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Climate Policy Diffusion Across US States

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Climate Policy Diffusion Across US States
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ABSTRACT: Climate policy at the subnational level is sometimes framed as being counterproductive, because climate change is considered a collective action problem that can be best addressed in a coalition that should be as large as possible. Using comprehensive data from US states on climate policy and policy outcomes, we show that state-level policy is effective in accelerating the adoption of solar energy. Crucially, however, state policies also have positive spillovers to other states, by making it more likely that neighboring states adopt climate policy as well. By proportionally attributing the spillover effects, we find that many US states achieve more climate benefits through the spillovers to other states than within their own jurisdiction. In a further step, we distinguish between climate policies in the energy sector and policies addressed either at other sectors or greenhouse gas emission (GHG) reductions generally. We find that climate policies in the energy sector are distinct from other climate policies in two ways: They have a significant effect on solar capacity growth and they diffuse more broadly.

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Climate policy diffusion across US states

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[1] We thank Jeff Mullen for helpful comments as well as Samuel Kuersteiner and Mona Wang for excellent research assistance. We also thank Nick Hillman for sharing his curated data on state legislative control. This draft paper is confidential and reflects research and/or analysis in progress by the author(s). It is being shared with you to elicit comments and should not be circulated without explicit permission. Note that the views expressed are those of the author(s) and do not necessarily represent the views of the IMF, its Executive Board, or IMF management. And, because this paper is not final, its contents should not be quoted, cited, or distributed.

1 Introduction

Economic theory has developed a very influential narrative, according to which climate change is a public good problem, which should be addressed by a large coalition of governments simultaneously. In game theory, unilateral contributions cause other participants to contribute less to a public good, thus offsetting the initial show of goodwill (Hoel, 1991). The policy implication is to form coalitions (Barrett, 1994; Carraro and Siniscalco, 1993) and possibly use sanctions on countries which do not participate to incentivize participation (Nordhaus, 2015). As a result, policymakers trying to introduce climate policy unilaterally can theoretically be accused of taking a counterproductive stance based on simplistic ethics, which is strategically harmful to the intended outcome. This critique can be seen as the more powerful the smaller the jurisdiction of a policy maker and the less clear the contribution of the specific policy to the reduction of GHG emissions. In this article, we study the benefits of climate policy adoption more holistically than a narrow game theoretic model would suggest, showing that unilateral climate policy is neither pointless nor strategically harmful, but may instead reflect an understanding of the dynamics of public good provision and the learning process in policy making. Concretely, we show that unilateral climate policy has positive spillover effects to other jurisdictions, which provide important benefits that diffuse beyond the policy innovator.

Before the 2022 Inflation Reduction Act, efforts to introduce major climate policy at the federal level failed several times in the United States. Between 2003 and 2007, several versions of the Climate Stewardship Act, which aimed at introducing a cap-and-trade system for greenhouse gases, died in committee. In 2007, the Global Warming Pollution Reduction Act was referred to the Senate Committee on Environment and Public Works, but it was not enacted into law. In 2009, the American Clean Energy and Security Act was approved by the House of Representatives but was not brought to the Senate due to a threatened filibuster. This inability to regulate greenhouse gas emissions effectively at the federal level gave impetus to climate policy at the state level.

However, it has been questioned whether policy action by individual states is meaningful to address a global challenge. Both economists and policymakers have argued that climate policy will only be effective if it is implemented in a comprehensive climate coalition, possibly with sanctions against non-members. The concern is that jurisdictions implementing climate policy unilaterally might discourage others from climate policy and cause them to free ride. However, there are also mechanisms which could explain why leadership in implementing climate policy encourages others to do the same (Schwerhoff, 2016). Previous research shows that introducing carbon pricing at the country level makes it more likely that neighboring countries do the same (Linsenmeier et al., 2023). In this paper, we investigate if there is a similar effect at US state level, that is, if states encourage each other with climate policy, possibly building the momentum to compensate for the lack of federal action to some extent.

Climate policies have been introduced by many states, at different times, and over several decades since 1980. This sequential adoption of policies suggests that states which introduced a policy potentially influenced others' climate policies by the types of policy that were introduced. An important dynamic in this context is that states act as a policy laboratory. In the context of smoking bans, for example, Shipan and Volden (2008) describe how US cities observe and learn from each other. They document that it is typically the largest cities, with the highest administrative capability, which innovated policies. Other cities observed the outcome and adopted the best policy solutions. It is plausible that a similar process occurs for climate policy at the state level. The data shows that California, the most populous US state, is often one of the first to introduce a new climate policy. Other mechanisms of mutual influence through policy are competition and emulation (Gilardi and Wasserfallen, 2019).

Policy diffusion among US states has been intensively researched for a long time (Walker, 1969) and applied to various kinds of policy. In a comprehensive meta study, Mallinson (2021) finds that the probability of adopting policy innovations is influenced positively by initiative availability (meaning that citizens can directly vote on proposed laws) and government liberalism, but negatively influenced by Republican control of government and divided government. Importantly for the context of policy diffusion, adoption is also positively influenced by neighbor adoptions and ideological proximity. This means that given ideologies, policies spread among neighbors, and given distance, it also spreads among ideologically-aligned states. A positive effect of geographic or ideological proximity is also identified in the context of climate policy among countries by Linsenmeier et al. (2023). The evidence, therefore, suggests that positive interactions between jurisdictions, including policy diffusion, outweigh negative interactions, such as free riding, pointing to a net positive effect of climate leadership.

Some previous research has analyzed policy diffusion for climate policy among US states. Bromley-Trujillo et al. (2016) introduce the idea that climate policies might have to be analyzed jointly, because states might have a broad commitment to climate policy. They might thus be influenced by a neighboring state's climate policy adoption, without necessarily adopting the same policy. Their results show that climate policies, taken together, show significant diffusion in the form of a strong effect on neighbor adoption, while diffusion can be found only for a few policies individually. Trachtman (2020) explores political determinants more deeply. He finds that underlying partisanship is a stronger predictor of policy adoption than current government control and that some policies depend more strongly on partisanship than others. Glasgow et al. (2021) find that state-specific factors, like socioeconomic characteristics, are decisive for policy adoption (the extensive margin), while both internal and external factors determine the policy goal intensity (the intensive margin). Jett and Raymond (2021) go beyond the type of policy introduced by analyzing how it was introduced in terms of framing. They find that positive economic frames are the most effective.

We extend the knowledge on climate policy spillovers in three ways. First, we use the most comprehensive dataset on climate policies yet, based on Bergquist and Warshaw (2023), and implement several methodological innovations for the diffusion analysis. The most important methodological innovation is a careful analysis of biases arising from selection into treatment and a statistical learning approach to navigate the trade-off between these biases and multi-collinearity. Second, we quantify the effect of climate policies on outcome variables relevant to the policy objective. We find that climate policies had a significant effect on solar energy capacity and electricity production, but not on several other outcome variables, where it might be expected. Third, we identify two subgroups of climate policies, which have very different effects. Policies in the energy sector have a strong effect on solar energy and are supported by a broad range of US states, while policies for other sectors and those targeting GHG emissions directly spread mostly in a subset of states and don't show (yet) a significant effect on energy generation and greenhouse gas emissions.

In Section 2 we analyze the effectiveness of climate policies within the state. In Section 3, we identify and quantify the diffusion of climate policy between states. Section 4 concludes.

2 Climate policy effects in US states

In this section we quantify the effectiveness of state-level climate policies. To do so we combine data on the adoption of 23 state-level climate policies in different states in different years with data on the expansion of renewable energy technologies and on greenhouse gas emissions. This allows us to quantify how much the introduction of a policy increased renewable energy technologies and/or reduced greenhouse gas emissions. Estimating the effectiveness of climate policies is also useful to quantify the benefits of policy diffusion. If a policy is effective in a state and also shows significant policy diffusion to other states, we can estimate how much the introduction of a policy reduces emissions in other states through increasing the probability that the other state introduces the same policy with a similar effectiveness. Linsenmeier et al. (2023) estimate "indirect" emission reductions through policy diffusion at country level. However, they do not estimate direct emission reductions. Our analysis of policy effectiveness therefore also allows us to contribute to the existing literature with more empirically calibrated estimates of the benefits of diffusion.

2.1 Methods

2.1.1 Data

We use data on the adoption of 26 climate policies collected by Bergquist and Warshaw (2023). The large number of policies reflects the diversity of existing climate policies. The ultimate objective of all these policies is the reduction of greenhouse gas emissions. We drop three policies that have a continuous scale of adoption, namely the level of gasoline taxes, energy efficienty resource standards, and RPS targets, because they do not lend themselves to studying effectiveness and diffusion in the same way as the other policies that have a binary coding. We classify the remaining 23 policies according to their instrument type based on a slightly adjusted version of the IEA classification proposed by (Linsenmeier et al., 2022). According to this classification, our datset includes policies of six different instrument types, with the highest number being regulatory instruments, followed by grants, subsidies, and other financial incentives (Figure 1). All policies are listed in SI Table A1. The adoption of policies over time is illustrated in SI Figure A1.

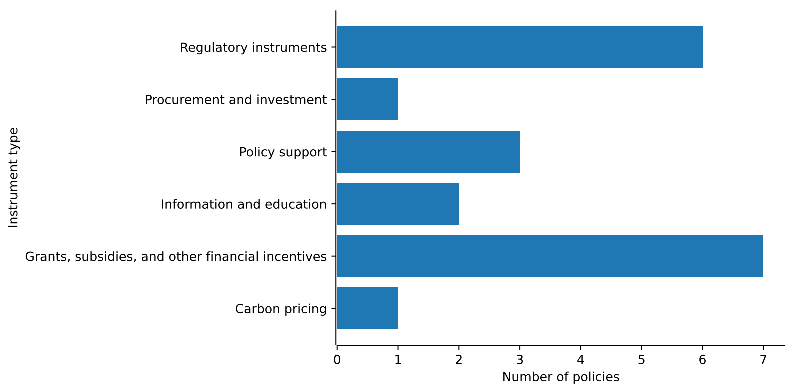

Figure 1: Policies in the dataset. See also SI Table A1.

We use a variety of control variables including those pertaining to the state's economy, energy sector, and geography. In total we consider 15 control variables (Table 1). The sources of all control variables can be found in SI Table A3. We complement the data with information on GHG emissions by state and sector, obtained from the US Energy Information Administration.

Variable	Unit	Mean	Sd	Min	Max
log GDP pc	USD per capita	-3.47	0.51	-4.64	-2.39
GHG intensity	t CO2eq per USD	58.64	11.71	25.65	85.70
Democratic control	Binary	0.45	0.50	0.00	1.00
Employment in mining	Percent	2.19	2.22	0.22	14.29
Employment in transport and utilities	Percent	24.27	2.22	18.01	31.86
GDP of mining	Percent	3.18	6.72	0.00	46.36
GDP of transport and utilities	Percent	6.91	2.74	2.62	18.67
Population density	People per km2	69.76	95.23	0.27	472.81
log Population	People	15.04	1.02	12.90	17.49
log Land area	km2	11.61	1.16	7.89	14.21
Wind power potential	m/s	6.61	0.63	5.26	7.80
Solar power potential	kWh per m2 per day	4.40	0.58	2.27	5.68
Coal production	Binary	0.50	0.50	0.00	1.00
Share evangelical	Percent	19.32	14.21	1.10	74.00
Concern about climate change	Score	0.13	0.77	-2.67	2.86

Table 1: Descriptive statistics.

2.1.2 Econometric analysis

Estimating the effect of a single policy is difficult for two key reasons. First, policy instruments are often too weak to show a strong effect in the short term. In addition, the introduction of a single policy provides too few observation to derive statistically significant results. Studies of single policies have thus concentrated on carbon pricing, where the variation in the price level provides more observations. Pretis (2022) finds that the carbon tax in British Columbia reduced transportation emissions but has not led to large statistically significant reductions in aggregate CO2 emissions. The author explains this with the low level of the carbon tax. Bayer and Aklin (2020) investigate the effect of the EU-ETS on emissions in Europe between 2008 and 2016, a time when the price in the EU-ETS was still low. The authors estimate counterfactual carbon emissions and find that emission reductions have been substantial. Yan (2021) finds that the Regional Greenhouse Gas Initiative (RGGI) reduced coal and natural gas consumption considerably in the regulated states and increased it in unregulated states, leading to a net decrease in emissions. In a meta-analysis on the effect of carbon pricing on emissions, Green (2021) finds that the majority of studies estimate the aggregate reductions from carbon pricing on emissions to between 0% and 2% per year.

To avoid the challenges of measuring the effectiveness of each individual policy, we employ a pooled regression model that estimates the average effect of all policies in our sample. The model is based on a "canonical" distributed lag model for each policy that includes state i and year t fixed effects:

$$\text{Growth rate in emissions}_{i,t} = \sum_{n=0}^{5} \text{Policy adoption}_{i,t-n}\beta + \mu_i + \xi_t + \epsilon_{i,t}. \tag{1}$$

In the pooled regression, the state and year fixed effects become policy-by-state and policy-by-year fixed effects. With index p for policies, the pooled model can be written as:

$$\text{Growth rate in emissions}_{i,t,p} = \sum_{n=0}^{5} \text{Policy adoption}_{i,t-n,p}\tilde{\beta} + \mu_{i,p} + \xi_{t,p} + \epsilon_{i,t,p}. \tag{2}$$

where the coefficient $\tilde{\beta}$ quantifies the average effectiveness of all policies. We cluster standard errors by state thereby accounting for their correlation over time as well as across policies.

The main explanatory variables are cumulative climate policies, meaning the total number of climate policies in a given state by a given year. The climate policies are those collected by Bergquist and Warshaw (2023), see Table A1. We consider three groups of policies. First, all policies are pooled together. Second are those policies that target renewable energy or the energy sector broadly. They are marked as "R/E" in Table A1. Third are those policies that either target a sector other than energy (transportation or buildings) or target greenhouse gas (GHG) emissions without sectoral limitation. They are marked as 'Sect." in Table A1. This grouping is based on the hypothesis that the policies targeting the energy sector might have a more immediately measurable effect as they are more concrete and short-term.

We use three different groups of dependent variables. The first group is the share of renewable energy in total energy capacity. With this, we can test the hypothesis that climate policies cause a shift in capacity additions from fossil fuels to renewable energy. In addition to the effect on the total of all renewable energy sources, we estimate the effect of policies on solar energy capacity, wind energy capacity and hydropower capacity. The second group of variables is the share of renewable energy in net generation of electric power, which is similarly divided into the share in net generation for total renewable energy, solar power, wind power and hydropower. The third group of dependent variables are state energy-related CO2 emissions. This variable covers energy-related emissions from all sectors. Data on total emissions are not available at the state level. For this group, we estimate the effect of climate policies on total emissions and on per capita emissions.

2.2 Results

Estimation results for the first group of dependent variables are shown in Table 2. The tables show the third lag, meaning that the outcome variables are observed three years after the policy variables. The third lag seems suitable, because constructing power plants and reducing emissions takes time. However, we estimate the effects for other lags as well as a robustness test. The results are very similar for different lags. These results are available in the appendix.

Columns (1) to (3) show that none of the three policy groups have a significant effect on the share of all renewable energy sources in total energy capacity. This is surprising, because most of the climate policies in US states are targeted at the energy sector and most of these directly facilitate the production of renewable energy. However, during the period covered in our data, the share of wind and solar energy in total electricity production was very small. By contrast, there are considerable shares of hydropwer. The effect of policies on wind and solar power might thus become indistinguishable, given the large amount of hydropower. In addition, hydropower plants are very large investments and require suitable technical potential. They are thus constructed independently of climate policies as individual major projects.

To identify the effect of climate policy on the production of renewable energy capacity, we thus estimate the effect of policies for each type of renewable energy separately. For solar power, we find that all policies together had a positive and significant effect on solar energy capacity, see column (4). It is plausible to expect that the results for solar energy were driven by the policies passed for the energy sector. Column (5) shows that this is indeed the case. The policies targeted at sectors other than energy and targeted at GHG emission reduction, do not have a significant effect. As mentioned earlier, hydropower capacity cannot be expected to react to policies on incentives for renewable energy. Indeed, columns (7) to (9) show not significant effects.

The estimates for the effect of climate policies on wind energy capacity have negative coefficients and the estimate for sectoral and GHG reduction is significant at 10%. This result is contrary to intuition. It is not plausible that policies aimed at reducing emissions in transportation, buildings or economy-wide would discourage the construction of wind energy capacity. Instead, it seems that the negative correlation originates in difference between states with different shares of rural populations. US states with large rural populations are less likely to pass policies aimed at GHG emissions. At the same time, they have more space and lower population density, which means they have good conditions for generating wind energy[1]. Trachtman (2020), for example, finds that local co-benefits can overpower partisan concerns, so that "windy, rural states" would build wind energy to benefit from it economically.

The effect of climate policies on renewable energy generation is most likely to be detectable for energy capacity, because capacity does not depend on weather and technical limitations. Energy production by contrast is affected by these factors, for example if hydropower production reduces during a drought or power plants are off the grid for maintenance work. Table 3 shows that the results for energy capacity apply to energy generation as much as for energy capacity.

The positive effects of climate policies on solar energy production are not reflected in a significant decrease in energy-related GHG emissions in our estimates, see Table 4. This reflects that climate policies did not have a significant effect on aggregate renewable energy production. Solar energy, for which we identify a positive and significant effect, had a share of only 0.1% of electricity generation in 2012 and 4.8% in 2022. While these numbers are too small to have a detectable effect on aggregate emissions, the strong increase means that it is plausible to expect a measurable effect in a few years. We also estimate the effect of climate policy on emissions only from the energy sector, but results were insignificant as well, see the appendix.

[1] CNN writes, "Wind energy is booming in deep-red Republican states". https://www.cnn.com/2022/04/22/politics/wind-energy-oklahoma-republican-states-climate/index.html

Table 2: Climate Policies and the Share of Renewables in Total Energy Capacity

Variable	*All Renewables* (1) All policies	(2) R/E policies	(3) Other policies	*Solar* (4) All policies	(5) R/E policies	(6) Other policies	*Hydro* (7) All policies	(8) R/E policies	(9) Other policies	*Wind* (10) All policies	(11) R/E policies	(12) Other policies
Climate policies	-0.00379	0.00290	-0.0352	0.0288**	0.0257**	0.0262	0.0307	0.0144	0.0311	-0.0633	-0.0372	-0.0925*
	(0.0388)	(0.0482)	(0.0500)	(0.0140)	(0.0126)	(0.0231)	(0.0207)	(0.0313)	(0.0243)	(0.0392)	(0.0426)	(0.0534)
State GDP	-10.55	-10.64	-10.34	-8.009	-7.991	-7.975	-10.60*	-10.58*	-10.60*	8.066	7.927	8.236
per capita	(11.72)	(11.71)	(11.72)	(6.981)	(6.972)	(6.986)	(5.536)	(5.552)	(5.540)	(12.51)	(12.55)	(12.50)
State mining	0.0159	0.0159	0.0157	0.0109	0.0108	0.0109	-0.00260	-0.00268	-0.00255	0.00758	0.00777	0.00730
share of GDP	(0.0201)	(0.0201)	(0.0202)	(0.00890)	(0.00890)	(0.00890)	(0.00848)	(0.00852)	(0.00850)	(0.0186)	(0.0187)	(0.0186)
Democrat-controlled	0.181	0.181	0.184	0.0792	0.0793	0.0790	0.0587	0.0599	0.0587	0.0434	0.0417	0.0461
	(0.122)	(0.121)	(0.122)	(0.0587)	(0.0588)	(0.0588)	(0.0699)	(0.0698)	(0.0706)	(0.0906)	(0.0902)	(0.0908)
Constant	-0.243	-0.248	-0.253	-0.0439	-0.0523	-0.0464	0.601***	0.616***	0.600***	-0.800**	-0.811**	-0.806**
	(0.414)	(0.417)	(0.416)	(0.201)	(0.201)	(0.201)	(0.205)	(0.210)	(0.205)	(0.347)	(0.347)	(0.348)
Observations	34,350	16,437	11,947	34,350	16,437	11,947	34,350	16,437	11,947	34,350	16,437	11,947
Number of states	50	50	50	50	50	50	50	50	50	50	50	50
R-squared (within)	0.139	0.139	0.139	0.205	0.205	0.205	0.0402	0.0401	0.0403	0.159	0.159	0.160
R-squared (overall)	0.209	0.209	0.209	0.286	0.286	0.286	0.117	0.117	0.117	0.295	0.295	0.296

Notes: Table 1 reports results from a regression of the renewable energy share of total energy capacity on [23] climate poliices and state-level control variables. We further disaggregate the results for the 11 renewable and energy policies only ("R/E policies") and 8 sectoral and GHG emissions policies only ("Other policies"). The unit of observation in this regression is a state-year-policy. That is, for "All policies", the data are stacked such that for every state and every year there are [23] distinct policy variables, each taking the value 1 if the climate policy is in effect and 0 otherwise (for the R/E and Other policies regressions are stacked with 15 and 8 observations per state-year). Under this stacked specification, every regression is able to include a full set of state X climate policy and year X climate policy fixed effects. Standard errors reported in parenthesis below the regression coefficient estimates are robust (clustered at the state level). *** p<0.01, ** p<0.05, * p<0.1

Table 3: Climate Policies and the Share of Renewables in Net Generation of Electric Power

Variable	*All Renewables* (1) All policies	(2) R/E policies	(3) Other policies	*Solar* (4) All policies	(5) R/E policies	(6) Other policies	*Hydro* (7) All policies	(8) R/E policies	(9) Other policies	*Wind* (10) All policies	(11) R/E policies	(12) Other policies
Climate policies	-0.004	0.003	-0.035	0.028**	0.026**	0.026	0.0307	0.0144	0.0311	-0.0633	-0.0372	-0.093*
	(0.0388)	(0.0482)	(0.0500)	(0.0140)	(0.0126)	(0.0231)	(0.0207)	(0.0313)	(0.0243)	(0.0392)	(0.0426)	(0.0534)
State GDP per capita	-10.5	-10.6	-10.3	-8.01	-7.99	-7.98	-10.6*	-10.6*	-10.6*	8.07	7.93	8.24
	(11.7)	(0.117)	(0.117)	(0.069)	(0.069)	(0.07)	(0.055)	(0.055)	(0.055)	(0.125)	(0.125)	(0.125)
State mining share of GDP	0.02	0.02	0.02	0.0109	0.0108	0.0109	-0.0026	-0.00268	-0.00255	0.00758	0.00777	0.0073
	(0.020)	(0.020)	(0.020)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.018)	(0.018)	(0.018)
Democrat-controlled	0.181	0.181	0.184	0.0792	0.0793	0.079	0.0587	0.0599	0.0587	0.0434	0.0417	0.0461
	(0.122)	(0.122)	(0.122)	(0.0587)	(0.0587)	(0.0587)	(0.0699)	(0.0699)	(0.0706)	(0.0906)	(0.0902)	(0.0908)
Constant	-0.243	-0.248	-0.253	-0.0439	-0.0523	-0.0464	0.601***	0.616***	0.600***	-0.800**	-0.811**	-0.806**
	(0.414)	(0.417)	(0.416)	(0.201)	(0.201)	(0.201)	(0.205)	(0.210)	(0.205)	(0.347)	(0.347)	(0.348)
Observations	34,350	16,437	11,947	34,350	16,437	11,947	34,350	16,437	11,947	34,350	16,437	11,947
Number of states	50	50	50	50	50	50	50	50	50	50	50	50
R-squared (within)	0.139	0.139	0.139	0.205	0.205	0.205	0.0402	0.0401	0.0403	0.159	0.159	0.160
R-squared (overall)	0.209	0.209	0.209	0.286	0.286	0.286	0.117	0.117	0.117	0.295	0.295	0.296

Notes: Table 2 reports results from a regression of the renewable energy share of total net generation of electricity on [23] climate policies and state-level control variables. We further disaggregate the results for the 11 renewable and energy policies only (”R/E policies”) and 8 sectoral and GHG emissions policies only (”Other policies”). The unit of observation in this regression is a state-year-policy. That is, for ”All policies”, the data are stacked such that for every state and every year there are [23] distinct policy variables, each taking the value 1 if the climate policy is in effect and 0 otherwise (for the R/E and Other policies regressions are stacked with 15 and 8 observations per state-year). Under this stacked specification, every regression is able to include a full set of state X climate policy and year X climate policy fixed effects. Standard errors reported in parenthesis below the regression coefficient estimates are robust (clustered at the state level). *** p<0.01, ** p<0.05, * p<0.1

Table 4: Climate Policies and the Share of Renewables in Net Generation of Electric Power

Variable	*Total state energy-related CO2 emission* (1) All policies	(2) R/E policies	(3) Other policies	*Per capita state energy-related CO2 emissions* (4) All policies	(5) R/E policies	(6) Other policies
Climate policies	-0.72	-0.30	-1.28**	0.220	0.249	0.204
	(0.46)	(0.51)	(0.53)	(0.223)	(0.220)	(0.239)
State GDP per capita	-121.50	-123.40	-119.1	-35.78	-35.84	-35.71
	(252.3)	(253.1)	(251.3)	(83.05)	(82.98)	(83.07)
State mining share of GDP	0.35	0.36	0.347795	0.190	0.190	0.190
	(0.35)	(0.359)	0.355)	(0.127)	(0.127)	(0.128)
Democrat-controlled	-2.00	-2.03	-1.962	0.251	0.246	0.253
	(1.99)	(2.01)	(1.962)	(0.408)	(0.408)	(0.407)
Constant	26.02***	26.13***	25.9***	56.35***	56.35***	56.34***
	(8.82)	(8.86)	(8.77)	(2.684)	(2.684)	(2.683)
Observations	34,500	16,500	12,000	34,500	16,500	12,000
Number of states	50	50	50	50	50	50
R-squared (within)	0.342	0.342	0.343	0.507	0.507	0.507
R-squared (overall)	0.992	0.992	0.992	0.987	0.987	0.987

Notes: Table 3 reports results from a regression of million metric tons (columns 1-3) and metric tons per capita (columns 4-5) of energy-related CO2 emissions on [23] climate policies and state-level control variables. We further disaggregate the results for the 11 renewable and energy policies only ("R/E policies") and 8 sectoral and GHG emissions policies only ("Other policies"). The unit of observation in this regression is a state-year-policy similar to Tables 1 and 2. All regression include a full set of state X climate policy and year X climate policy fixed effects. Standard errors reported in parenthesis below the regression coefficient estimates are robust (clustered at the state level). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

3 Diffusion of climate policies between US states

In this section we study the diffusion of climate policies between states of the US. We first examine empirically how policy adoption in any one state was influenced by prior adoption of the same policy in other states. We then use our empirical estimate of the strength of diffusion for simulations of future policy adoption. Finally, we use the simulations to quantify the benefits of diffusion. This allows us to compare the magnitude of the direct benefits of policy adoption and the indirect benefits from policy diffusion. We conduct our simulations for a wide range of parameter values to assess the robustness of our results. These sensitivity tests include a parameter that determines the policy effectiveness. For this parameter we also use our empirical estimate from Section 2.

3.1 Methods

3.1.1 Empirical model

We model diffusion with a Cox proportional hazard model which is frequently used in the study of policy diffusion (Sugiyama, 2011; Sauquet, 2014; Abel, 2021; Dolphin and Pollitt, 2021; Linsenmeier et al., 2023). The main advantages of this model are that it accounts for the binary nature of the outcome variable (policy adoption) and that it accounts for the right-censoring of that variable. In mathematical notation the model can be written as:

$$h(t, X_{i,t}, W_{i,t}) = h_0(t) \exp(X_{i,t-1}\beta_X) \exp(W_{i,t-1}\beta_W) \tag{3}$$

The hazard function $h(.)$ of state i in year t represents the probability that the policy is adopted by that state in that year conditional on it not yet being adopted. This hazard rate is composed of a baseline hazard rate $h_0(t)$ and a second partial hazard term that includes the time-dependent matrices of control variables $X_{i,t-1}$ and of spatial lags $W_{i,t-1}$. The non-parametric Cox baseline hazard term means that the results are more robust to miss-specification than results from parametric survival models (Lee and Wang, 2003). Standard errors are clustered at the level of states to account for serial correlation.

The model is estimated using panel data. We use the same dataset on climate policies as in Section 2, but we drop three policies for which our model does not converge. These policies have somewhat peculiar patterns of adoption that hinder the study of diffusion. The first policy, state preemption of local gas bans, has been adopted by only four states. A second policy, low income energy efficiency policies, has been adopted in only three different years. A third policy, environmental policy acts, were, with one exception, already adopted in the 1970s, much earlier than all other policies.

The matrix of control variables $X_{i,t-1}$ accounts for possible domestic influences on policy adoption in state i in year $t-1$. To reduce the risk of reverse causality, the matrix is lagged by one year. We consider 15 control variables for our model (Table 1). The selection of control variables is further discussed further below.

The model structure implies a proportional hazard assumption, which we test using Schoenfeld residuals (Grambsch and Therneau, 1994). The assumption is generally satisfied for most models that we estimate. We drop all other results from further statistical analysis. The binary outcome variable $P_{i,t}$ is policy adoption in state i in year t and is also used for the construction of the spatial lag, as explained in the following.

3.1.2 Proximity metrics

The matrix $W_{i,t-1}$ is a weighted average of policies adopted in other states $j = 1, ..., N, i \neq j$ at time $t-1$. Formally, the matrix is calculated as

$$W_{i,t} = \frac{\sum_{j=1,j\neq i}^{N} w_{i,j,t} P_{j,t}}{\sum_{j=1,j\neq i}^{N} w_{i,j,t}} \tag{4}$$

with $w_{i,j,t}$ the weight of state j for state i in year t. We use six alternative specifications for these weights. For geographical proximity we use a binary variable indicating whether two states (i, j) border each other. As an alternative metric we use the inverse of the distance between the geometric centroids of states $d_{i,j}$ as $w_{i,j} = \frac{1}{d_{i,j}}$. A third metric is motivated by gravity models of trade, taking into account not only distance but also the size of the economy of a state. For that metric, we divide the log GDP of a state by the distance: $w_{i,j} = \frac{\log \text{GDP}_j}{d_{i,j}}$. Furthermore, we construct a metric that uses information about the party in political control of a state. We measure political control based on the majority of the state house of representatives, state senate, and the governor. We quantify control as a continuous (maybe "discrete"? takes only 3 values) variable C with three possible values (Democrats = 0, Republican = 1, Split either between house and senate or between the governor from the house and senate = 0.5) and calculate the fourth weight as $w_{i,j} = 1 - |C_i - C_j|$. This metric therefore assigns the weight 1 to all states controlled by the same party in a given year and 0 to all states controlled by the opposite party in that year.

Furthermore, to examine the presence of additional biases we conduct a placebo test. For this purpose we construct an additional matrix $W_{i,t}$ for which we assign a random value for proximity to every state pair for every year $w_{i,j,t}$ by drawing from a Weibull distribution that we fit to the distribution of the physical distance between states.

3.1.3 Monte Carlo simulations

We also use our empirical estimates for Monte Carlo simulations to illustrate the magnitude of the estimates and to quantify the benefits from policy diffusion. The methodology follows closely the methodology developed in Linsenmeier et al. (2023).

For all simulations we use our empirically estimated model (Equation 3). The model predicts the hazard of adoption given the known covariates X and the spatial lag W. In our simulations we keep all covariates constant and iteratively simulate adoption and diffusion. We start the simulations in 2024 and stop in 2050. At every time step we first calculate for every state the probability of policy adoption based on its covariates and its spatial lag. We then simulate adoption given this probability of success with the use of a random variable. After we simulate adoption for all states, we update all spatial lags and proceed to the next timestep. Because our model of policy adoption is probabilistic, we conduct Monte Carlo simulations. In practice, this means that we simulate every scenario 10,000 times and then quantify the effects of diffusion based on the distribution of results across the Monte Carlo simulations.

We first quantify the benefits of diffusion in terms of the number of other states whose policy adoption by 2050 can be attributed to diffusion. To do so, we simulate two counterfactual scenarios. For both scenarios all simulations start in the year 2024 without any policy yet being adopted. From that starting point we simulate several scenarios with different values of the baseline hazard and the diffusion parameter β_W. These scenarios include a scenario with $\beta_W = 0$. This scenario without diffusion can be considered the baseline scenario. For all other scenarios with $\beta_W > 0$ and different values of the baseline hazard the difference from the baseline scenario in terms of the number of states that adopt the policy in a given year gives us the effect of diffusion.

In a second exercise we study for which states leadership and subsequent diffusion is particularly effective. Furthermore, to make the benefits of diffusion more illustrative, we translate the number of additional states with the policy from diffusion into cumulative emission reductions in those other states. We refer to these emission reductions in other states that can be attributed to policy diffusion as indirect emission reductions. To make sense of their magnitude, we compare them with the direct emission reductions in the same state. To make the two estimates of emission reductions comparable, we assume that the policy is equally effective in all states. For this exercise, we simulate two counterfactual scenarios for every state, one in which the state adopts the policy in the year 2024 and one in which the state does not do so. All other states are assumed to not adopt the policy in 2024. In both scenarios we set the diffusion parameter to the empirical estimate. This means that the difference between the two scenarios gives us the benefits of the adoption and diffusion of that first policy in that state.

In a third and a fourth exercise, we make more realistic assumptions that are based on our empirical analysis of the effectiveness of climate policies. These exercises use our estimated coefficient presented in Table 2 Column 4 that quantifies how much the adoption of a climate policy increases the share of total installed generation capacity of solar power relative to a counterfactual (about 0.029 percentage points per year in every year following the adoption of the policy). With this estimate we first translate policy adoption into the installation of additional direct solar generation capacity (in the state that adopts the policy first) and additional indirect solar generation capacity (in all other states). We again calculate the additional capacities using the difference between a scenario and a counterfactual scenario. For our last exercise, we translate these additional installed capacities into cumulative greenhouse gas emissions. For this translation, we assume that solar replaces the dirtiest existing electricity generation technology in each state.

3.2 Results

3.2.1 Empirical analysis of policy diffusion

We first study the adoption and diffusion of state-level climate policies empirically. Similar to Bromley-Trujillo et al. (2016) we are interested in the average strength of diffusion of climate policies. We extend the analysis in Bromley-Trujillo et al. (2016) in three ways. First, we include a larger set of climate policies in the analysis (20 versus 14). Furthermore, our policies consistently focus on mitigation. For example, Bromley-Trujillo et al. (2016) include the policies "Adaptation Plan" and "Advanced Coal Technology". We do not consider these policies as mitigation policies, the latter because of the need to discontinue the use of coal altogether. Second, we study a longer and more recent time period. Our analysis covers the years 1980-2020, whereas Bromley-Trujillo et al. (2016) cover only the years 1994 to 2009. Third, we study diffusion with several alternative proximity metrics. These metrics can be related to different channels or mechanisms of diffusion. Bromley-Trujillo et al. (2016) consider only the effect of neighbor states, whereas we include also the physical distance between states, size of a state's economy, and political alignment (Section 3.1.2).

An important consideration concerns the inclusion of control variables in our empirical model. The inclusion represents a dilemma. On the one hand, not including a potentially important variable risks that the estimate of our main parameter of interest, the coefficient of the spatial lag of policy adoption, is biased due to that omitted variable. On the other hand, including variables that are strongly correlated with the spatial lag risks that the same parameter is biased due to collinearity. To better understand this trade-off we estimate a variety of models with the spatial lag constructed from the placebo metric. The use of this metric has the advantage that the expected estimate of the diffusion parameter is zero and we can thus scrutinize any deviations from this value.

We provide an extensive discussion of omitted variable biases and collinearity in SI Section B which we summarise here briefly. Our analysis reveals that any omitted variable that affects the probability of policy adoption either positively or negatively leads to a negative (downward) bias of the coefficient of the spatial lag. This can be explained with the fact that the spatial lag is constructed from the history of policy adoption and thus mechanically correlated with any variable that affects the probability of adoption. Furthermore, we find that some of the control variables that are important for explaining policy adoption, especially log GDP per capita, are strongly correlated with the spatial lag (Pearson correlation coefficient > 0.8). We address this collinearity with a principal component analysis and a machine learning algorithm for variable selection. After careful analysis we find that a model with 6 principal components constructed from our 15 control variables represents the best trade-off between omitted variable bias and collinearity (SI Section B).

Using this preferred model, we first study the relative strengths of the different proximity metrics in explaining diffusion of climate policies over time. For this purpose, we quantify for every proximity metric the average model fit across the 20 climate policies based on the log likelihood ratio. We find that the metric based on the political alignment of states models the process of diffusion between states best (Table 5). When we distinguish between energy policies and other policies, we find that the party performs particularly well for other policies. For energy policies, the performance of the different metrics is very similar (Table 5).

Policies	Distance	Gravity	Neighbour	Party
All	169.2	169.0	168.9	167.6
R/E	168.6	168.7	168.2	168.6
Others	169.9	169.4	169.7	166.5

Table 5: Results for different proximity metrics. The table shows the log likelihood ration (llr). Note that a smaller value indicates a better model fit.

We next quantify the strength of diffusion for this metric. We again use our preferred model with six principal components. Similar to Bromley-Trujillo et al. (2016) we find a relatively large variation of the estimated parameter across climate policies (SI Figure C1. Across policies, the interquartile range of the parameter is [-0.5, 4.6] with a median estimate of 1.95. We also compare our central estimate with coefficients estimated in prior literature on climate policy diffusion. Reassuringly, we find that our estimate is well in the range of prior estimates (Figure 2 left).

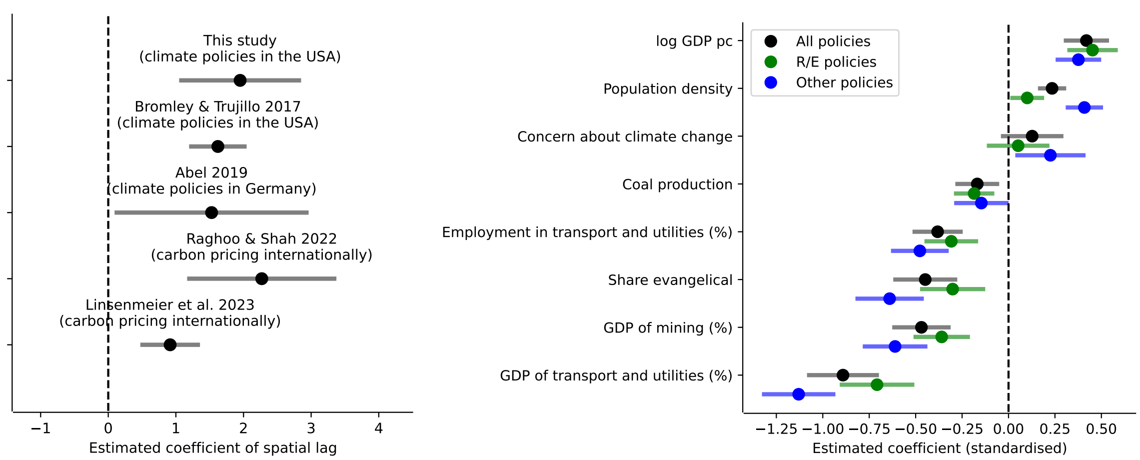

Figure 2: Left: Comparison of estimated coefficient of diffusion with values in the prior literature. Right: Estimated coefficients of control variables representing their average effect on policy adoption across policies. The coefficients are standardised to make their magnitude comparable.

Our approach based on principal components instead of the original variables means that the interpretation of the estimated coefficients of our control variables is not straightforward. To address this limitation, we use the estimated model of each policy and make predictions. We then average the predicted values across policies and regress the average predicted values of all states on our original variables. We use a Lasso regression to identify the most important predictors. The results are shown in Figure 2 (right). Overall, GDP per capita has the largest positive effect on climate policy adoption, followed by population density and expressed concern about climate change. At the other end of the spectrum, the share of transportation, utilities, and mining in a state's GDP are most negatively associated with the probability of climate policy adoption. Also the share of the Evangelical population and the presence of coal mines have a negative estimated coefficient.

When we distinguish between energy policies and other policies, we find that other policies tend to exhibit a stronger positive influence of population density and concern about climate change and a stronger negative influence of the share of evangelicals as well as the employment in transport and utilities and GDP of mining and utilities (Figure 2). These results are consistent with the hypothesis that the adoption of these policies, which are generally more about climate change than about energy, is stronger influenced by the political context.

3.2.2 The indirect benefits from policy diffusion

We next use our empirical estimates to simulate future policy adoption. We first quantify how many more states may adopt a future climate policy because of policy diffusion. The results of the simulations depend primarily on two key parameters: the baseline hazard and the strength of policy diffusion. For the baseline hazard we assume a mean value across states and then use our empirically estimated coefficients of all covariates (Figure 2 right) to map these coefficients onto the covariate-adjusted baseline hazard of each state. The covariate-adjusted baseline hazards of all states for a mean baseline hazard of 5 percent are shown in SI Figure C3. Because the baseline hazard of a future policy is unknown, we conduct a sensitivity analysis in which we vary the parameter between 1 and 10 percent.

For the second parameter, the strength of policy diffusion as expressed by the coefficient of the spatial lag β_W, we use our empirical estimate of 1.95 but also conduct again a sensitivity analysis in which we vary the parameter between 0 and 10. The scenario with $\beta_W = 0$ can be considered the counterfactual scenario because this parameter value imples that the simulations do not include any effect of diffusion.

Overall, we find that policy diffusion can substantially increase the adoption in other states for a wide range of values of our two key parameters (Figure 3 left). For our empirical estimate of the strength of diffusion and a mean baseline hazard of 5 percent, we find that the share of states that adopt the policy by 2030 increases by 10 percentage points relative to a scenario without diffusion (40% versus 31%). For a diffusion parameter of 4, the effect of diffusion increases to 38 percentage points (69% versus 31%). For the same diffusion parameter of 1.95 but a twice as large baseline hazard of 10 percent, the effect of diffusion increases to 30 percentage points (80% versus 50%).

We also examine how much policy diffusion helps to achieve a certain coverage of a policy in terms of states earlier than in a counterfactual scenario without diffusion. For our empirical estimate $\beta_W = 1.95$ we find that the same coverage that is achieved in the scenario without diffusion by the year 2050 is already reached about 15 years earlier, by around the year 2035 (Figure 3 right).

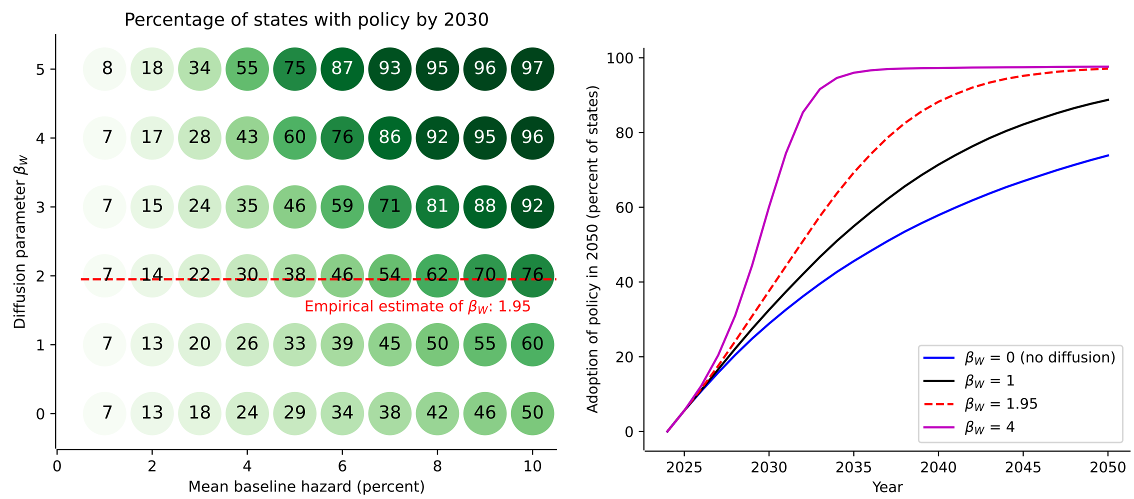

Figure 3: Left: Policy adoption for different values of the baseline hazard and the diffusion parameter. Right: Policy adoption over time for a baseline hazard of 5% and different values of the diffusion parameter.

We next quantify the benefits of diffusion for this metric relative to the domestic benefits of adoption. For the simulations we assume for each state S that it is the first to adopt the policy in 2024 and simulate diffusion and adoption of all states until 2050. We compare the results with a counterfactual scenario in which no state adopts the policy in 2024. The difference between the two scenarios gives us the effect of the policy adopted by state S on policy adoption in all other states. We conduct these simulations for every state S as the pioneering state to examine for which states the benefits from leadership and policy diffusion are relatively large. We again conduct the simulations for a baseline hazard of 5% and our empirical estimate of the diffusion parameter of 1.95.

For these simulations we use our estimates for the effectiveness of climate policies in increasing solar electricity generation from Section 2 which allow us to translate the adoption of a policy into climate benefits. We first simulate how policy adoption and policy diffusion affect the expansion of solar capacity and then translate this capacity into reductions in GHG emissions. For this latter calculation we assume that additional solar electricity generation replaces coal, if the state uses more than 0.5% of coal in it's electricity mix. If the state has less than 0.5% coal, we assume it replaces natural gas. Only Vermont uses neither coal, nor natural gas, so that we assume that additional solar energy does not reduce emissions in Vermont. For coal and natural gas, we use the respective emissions per MWh to determine the emission reductions caused by more solar energy generation. See table A2 for an overview by state.

Our results suggest that the benefits of diffusion are of a similar magnitude as the domestic benefits of adoption. In terms of additionally installed solar capacity, we find that 38% of states have larger indirect than direct benefits of policy adoption (Figure 4 left). Translating this expansion of solar capacity into cumulative direct and indirect emission reductions we find that the indirect emission reductions are larger than direct emission reductions for 46% percent of states (Figure 4 right). This suggests that leadership by those states is particularly beneficial for total emission reductions in the US. The geographic distribution of the indirect emission reductions is shown in SI Figure C5.

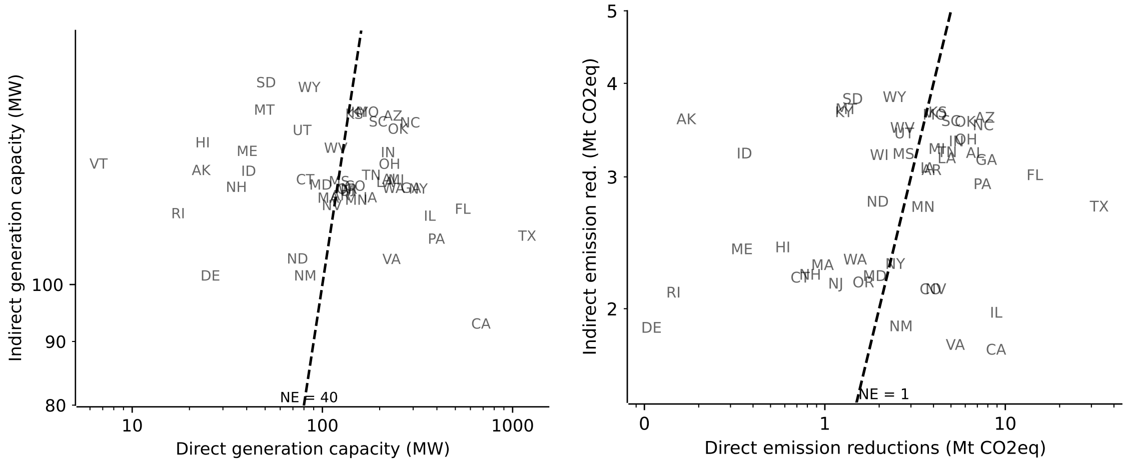

Figure 4: Direct and indirect installed capacity of solar power (left) and cumulative emission reductions (right) using our empirical estimate of the effectiveness of climate policies in increasing the share of solar in total installed generation capacity. See Methods section for more details. Metric based on party.

These results are based on our empirical estimates of policy effectiveness in increasing solar capacity and use heterogeneity across states in terms of the existing mix of generation technologies. In robustness tests, we follow Linsenmeier et al. (2023) and assume that a policy reduces GHG emissions by 1% per year in every year after policy adoption. We assume the same effectiveness for emission reductions in all states, which has the advantage that our comparison of direct and indirect emission reductions is mostly insensitive to this parameter (see Linsenmeier et al. (2023) for a formal analysis). Reassuringly, with this simplification we find essentially the same relative magnitudes of indirect and direct benefits as above (indirect > direct for 46 % of states; SI Figure C4 left). For additional robustness, we repeat these simplified simulations using the metric based on the physical distance between states. This is motivated by the fact that prior studies such as Bromley-Trujillo et al. (2016) also used this metric. The overall result is again very similar, with indirect emissions being larger than direct emission reductions for 56% of states (SI Figure C4 right).

4 Conclusion

The results of this paper shed light on the potential and limitations of state climate policy. Starting with the potential, we find that climate policy has a measurable effect within the implementing state, but importantly it also has positive spillover effects. We find that climate policies, in particular those in the energy sector, accelerate the uptake of solar energy. Given the strong growth rates of solar energy across the world, setting a supportive framework early on can be expected to make these policies extraordinarily successful in the long run. Further, by taking the lead, states encourage each other to take further steps. Taking previous research on policy innovation and policy learning into account, the spillover effect could partly be driven by a dynamic where the most capable states develop new policy ideas and try them out. Other states benefit from getting a blueprint to copy and from observing the success of the policy. The combination of direct benefits and spillovers offers a plausible explanation for why states might implement climate policy unilaterally in the first place.

The absence of results for state climate policies beyond solar energy shows that they have little direct effect. Other research does identify an effect of individual policies, carbon pricing in particular, on GHG emissions in the US (Yan, 2021) as well as other variables, like health (Perera et al., 2020).

Further, states might be laying an effective foundation for emission reductions in the long term. Nevertheless, the results indicate that state climate policy is at least much less effective than federal policy. The Inflation Reduction Act, for example, is expected to reduce emissions dramatically (Bistline et al., 2023). A key difference between state and federal level legislation, is that states need to be much more mindful of competitiveness, as shifting economic activity between US states is easier than across international borders and the federal government can control international trade in a way that is not available to state governments for interstate trade.

The results regarding groups of climate policies shows that there are two levels of difficulty for climate policies. Policies in the energy sector, most of which are designed to facilitate renewable energy production, have a tangible benefit. Solar energy has recently become the lowest cost form of electricity generation (Way et al., 2022). In addition, it is flexible enough to be added to private homes and businesses, thus creating the option for households and businesses to profit financially. The policies thus produce concrete and short-term benefits without concerns about competitiveness. These policies are spreading broadly. Regulating sectors like transportation and buildings and reducing greenhouse gases often make economic sense: Encouraging the adoption of EVs prepares the industry for a coming trend. Insulating buildings saves on future fuel expenses. Reducing emissions has tangible health benefits for the population. Yet, the link between the policy and the benefit is less direct and might feel like a restriction. Passing policies of this kind requires a more determined state government. It is also the type of policy, where federal regulation is most warranted.

A future research step is to investigate to which degree state climate policy enables states to benefit from federal policy, once it is passed. The IRA provides the opportunity to study this question, given that it is designed to allocate investments to the states which are able to make use of them. The research could analyze if states with more climate policies are better prepared to absorb IRA funding. First results show that states with particularly good potential for renewable energy attact much FDI funding, but the renewable energy potential could be controled for. In a second step, the most effective policies for attracting IRA funding could be identified.

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Supplementary Information (SI)

A Descriptive statistics

Policy	Instrument type	Count	Group	Diffusion	Effects
On-site renewable generation	Incentives	44	R/E	Yes	Yes
Environmental utility deregulation	Regulatory instruments	17	R/E	Yes	Yes
Fuel generation mix disclosure	Information and education	24	R/E	Yes	Yes
Public Benefit Fund	Incentives	24	R/E	Yes	Yes
Environmental Building Standards	Regulatory instruments	42	Other	Yes	Yes
Gas decoupling	Incentives	38	R/E	Yes	Yes
Electric decoupling	Incentives	41	R/E	Yes	Yes
Climate action plan	Policy support	33	Other	Yes	Yes
Mandatory green power option	Regulatory instruments	12	R/E	Yes	Yes
GHG target	Policy support	27	Other	Yes	Yes
Energy efficiency target	Policy support	26	R/E	Yes	Yes
CA Car Emissions Standard	Regulatory instruments	17	Other	Yes	Yes
Greenhouse Gas Cap	Carbon pricing	14	Other	Yes	Yes
Greenhouse gas registry/ reporting	Information and education	18	Other	Yes	Yes
Emissions performance standards	Regulatory instruments	6	Other	Yes	Yes
Community Solar	Incentives	20	R/E	Yes	Yes
Complete Streets Policies	Procurement and investment	25	Other	Yes	Yes
PACE authorization	Incentives	36	R/E	Yes	Yes
Renewable Portfolio Standard	Regulatory instruments	31	R/E	Yes	Yes
Solar Tax Credit	Incentives	37	R/E	Yes	Yes
Low-income energy efficiency programs	[Simultaneous adoption]	43	R/E	No	Yes
Environmental Policy Act	[Adopted before 1990]	16	Other	No	Yes
State preemption of local gas bans	[Less than 5 adoptions]	4	Other	No	Yes
Gas tax	[Non-binary coding]	n/a	Other	No	No
Energy efficiency resource standard	[Non-binary coding]	n/a	R/E	No	No
RPS target	[Non-binary coding]	n/a	R/E	No	No

Table A1: The climate policies collected by Bergquist and Warshaw (2023).

Note: The instrument types are adopted from Linsenmeier et al. (2022). “Incentives” is short for the instrument type “Grants, subsidies, and other financial incentives”. The text in square brackets explains why policies were not included in the analysis: Low-income energy efficiency programs were adopted simultaneously in 39 states, so that a diffusion analysis is impossible. State Environmental Policy Acts were passed between 1970 and 1990, thus preceding the time of our analysis. State preemption of local gas bans were adopted only four times, so it has too few observations to be included. The last three policies are excluded, because they have non-binary coding. The last two columns indicate which policies have been included in the diffusion analysis and the analysis on the effects of climate policies.

State	Share coal	Share natural gas	Dirtiest source	metric tons/mWh	metric tons/MW
Alabama	13.70%	43.70%	Coal	1.04	2204
Alaska (1)	10.80%	52.40%	Coal	1.04	548.7
Arizona	10.80%	47.40%	Coal	1.04	2438
Arkansas	27.40%	39.60%	Coal	1.04	2330.3
California	0.10%	43.60%	Natural gas	0.44	965.6
Colorado	32.90%	30.00%	Coal	1.04	1922.1
Connecticut	0.00%	61.30%	Natural gas	0.44	665.3
Delaware	-0.10%	91.30%	Natural gas	0.44	314.6
District Of Columbia	0.00%	55.10%	Natural gas	0.44	523.7
Florida	4.50%	75.80%	Coal	1.04	1974.6
Georgia	12.60%	46.90%	Coal	1.04	1990.7
Hawaii	0.00%	0.00%	Petroleum	1.09	1845.3
Idaho	0.00%	32.10%	Natural gas	0.44	648.4
Illinois	15.30%	15.90%	Coal	1.04	1796.9
Indiana	45.00%	38.70%	Coal	1.04	1791.7
Iowa	23.40%	14.90%	Coal	1.04	1538.5
Kansas	27.50%	8.40%	Coal	1.04	2128
Kentucky	68.40%	23.10%	Coal	1.04	602.7
Louisiana	5.00%	76.00%	Coal	1.04	1643.4
Maine	0.40%	29.40%	Natural gas	0.44	638.9
Maryland	4.70%	42.50%	Coal	1.04	1430
Massachusetts	0.00%	76.10%	Natural gas	0.44	669.8
Michigan	19.20%	45.70%	Coal	1.04	1265.1
Minnesota	22.10%	24.40%	Coal	1.04	1720
Mississippi	5.50%	75.90%	Coal	1.04	1647.7
Missouri	60.10%	13.50%	Coal	1.04	1745.5
Montana	44.80%	3.90%	Coal	1.04	1965
Nebraska	44.60%	5.80%	Coal	1.04	1852.5
Nevada	4.90%	58.10%	Coal	1.04	2724
New Hampshire	1.00%	25.70%	Coal	1.04	1739.8
New Jersey	0.00%	51.40%	Natural gas	0.44	626
New Mexico	19.10%	35.20%	Coal	1.04	2410.5
New York	0.00%	47.70%	Natural gas	0.44	569.8
North Carolina	11.00%	41.50%	Coal	1.04	1935.7
North Dakota (2)	55.20%	4.90%	Coal	1.04	1965
Ohio	23.80%	58.80%	Coal	1.04	1991.7
Oklahoma	5.80%	49.80%	Coal	1.04	1780.1
Oregon	0.00%	38.10%	Natural gas	0.44	900.5
Pennsylvania	5.40%	59.00%	Coal	1.04	1390.7
Rhode Island	0.00%	89.20%	Natural gas	0.44	615.8
South Carolina	14.90%	23.80%	Coal	1.04	1884.8
South Dakota	9.20%	13.90%	Coal	1.04	2087.8
Tennessee	19.90%	19.30%	Coal	1.04	1948.5
Texas	13.20%	51.10%	Coal	1.04	2059.9
Utah	47.20%	35.40%	Coal	1.04	2607.2
Vermont	0.00%	0.00%	Renewables	0	0
Virginia	1.50%	56.00%	Coal	1.04	1695.2
Washington	4.10%	17.60%	Coal	1.04	459.9
West Virginia (3)	85.60%	6.90%	Coal	1.04	1695.2
Wisconsin	32.60%	43.60%	Coal	1.04	1092.9
Wyoming	71.30%	5.10%	Coal	1.04	2110.5

Table A2: Solar effectiveness by state.

Note: The shares for coal and natural gas are 2022 data from the US Energy Information Administration. Solar effectiveness based on capacity (last column) is based on calculation of the capacity factor using solar capacity and solar electricity generation in the state. (1) Alaska didn't have utility-scale solar in 2022. We inserted the average capacity factor of Canada. Canada's capacity factor is 6% according to: https://www.eia.gov/outlooks/ieo/pdf/0484(2016).pdf (2) North Dakota didn't have utility-scale solar in 2022. We inserted the capacity factor of Montana. (3) West Virginia didn't have utility-scale solar in 2022. We inserted the capacity factor of Virginia.

Figure A1: The temporal pattern of adoption of climate policies in US states.

Variable	Source
State GDP	U.S. Department of Commerce, Bureau of Economic Analysis
Population	U.S. Census Bureau
GHG intensity	U.S. Energy Information Administration
Democratic control	Hillman (2023)
Employment in mining	U.S. Bureau of Labor Statistics
Employment in transport and utilities	U.S. Bureau of Labor Statistics
GDP of mining	U.S. Department of Commerce, Bureau of Economic Analysis
GDP of transport and utilities	U.S. Department of Commerce, Bureau of Economic AnalysisS
Population density	U.S. Census Bureau
log Population	U.S. Census Bureau
log Land area	U.S. Census Bureau
Wind power potential	U.S. Department of Energy (NREL)
Solar power potential	U.S. Department of Energy (NREL)
Coal production	U.S. Energy Information Administration
Share evangelical	Sellers (2017)
Concern about climate change	Bergquist and Warshaw (2019)

Table A3: The sources of all control variables used in this study.

B Model specification

We consider 15 different control variables to address concerns about omitted variable biases. A particular concern arises from the inclusion of the spatial lag of policy adoption. This is because in a Cox model a variable that is constructed from the history of past events is prone to biases from selection into treatment. To understand this specific risk of a bias, consider the example of a binary time invariant variable A that prevents the adoption of the policy in all states with $A = 1$ and has no effect on adoption if $A = 0$. Remember that in the Cox framework, a state-year observation is only included in the estimation of the hazard function if that state has not yet adopted the policy in that year. This means that in the sample of state-year observations, in later years states with $A = 1$ will be over-represented relative to states with $A = 0$. Furthermore, the remaining states will tend to have a higher value of the spatial lag than states that adopted the policy in earlier years because the spatial lag monotonically increases with the total number of policies adopted. Because of these two patterns, the variable A will be positively correlated with the spatial lag across state-year observations in the sample used for the Cox model. If the variable A is not included in the model, it will thus bias the estimated parameter of the spatial lag. And because the variable A suppresses policy adoption and is positively correlated with the spatial lag it will bias the parameter of the spatial lag downwards. For the same reason, any latent variable that has a positive effect on selection into treatment will be negatively correlated with the spatial lag, again biasing its coefficient downwards.

In the absence of exogeneous variation, the only way to address this bias from omitted variables is the inclusion of relevant variables. However, a trade-off arises between omitted variable biases and multicollinearity. For the reasons explained above, variables that affect selection into treatment will be correlated with the spatial lag. Including too many variables therefore leads to unreliable estimates of its coefficient.

We use techniques from statistical learning to navigate this tradeoff. We first examine the Variance Inflation Factor of a model that includes all 15 variables. Reassuringly, we find that it is below the critical threshold of 5 for all variables. This means that without the spatial lag a model with all 15 control variables could be reliably estimated. In the following we examine whether this insight extends to a model that also includes the spatial lag of policy adoption, taking into account that not all control variables may be equally important to be included in the model.

To do so, we next identify the most influential control variables to explain policy adoption. For this purpose, we estimate a logistical model using our panel data set with a recursive feature selection algorithm [2]. This algorithm first estimates the full model and then iteratively drops variables that have the smallest explanatory power.

We next calculate the correlation between each of the 15 variables and the spatial lag of policy adoption. Unfortunately, and as expected from the discussion above about the systematic correlation between the spatial lag and any variable that affects selection into treatment, the correlation is high for some variables and tends to be highest for the variables with the largest explanatory power of policy adoption, specifically for log GDP per capita. For this variable, the mean Pearson correlation coefficient with the spatial lag is larger than 0.8, supporting our initial concerns about multicollinearity.

To address the issue of multicollinearity, we next use Principal Component Analysis (PCA) as a dimensionality reduction technique. We apply PCA to our panel dataset of 15 control variables, resulting in 15 independent principal components. We next calculate the pairwise correlation between each of these components and the spatial lag. The results are promising: We find that the mean correlation coefficient across policies is now smaller than 0.6, remediating most concerns about multicollinearity.

These results suggest that the use of PCA addresses the concern of multicollinearity, but it remains to be checked whether a model with PCA instead of the original variables is equally effective in explaining policy adoption and thus controlling for potentially confounding variation related to selection into treatment. Furthermore, it is not clear how many principal components should be included in the model.

To shed some light on these two questions, we estimate again a logistic model for every policy and quantify the model fit using 10 fold crossvalidation. For this estimation, we once use our original dataset with the 15 original control variables, and once our new dataset with the 15 principal components. For both datasets we compare models with the most important 1, ..., 15 variables using the same recursive feature selection method as above to identify those variables.

Overall, we find that a model with six variables seems to have the largest predictive power (Figure B1). Models with fewer and models with more variables tend to have lower explanatory power. We find this result for both datasets, our original control variables and the principal components. Furthermore, and reassuringly, we find that the explanatory power of a model with the 6 most important control variables is almost identical to the explanatory power of a model with the 6 most important principal components. Given the advantage of the model with principal components regarding collinearity with the spatial lag, this model thus emerges as our preferred choice.

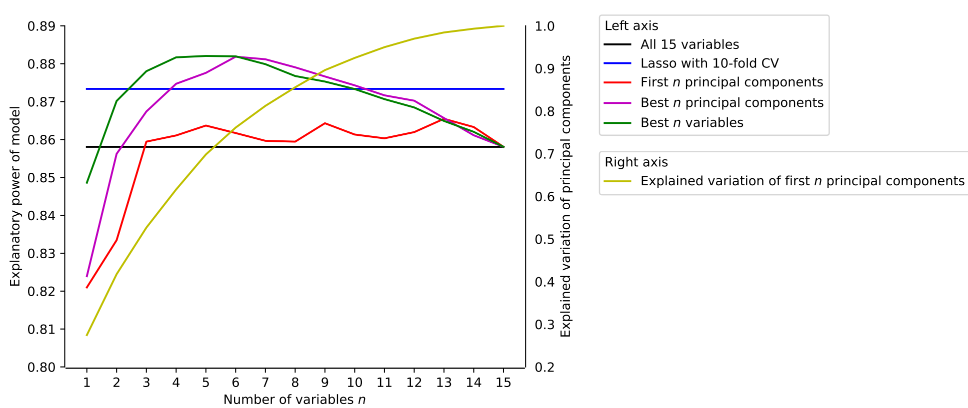

Figure B1: Explained variation of models with different explanatory variables. All results are obtained with logistical models of policy adoption and show the mean of 20 models, one for each climate policy.

[2]See https://scikit-learn.org/stable/modules/generated/sklearn.feature_selection.RFE.html.

To illustrate and quantify the bias from omitted variables, we estimate the Cox model with only the spatial lag using the placebo metric between states. This metric assigns a random weight to every state in every time period. Because of the random assignment the spatial lag should be irrelevant for policy adoption. The expected value of its coefficient is thus zero. However, we find that the coefficient tends to be negative across the climate policies in our sample (Figure B2 left). This aligns with our concerns about the effect of latent variables on selection into treatment. By contrast, for our preferred model, the mean coefficient becomes much closer to zero (Figure B2 right). This suggests that this model with six principal components can remediate at least a large portion of the concern about selection bias.

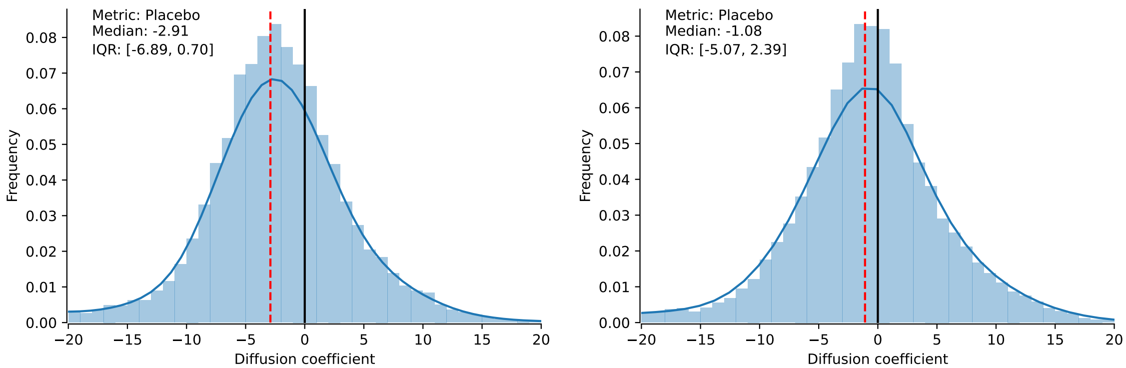

Figure B2: Estimated coefficients of spatial lag for model without control variables (left) and with six principal components as control variables (right). The distribution is based on one estimate for each of the 20 climate policies. To illustrate the coefficients as a distribution, we resample from standard distributions using the estimated mean and standard deviations. The spatial lag is constructed with a Placebo metric. The expected unbiased estimated coefficient is thus zero.

C Additional results

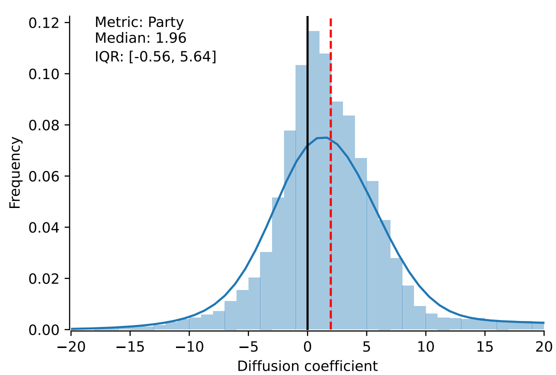

Figure C1: Estimated coefficients of spatial lag for model with six principal components. The distribution is based on one estimate for each of the 20 climate policies. To illustrate the coefficients as a distribution, we resample from standard distributions using the estimated mean and standard deviations. The spatial lag is constructed with a metric based on the political alignment of states.

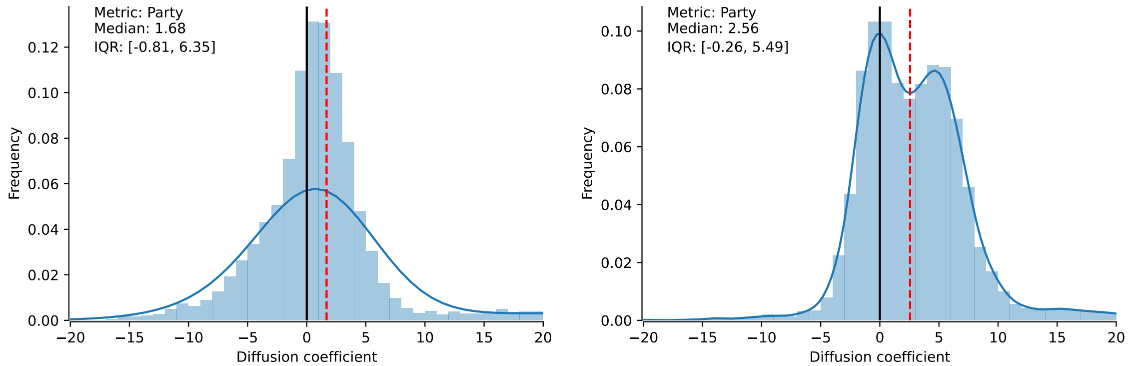

Figure C2: Estimated coefficients of spatial lag for model with six principal components. The same illustration as Figure C1, but for different subgroups of policies: renewable/energy policies (left), all other/sectoral policies (right).

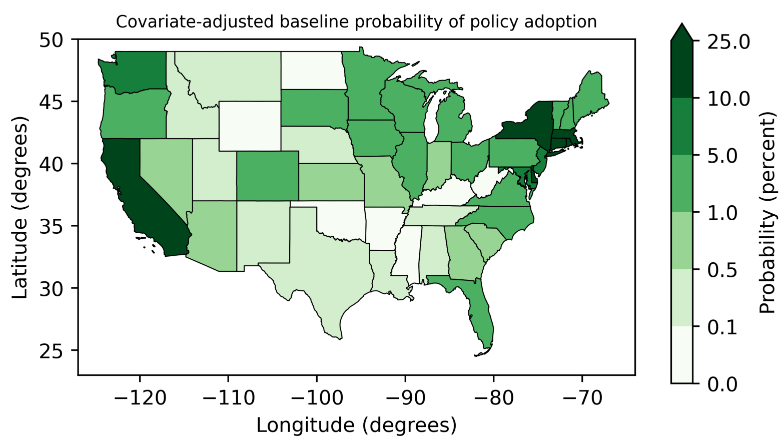

Figure C3: The map shows the predicted covariate-adjusted baseline hazard for a baseline hazard of 5 percent. The coefficients of all covariates can be found in Figure 2 right.

PUBLICATIONS

[Title of Working Paper: Subtitle (As Needed)]

Working Paper No. [WP/YYYY/###]

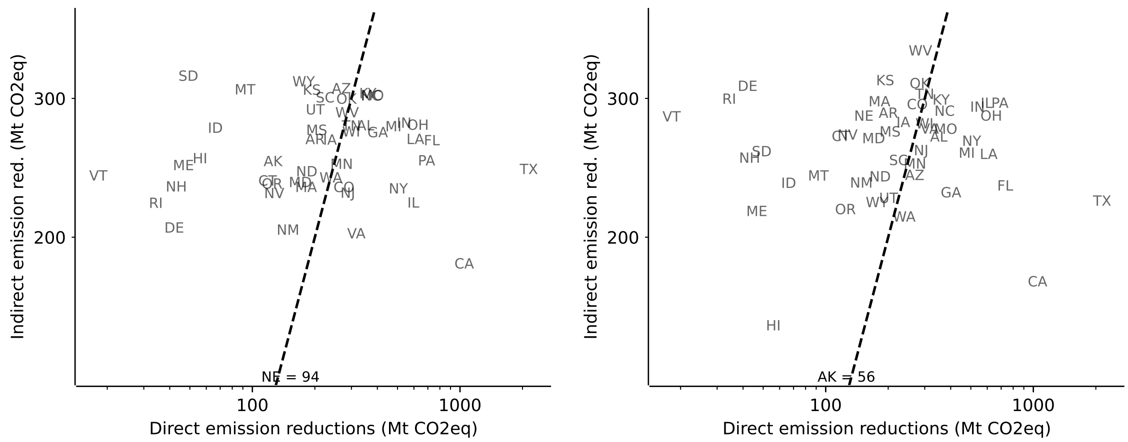

Figure C4: Direct and indirect emission reductions for each US state. Left: Proximity metric based on party. Right: Proximity metric based on physical distance. These simulations assume a baseline hazard of 5 percent and a reduction of GHG emissions of 1 percent per year after adoption of a policy.

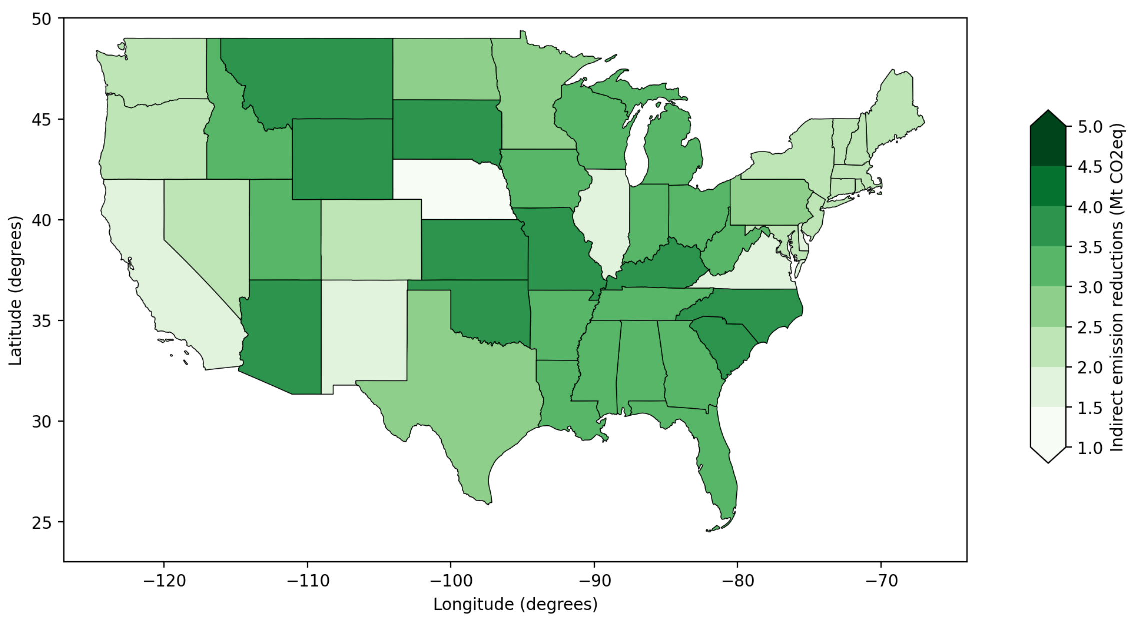

Figure C5: Indirect emission reductions for each US state. Proximity metric based on party. The simulations assume a baseline hazard of 5 percent. The reduction of GHG emissions is calculated using the empirical estimate of policy effectiveness. See Figure 4 (right) for the comparison between direct and indirect emission reductions.