the Industrial Training and Assessment Centers (ITACs) of the US Department of Energy.

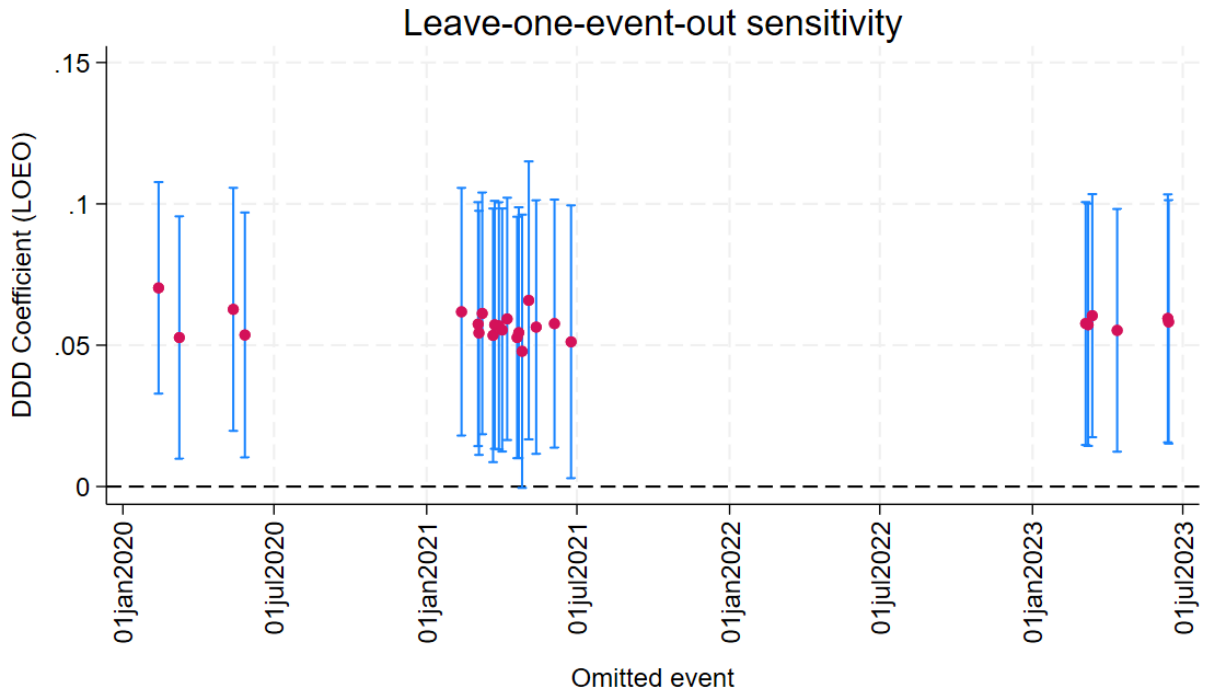


Figure AIII: This chart shows the coefficients of the triple Difference-in-Differences estimation of equation 3 following the baseline specification (column 5 of Table IV), under the Leave-one-event-out (LOEO) approach. Events correspond to preemption laws that each state pass in our sample. For the list of events, please see Table AII.

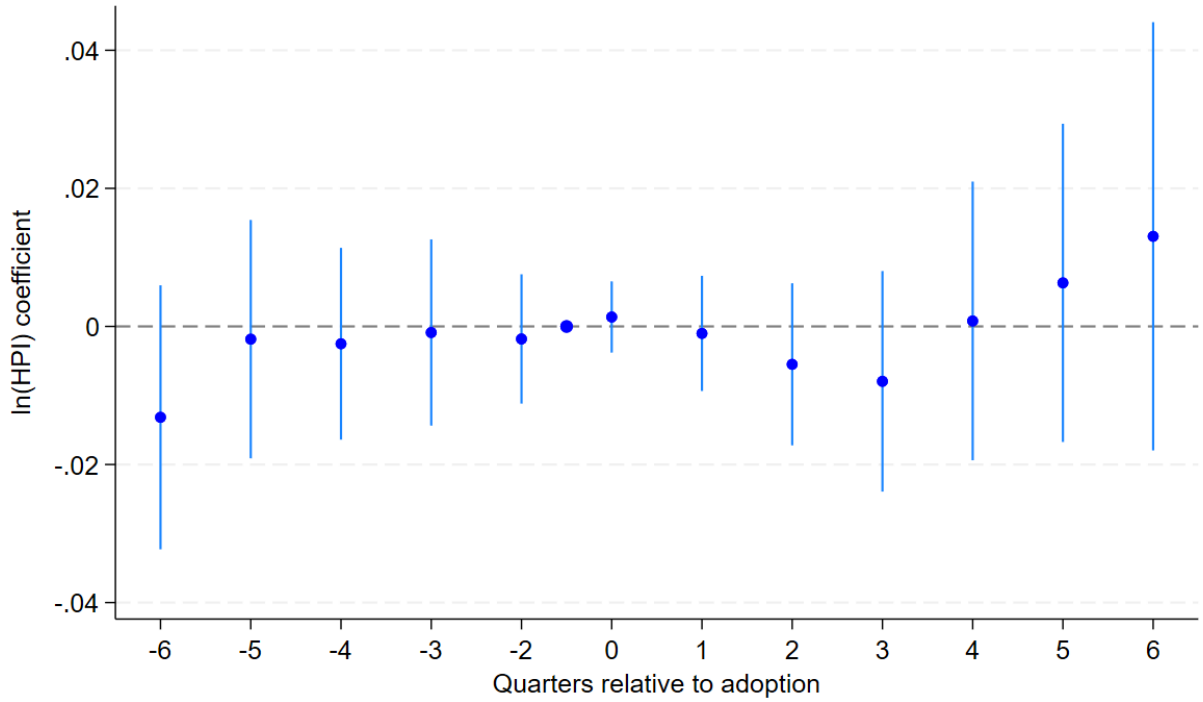


Figure AIV: Event-study estimates of the effect of preemption law adoption on house prices. Coefficients are estimated using a two-way fixed effects model with state and quarter fixed effects, as well as state-year controls (see Table III for the list of variables). The dependent variable is the log FHFA all-transactions House Price Index, seasonally adjusted, indexed to 2017q4 = 100. Each coefficient represents the average treatment effect on the treated (ATT) relative to the quarter immediately preceding adoption (t = 1). Dashed vertical line marks the adoption quarter (t = 0). Whiskers denote 95% confidence intervals based on standard errors clustered at the state level.

Table AI: Gas Bans and Loan Amount

This table presents the regression results on SME lending using loan-level data from Small Business Administration (SBA) programs. The sample spans the period January 2016-December 2024. *Gasban* is a dummy variable taking value 1 after a city or county passes a gas ban (see Table **??** for more details). Fixed effects are at disbursement method × SBA subprogram, bank, industry, and state-month (columns 1 and 2), county-month (column 3), or city-month (column 4) level. Standard errors are reported in parentheses and clustered at the county or city level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dep.var.: log(LoanAmount)	(1)	(2)	(3)	(4)
Gas Ban	0.0604	0.199***		
	(0.0455)	(0.0455)		
Gas Ban × Gas Dependence	0.0110	-0.111	-0.0871**	-0.0709
	(0.0658)	(0.147)	(0.0430)	(0.155)
Observations	141,072	92,424	68,801	46,356
Mean	12.31	12.34	12.31	12.23
R-squared	0.731	0.732	0.797	0.841
Treatment-level	County/City	County	County	City
Loan Controls	Yes	Yes	Yes	Yes
BankFE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	No	No
County × Month FE	No	No	Yes	Yes
City × Month FE	No	No	No	Yes

Table AII: Passage of Preemption Laws by State

Note: This table presents the exact dates when the gas ban preemption law is introduced, passed by the first Chamber, and passed by the Second chamber, respectively. Data source: Walker and Malmuth (2024).

State	Bill	Introduced	First chamber	Second chamber
AK	SB 137	01/14/2021	02/08/2021	03/04/2021
AL	HB 446	02/24/2021	04/01/2021	04/20/2021
AZ	HB 2686	01/22/2020	02/12/2020	02/13/2020
FL	HB 919	02/10/2021	04/22/2021	04/26/2021
GA	HB 150	01/27/2021	02/22/2021	03/22/2021
IA	HF 555	02/16/2021	3/15/2021	03/29/2021
ID	HB 106	02/09/2023	2/17/2023	03/14/2023
IN	HB 1191	01/07/2021	4/21/2021	04/22/2021
KS	SB 24	01/13/2021	02/04/2021	03/24/2021
KY	HB 207	01/07/2021	02/23/2021	03/05/2021
LA	SB 492	03/31/2020	05/14/2020	05/27/2020
ME	LD 894	02/28/2023	06/13/2023	06/14/2023
MI	HB 4575	09/21/2022	9/28/2022	
MO	HB 734	01/07/2021	05/13/2021	05/13/2021
MS	HB 632	01/18/2021	02/04/2021	03/09/2021
MT	SB 208	01/23/2023	03/02/2023	04/13/2023
NC	HB 130	02/15/2023	06/08/2023	06/13/2023
ND	HB 1234	01/10/2023	02/01/2023	03/09/2023
NH	SB 86	01/26/2021	03/04/2021	06/04/2021
OH	HB 201	03/10/2021	05/06/2021	06/24/2021
OK	HB 3619	02/03/2020	03/09/2020	05/13/2020
PA	SB 143	01/30/2023	03/08/2023	
SD	SB 174	01/30/2023	02/21/2023	03/06/2023
TN	SB 1934	01/28/2020	03/02/2020	03/09/2020
TX	HB 17	03/05/2021	03/31/2021	05/04/2021
UT	HB 17	12/15/2020	02/03/2021	02/12/2021
VA	HB 1783	01/10/2023	01/24/2023	
WI	SB 49	02/14/2023	06/07/2023	
WV	HB 2842	03/02/2021	03/26/2021	04/08/2021
WY	SF 0152	03/02/2021	03/19/2021	04/02/2021

Table AIII: Gas Dependence Robustness

Note: Robustness of Main Results to Alternative Definitions of Gas Dependence. Each column replicates the baseline specification using an alternative measure of sector-level gas dependence computed on a restricted subsample of ITACS assessments. Columns (1) and (2) restrict the sample used to construct gas dependence to assessments conducted after 2010 and after 2015, respectively. Columns (3) and (4) exclude NAICS 6-digit sectors with fewer than 2 and fewer than 5 assessments, respectively. All specifications include the same controls and fixed effects as the baseline (column 5 of Table IV.

Dep.var.: log(LoanAmount)	(1)	(2)	(3)	(4)
Preemption Law × Gas Dependence	0.060**	0.062**	0.091***	0.116***
	(0.022)	(0.026)	(0.030)	(0.041)
Corporation	0.339***	0.332***	0.358***	0.352***
	(0.018)	(0.020)	(0.025)	(0.027)
Collateral	0.584***	0.582***	0.609***	0.605***
	(0.026)	(0.027)	(0.026)	(0.034)
SBA Contribution	-7.036***	-7.046***	-6.863***	-6.812***
	(0.256)	(0.250)	(0.233)	(0.255)
Sold Secondary Market	0.061	0.059	0.052	0.068*
	(0.037)	(0.036)	(0.035)	(0.035)
Loan Term	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
ITAC Assessments	$\geq$ 2010	$\geq$ 2015	$\geq$ 2 per sector	$\geq$ 5 per sector
Observations	136,705	127,030	85,104	59,523
Mean	12.322	12.360	12.474	12.549
R-Squared	0.730	0.731	0.743	0.755
Loan Controls	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes

Table AIV: Preemption Laws and Loan Amount: state-NAICS fixed effects

This table presents the results of regressing loan amount (in log) on the interaction between preemption law and a firm's gas dependence. The sample spans the period 2018-2024. For variables definition, please see Table AVI. Standard errors are reported in parentheses and clustered at the state level. Fixed effects are at disbursement method × SBA subprogram, bank, industry (6-digit NAICS), state-month and/or state-industry level. Standard errors are reported in parentheses and clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dep.var.: log(LoanAmount)	(1)	(2)
Preemption Law	-0.041*	
	(0.024)	
Preemption Law × Gas Dependence	0.065**	0.072**
	(0.027)	(0.027)
Corporation	0.330***	0.330***
	(0.022)	(0.022)
Collateral	0.559***	0.567***
	(0.028)	(0.026)
SBA Contribution	-6.792***	-6.846***
	(0.263)	(0.267)
Sold Secondary Market	0.068*	0.065*
	(0.037)	(0.038)
Loan Term	0.005***	0.005***
	(0.000)	(0.000)
Observations	138,733	138,460
Mean	12.311	12.311
R-Squared	0.743	0.755
Loan Controls	Yes	Yes
Bank FE	Yes	Yes
Month FE	Yes	No
State × Month FE	No	Yes
State × Industry FE	Yes	Yes

Table AV: Strategic Bunching: Regression Discontinuity Difference-in-Difference-in-Differences (RD-DDD) Estimates

This table presents estimated coefficients of the model 4 across bandwidths (ϵ) for the \$1 Million threshold (Panel A), and alternative placebo thresholds of \$1.1 Million (Panel B) and \$0.9 Million (Panel C).

Dep. Variable: P(Guarantee<Threshold)	(1)	(2)	(3)	(4)
Panel A: Threshold=\$1 Million				
Preemption Law × Gas Dependence	-0.187***	-0.140***	-0.089**	-0.094*
	(0.074)	(0.046)	(0.039)	(0.050)
Bandwidth ϵ	6%	7.5%	9%	10.5%
Observations	470	839	1,376	2,008
R-squared	0.991	0.973	0.953	0.938
Industry FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes
Panel B: Threshold=\$1.1 Million				
Preemption Law × Gas Dependence	-0.019	0.068	0.079	-0.003
	(0.086)	(0.066)	(0.057)	(0.062)
Bandwidth ϵ	6%	7.5%	9%	10.5%
Observations	553	958	1,391	1,980
R-squared	0.982	0.962	0.947	0.931
Industry FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes
Panel C: Threshold=\$900,000				
Preemption Law × Gas Dependence	0.015	0.051	-0.004	0.038
	(0.136)	(0.138)	(0.118)	(0.089)
Bandwidth ϵ	6%	7.5%	9%	10.5%
Observations	367	832	1,387	1,856
R-squared	0.985	0.966	0.953	0.940
Industry FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes

Table AVI: Variable Definition

Panel A: State-level variables	**Description**
Preemption Law	An indicator taking value 1 if and after a gas ban preemption bill becomes law, when it is approved by the second chamber of the state's legislature (Walker and Malmuth, 2024)
Population	State-level population, in log.
Bachelor's degree or higher	Fraction of people with education above bachelor's degree or higher, in %.
Real income per capita	Household real income per capita
Unemployment rate	Unemployment rate
Bank sector depth	Total FDIC-insured deposits in a state as a fraction of annual nominal GDP of the state.
Tax revenue	Total tax revenue, in log.
State government trifecta	An indicator equal to one if one political party holds the governorship and majorities in both chambers of the state legislature.
Republican-leaning state	Fraction of the three key state governing bodies controlled by Republicans, in %.
Climate concerns	Fraction of the population who are worried about global warming, in %.
CO2 regulation support	Fraction of the population who support regulating carbon dioxide as a pollutant, in %.
Total Employment	State-level annual average of monthly employment levels for a given year.
Panel B: Loan-level variables	
log(Loan Amount)	Loan gross approval amount, in log.
Interest Rate	Total interest rate (base rate plus spread) at the time the loan was approved.
Cancel	Indicator variable equal to one if the loan is canceled either by the borrower or lender at some point before or after funds are disbursed.
Default	Indicator variable equal to one if the loan is in default at some point before maturity.
Loan term	Maturity of the loan, in months.
SBA Contribution	The share of SBA guaranty.
Corporation	Indicator variable taking value 1 for business types that are corporation, and zero otherwise (for individuals or partnerships).
Collateral	An indicator variable equal to one if a loan has collateral.
Sold Secondary Market	An indicator variable equal to one if the lender sells the loan in the secondary market.
Franchise	Indicator that equals to one if a small business operates under a franchising agreement.
Young	Indicator variable taking value one for startups (loan funds will open business).
log(Jobs Supported)	Total Jobs Created + Jobs Retained as self-reported by lender on SBA Loan Application (SBA does not review, audit, or validate these numbers).
Panel C: Industry-level variables	
Gas Dependence	Average share of all types of gas (natural gas, LPG, other gas) used by SMEs operating in a 6-digit NAICS industry.
Electricity Dependence	Average share of electricity used by SMEs operating in a 6-digit NAICS industry.
Fuel Oil Dependence	Average share of fuel oil used by SMEs operating in a 6-digit NAICS industry.
Wood Dependence	Average share of wood used by SMEs operating in a 6-digit NAICS industry.
LPG Dependence	Average share of liquefied petroleum gas (LPG) used by SMEs operating in a 6-digit NAICS industry.
Other Gas Dependence	Average share of other gas used by SMEs operating in a 6-digit NAICS industry.
Brown Industry	An indicator variable indicating environmentally sensitive industries as per SBA SOP 50 10 7 (Appendix 6: Pages 367-369).

Note: This table presents the description of each variable used in the paper.