Energy Policy 178 (2023) 113575

Contents lists available at ScienceDirect

Energy Policy

journal homepage: www.elsevier.com/locate/enpol

Are climate change policies politically costly?

Davide Furceri [a], Michael Ganslmeier [b,*], Jonathan Ostry [c]

[a] International Monetary Fund, USA
[b] Department of Methodology, London School of Economics and Political Science, UK
[c] Department of Economics, Georgetown University, USA

ARTICLE INFO

JEL classification:
D72
J65
L43
L51
O43
O47
P16

Keywords:
Climate change policies
Climate change
Political support
Political cost

ABSTRACT

Are policies designed to avert climate change (Climate Change Policies, or CCPs) politically costly? Using data on governmental popular support and the OECD's Environmental Stringency Index covering 30 countries between 2001 and 2015, our results show that CCPs are not necessarily politically costly: policy design matters. First, in contrast to non-market-based CCPs (such as emission limits), only market-based CCPs (such as emission taxes) entail political costs for the government. Second, the effects are only present when CCPs are adopted during periods of high oil prices, prior to elections, or in countries depending strongly on non-green (dirty) energy sources. Third, CCPs are only politically costly when inequality is high and/or social insurance/transfer does not sufficiently address the regressivity of CCPs. Our results are robust to numerous robustness checks including to address concerns related to endogeneity issues.

1. Introduction

There are few issues that have sparked more attention in recent years than how to avoid the environmental and human catastrophe that climate change is inflicting on our planet. Yet, despite the rising demand for greater governmental efforts to reduce carbon emission, the hesitancy of politicians to adopt the necessary measures is remarkable. This is disturbing from two perspectives.

From an *economic* perspective, the empirical evidence on the economics of climate change shows that the long-term costs of unmitigated climate change will outweigh by a wide margin the short-term adjustment costs from mitigation (Stern, 2008). Although economists expect that poor countries will carry larger costs (Diffenbaugh and Burke, 2019; Dell et al., 2012), industrial countries are also estimated to suffer losses of ½-2 percent of GDP if global temperatures rise by 2–4 °C by 2100 (Hsiang et al., 2017). Kahn et al. (2019) project an even greater economic damage showing that unmitigated climate change will reduce global real GDP per capita by more than 7 percent by the end of the century. Importantly, since the economic costs are increasing in time because of greater accumulation of emissions in the atmosphere (Burke et al., 2015), hesitancy comes at a very high price as later interventions are required to be at a larger scale.

From a *political* perspective, the politics of climate change has also become more favourable to pro-environmental policy outcomes. For instance, the number of green parties has increased in many advanced democracies in the last decades (Dolezal, 2010). Since green parties have been challenging many non-green parties on climate-related topics, the pressure of mainstream electoral platforms to adopt green issues in their manifestos should facilitate the adoption of CCPs (Meguid, 2005; Adams et al., 2006; Spoon et al., 2014; Green-Pedersen and Mortensen, 2010; Ezrow et al., 2011). Moreover, with more frequent natural disasters, people are becoming more risk averse about climate change, and large-scale global protests have granted green issues a dominant position on political agendas around the world (Liu et al., 2011; Steves and Teytelboym, 2013; Hsiang and Marshall, 2014; Herrnstadt and Muehlegger, 2014; Bird, 2014; Welsch and Biermann, 2014; PEW Research Center, 2019; EMDAT, 2020).

However, despite these push factors, governments in democratic countries remain hesitant even when effective mitigation instruments are broadly available (Weitzman, 1974; McKibbin and Wilcoxen, 2002; Aldy et al., 2003; Li and Lin, 2013; Carl and Fedor, 2016; Akerlof et al., 2019). To understand the absence of welfare-increasing reforms, the

* Corresponding author.
E-mail address: m.g.ganslmeier@lse.ac.uk (M. Ganslmeier).

https://doi.org/10.1016/j.enpol.2023.113575
Received 13 September 2022; Received in revised form 30 March 2023; Accepted 3 April 2023
Available online 27 April 2023

debate about political costs has long dominated the political-economy literature (Alesina and Drazen, 1989; Drazen and Grilli, 1993; Persson and Tabellini, 2002; Tsebelis, 2002; Alesina et al., 2006; Alesina et al. 2020; Alesina et al. 2021). While previous contributions focused mostly on economic and structural reforms, the tie-in with climate policies has been investigated less.[1] With the present paper, we aim to fill this gap by analyzing whether the adoption of climate-related policies entails political costs for the incumbent government.

To do so, we estimate the average effect of CCPs on popular support for the government implementing them (governmental support for short)—where CCPs are proxied by the OECD's Environmental Policy Stringency (EPS) indicators (Botta and Koźluk, 2014) and governmental support is proxied by the "Index of Popular Support" produced by the International Country Risk Guide (ICRG) (2020) from public opinion polls for the period 2001–2015.[2] In addition, we adopt an Instrumental Variable (IV) approach that exploits cross-sectional variation in a country's likelihood to implement CCPs—such as measures of country vulnerability to climate change event as the length of coastline—and time-varying variation at the global level—such as the global number of floods per annum. Overall, our results show that endogeneity in the OLS estimator indeed is likely to result in underestimation of the true causal political cost of climate change policies.

Our paper delivers three key findings. First, the results show that CCPs reduce, on average, popular support for the government, but this mainly reflects the impact of market-based policy measures, such as emission taxes, rather than non-market-based measures, such as emission limits. As many economists see Pigouvian taxation as the first-best corrective tool for carbon emissions, opting for second-best nonmarket-based measures can be an efficient alternative when market-based measures are not politically viable (Jenkins, 2014; Goulder and Parry, 2008; Goulder et al., 2019; Stiglitz, 2019; IMF, 2019). Second, we show that political costs are larger when they are adopted in times of high fuel prices and/or prior to elections. These two findings indicate that political costs depend on the visibility of the reform and on the existing price level of affected products (e.g. fuel). In addition, the empirical analysis reveals that CCPs create a larger political backlash when adopted in countries with whose industries depend strongly on non-green (dirty) energy sources. Third, our findings indicate that the political costs are salient only when inequality is increasing at the time of CCP implementation and when social benefits—in the form of direct transfers to households, unemployment benefits to workers that loss their job or active labor market policies to help job reallocation—do not counterbalance the short-term cost in terms of output and employment (Känzig, 2021). Remarkably, in the counterfactual to these situations, the political cost of CCPs is not statistically different from zero. The latter finding highlights that climate-related policymaking is ultimately a social question, implying that sufficient social insurance mechanisms are imperative to enable the adoption of far-reaching but necessary action against climate change.

The paper is structured as follows. In the next section, we review the literature on CCPs, paying attention to the allocation of costs and benefits from stricter environmental regulation, the challenges of international agreements, and strategies on the mitigation of political costs. In section III and IV, we present the data, outline our empirical approach and discuss the results of the direct effects of CCPs. Section V presents the results on how political costs of CCPs vary across periods, countries, and institutions. Section VI concludes with implications for policymakers in advanced economies.

2. The political economy of climate change policies

There are parallels between the literature on the political economy of structural reforms and the political economy of environmental policies. Both types of reform are subject to resistance and a lack of political will even when they are improve welfare in the long run. In seminal contributions by Alesina and Drazen, 1989, Drazen and Grilli (1993), Persson and Tabellini, 2002 and Tsebellis (2002), the reform process is modelled as a function of veto players who are required to distribute the benefits and costs in a way that satisfies all of them.[3]

This approach applies also to environmental policies. On the one hand, economic adjustments from CCPs can be costly in the short run, while benefits materialize only over time. Additionally, the benefits of CCPs are not directly observable because they materialize in the absence of environmental damage. On the other hand, CCPs put a large burden of adjustment on a few stakeholders, while benefits are distributed widely (Stokes, 2016). Indeed, the regressive nature of certain CCPs has spurred concerns related to inequality in many countries around the world (Goulder et al., 2019; Stiglitz, 2019; Rojas-Vallejos and Amy, 2020; Känzig, 2021). Thus, the concentration of costs is expected to create strong interest groups against climate-protecting policies, while the dispersion of benefits diffuses support for pro-environmental positions.

Beyond these institutional considerations at the country level, environmental protection has always been hostage to the political agenda at the international level. Despite ambitious international agreements over the last decade, enforceable solutions are hampered by a dearth of sanctioning mechanisms to prevent free-riding behaviour. For instance, governments can be incentivized to undercut other countries' environmental standards to attract foreign direct investment from multinational corporations, which would ultimately induce regulatory race-to-the-bottom dynamics (Koch and Mama, 2019). In addition, climate change may also elevate international economic inequalities further as the costs of global warming vary widely across countries. As the Center for Global Development (2015) estimates, 79 percent of global carbon emissions from 1980 to 2011 have been generated by developed countries. However, these countries have not borne most of the cost of climate change today (Eckstein et al., 2019).

To address the distributional consequences of reforms (Ostry et al., 2019; Ostry, 2021; Markkanen and Anger-Kraavi, 2019), it is essential to understand how they affect different groups of society. CCPs can affect individuals in two ways. First, they often entail mark-ups on certain products —either directly through taxation or indirectly through rises in production costs (Rao, 2013). Although price increases for oil and gas prices matter to all economic agents, they tend to have larger effects on poorer households, for whom energy consumption constitutes a larger share of household income, and because of the hit to employment for less skilled workers (Känzig, 2021). This regressivity is a common argument against CCPs and a source of political opposition (Metcalf, 2009; Habla and Roeder, 2017; Goulder et al., 2019).

Second, CCPs have negative side effects for certain industries (Steves and Teytelboym, 2013; Frankhauser et al., 2008). Although new employment opportunities created by CCPs can mitigate job displacement to some degree (IMF, 2019; IMF, 2020), the adjustment costs are nevertheless salient in the short run. This is especially the case for the old, and for employees with specialized industry-specific capital or low educational levels (OECD, 2012; Guivarch et al., 2011). Since income

[1] For instance, Klenert et al. (2018) study how optimal environmental taxation can be designed to prevent regressive consequences, Klenert et al. (2017) show how the public support of carbon pricing policies can be enhanced via revenue recycling, and Sovacool et al. (2015) discuss the role distributional considerations and competing interests play for the success or failure of climate adaption projects.

[2] As a robustness check we include alternative measures to examine the political effects of CCPs, such as the annual averages of the polls of the incumbent party, the vote share of all government parties, and the vote share of the largest government party.

[3] Alesina et al. (2020) find that domestic (product and labor market) and external (trade and capital) structural reforms tend to reduce the vote share of the incumbent's party, especially when implemented close to elections.

losses and unemployment are well-known to have a strong negative impact on satisfaction with the government, this paper tests whether complementary measures—such as direct transfers to households, unemployment benefits, and active labor market policies—can mitigate the political backlash against CCPs.

3. Data

To estimate the political costs of CCPs, we use a country-year panel dataset covering 30 developed and emerging economies between 2001 and 2015. The large number of countries and years—our sample covers countries that are responsible for nearly half of global emissions—enables us to estimate the effects of CCPs on governmental support and test how the political costs vary across countries characteristics and over time. The comparative perspective of this paper enables us to investigate the nature of effective strategies to overcome political obstacles.

The empirical analysis uses data from different sources. For the main **dependent variable,** we use a measure of governmental popular support constructed by the International Country Risk Guide (ICRG, 2020). The measure assesses the government's ability to carry out its declared program(s), and its ability to stay in office. The popular support measure is based on opinion polls and scaled between 0 (very high political risk) and 4 (very low political risk). In other words, a value of 1 implies that the current government is at high risk of losing office, while a value of 4 implies that the incumbent is able to carry out its declared program with ease and has a very high likelihood of re-election. In our sample, popular support was highest in the USA in 2002 and lowest in France in 2014.

Compared to indicators that are based on parties' vote shares, the popular support index provides annual time series per country over a relatively long period of time. While poll data are also available, they usually do not provide a similar level of country coverage. The dataset by ICRG has been used to examine political stability and popular support in empirical studies both in economics and political science (Lambsdorff, 2003; Torgler and Schneider, 2007; Chan and Frey, 2019; Yabré and Semedo, 2021; Cotoc et al., 2021). Nevertheless, to ensure that this main dependent variable is a valid proxy for popular support, we also use alternative indicators for popular support, such as the vote share of all government parties, the vote share of the largest government party, and the annual averages of the polls of the incumbent party.

As main **independent variable,** we use the Environmental Policy Stringency (EPS) indicators constructed by the OECD (Botta and Koźluk, 2014). These data are the most comprehensive source for environmental policy measures across countries (28 OECD and 6 BRICS countries) and time (1990–2015). In addition to its large geographical and temporal coverage, the dataset provides a breakdown by instrument type. The dataset includes both market-based and non-market-based measures, such as indices of taxation of emissions, trading schemes and feed-in tariffs (market-based), as well as indices of emission limits and R&D subsidies (non-market-based). The scores (0–6) of each indicator represent a country's instrument-specific stringency in a given year in comparison to all other country-year observations for the time period and country sample under consideration. In detail, the OECD indices are based on instrument-specific raw indicators, i.e. tax rate for NO_x per MWh/tonne, which are used to assign values from 1 to 6 that are based on the in-sample distribution using all country-year for this raw measure. For instance, a country with a NO_x tax rate per MWh/tonne larger than 0.03 but smaller/equal than 0.5 receives a score of 2 for the NO_x-tax indicator for this particular year, while a country with a NO_x tax rate per MWh/tonne larger than 4.5 larger receives a score of 6 for this particular year (Botta and Koźluk, 2014). If a given instrument is not implemented in a country at a given point in time, the country receives a score of 0 for this instrument.

The availability of these sub-indices strikes us as central as it allows us to test whether some instruments are politically more costly than others. Previous research has shown that market-based policies such as emission taxes are the most effective instruments to reduce carbon emission (Jenkins, 2014; Goulder and Parry, 2008; Goulder et al., 2019; Stiglitz, 2019; IMF, 2019). However, since they may not always be politically feasible due to their distributional consequences (Känzig, 2021), opting for non-market-based measures can be suitable second-best solution. Table 1 presents an overview and summary statistics for all variables.

4. The political costs of CCP

4.1. Empirical analysis

The baseline analysis examines whether stricter CCPs have an effect on popular support for the government and, if so, whether the negative effects differ across instruments. To test this hypothesis, we regress the measure for popular support of the incumbent on the change in the overall EPS indicator. Here, we use the change of the policy index to test whether alteration of the current status-quo lowers popular support for the government (Samuelson and Zeckhauser, 1988; Fernandez and Rodrik, 1991; Pierson, 1994). Our OLS specification (baseline) reads as follows:

$$y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t} \tag{1}$$

where y is popular support for the incumbent and ΔEPS is the change in the EPS indicator. Beyond including country (α_i) and year fixed effects (γ_t) to control for unobserved factors and limit omitted variable bias, the specification includes various economic, political, and demographic indicators within X. Specifically, we control for the fiscal deficit as percent of GDP (OECD), the share of the elderly (65+) in percent of population (OECD), real GDP growth rate (World Bank), unemployment rate (World Bank), consumer price index (OECD), lagged government's vote share (Alesina et al., 2020), the average tax wedge (World Bank), and five economic reform indicators by Alesina et al. (2020).

The inclusion of these variables is based on different streams of the political economy literature, including related to economic voting, political capabilities, and structural reforms. For instance, by controlling for the tax wedge, we take account of the empirical finding that voters oppose taxes in general[4] (Alesina et al., 2021). Similarly, controlling for macroeconomic factors such as GDP growth, inflation, unemployment rate, and fiscal space, we take into account a large body of empirical work on economic voting which has shown that the state of the economy has a significant impact on the government's chances for re-election. Additionally, since older individuals may have different preferences towards climate change policies (Torgler and Garcia-Valiñas, 2007; De Vries et al., 2014), we include the share of the elderly as an additional control. Finally, previous empirical evidence has shown that reforms in other domains influence the popularity of the government. To account for policymaking in other domains, we add five structural reform indicators covering reform dynamics in policy areas related to finance, labour, and product markets.

Overall, the baseline results show that increasing environmental policy stringency has significantly negative and sizeable effects on the popular support for the government (Table 2). A major change in CCPs—equivalent to an increase in the EPS indicator from the 25th to the 75th percentile of the EPS distribution—is found to reduce popular support by about 10 percent. The statistical significance of the results is robust to alterative sets of controls and the magnitude of the coefficients does not change with model specification. In line with previous findings of the literature (see Alesina et al., 2020 and references therein), we also find that better economic conditions, higher fiscal deficits, and lower inflation are associated with higher political support. We find that the level of popular support is typically larger in countries with an older

[4] The results remain unchanged if we use the change in tax revenue as control variable instead of the levels.

Table 1
Description and summary statistics of variables.

Variables	Source	N	mean	max	min
index of popular support for the government parties	ICRG	465	2.223	3.792	0.500
annual average poll of incumbent party	(Jennings and Wlezien, 2018)	279	35.40	57.20	7.113
vote share of government parties	ICRG	712	44.80	97.58	0
vote share of largest government party	ICRG	656	35.27	94	0
index of environmental policy index (EPS)	OECD	730	1.634	4.133	0.292
market-based EPS index	OECD	730	1.093	3.983	0
non-market-based EPS index	OECD	755	2.138	5.500	0.500
tax-based EPS index	OECD	733	1.370	4	0
limit-based EPS index	OECD	755	2.307	6	0
taxation on CO2 EPS index	OECD	755	0.140	6	0
taxation on Diesel EPS index	OECD	733	3.701	6	0
taxation on Nox EPS index	OECD	755	0.723	6	0
taxation on SOx EPS index	OECD	755	0.874	6	0
emission limits on Diesel EPS index	OECD	755	3.160	6	0
emission limits on NOx EPS index	OECD	755	2.428	6	0
emission limits on PM EPS index	OECD	755	1.421	6	0
emission limits on SOx EPS index	OECD	755	2.220	6	0
aggregated feed-in-tariffs EPS index	OECD	755	1.258	6	0
feed-in-tariffs for solar EPS index	OECD	755	1.224	6	0
feed-in-tariffs for wind EPS index	OECD	755	1.293	6	0
aggregated trading scheme EPS index	OECD	752	0.588	5.200	0
trading scheme for CO2 EPS index	OECD	752	0.916	6	0
trading scheme for Energy Efficiency Certificate EPS index	OECD	755	0.159	6	0
trading scheme for Reneable Energy Certificate EPS index	OECD	755	0.473	6	0
R&D subsidies for renewable energy EPS index	OECD	755	1.968	6	0
aggregated R&D subsidies EPS index	OECD	755	1.968	6	0
general government deficit (as % of GDP)	OECD	610	−2.686	18.63	−32.06
share of the elderly (65+) (as % of total population)	OECD	806	13.51	26.65	3.458
growth rate of GDP at market prices (LCU)	WB	798	2.533	25.16	−13.13
unemployment, total (% of total labor force)	WB	775	8.379	33.47	1.777
consumer price index	OECD	747	0.877	2.616	0.0698
vote share of government parties	Alesina	643	−2.532	27.23	−29.86
average tax wedge of couple with 2 children	OECD	563	31.68	50.56	8.019
domestic financial liberalization indicator	Alesina	754	0.837	1	0.111
product market liberalization indicator	Alesina	754	0.543	1	0
current account reform indicator	Alesina	754	0.894	1	0.250
capital account reform indicator	Alesina	754	0.856	1	0.250
labor market liberalization indicator	Alesina	722	0.628	1	0.298
global price for brent crude	FRED	806	47.79	112.0	12.72
global price of natural gas	FRED	775	3.950	8.895	1.451
value-added weighted mean of input share of "mining and extraction of energy producing products" and "Coke and refined petroleum products" for other industries	OECD	465	0.0272	0.265	0.000427
share of pre-tax national income of top 1%	WID	519	0.113	0.296	0.0445
share of pre-tax national income of top 10%	WID	519	0.346	0.651	0.229
share of pre-tax national income of top 20%	WID	519	0.493	0.792	0.377
share of pre-tax national income of bottom 1%	WID	519	$-4.91*10^{-5}$	0.00180	−0.00570
share of pre-tax national income of bottom 10%	WID	519	0.0104	0.0265	−0.0195
share of pre-tax national income of bottom 20%	WID	519	0.0438	0.0784	0.0125
market-based Gini coefficient	OECD	705	46.70	69.40	28.23
net Gini coefficient	OECD	705	31.61	59.40	17.55
total public social expenditure as percentage of GDP	OECD	683	20.03	34.18	2.584
public social expenditure for ALPM as % of GDP	OECD	684	0.616	2.714	0.00400
public social expenditure for unemployment as % of GDP	OECD	655	1.056	4.643	0
years left in current term	ICRG	705	1.780	5	0
social benefits to households (in-cash)	OECD	627	12.74	23.12	0.0314
social benefits to households (in-kind)	OECD	585	11.08	19.39	4.240
index of globalization (total) (global average)	ICRG	806	53.80	61.75	43.63
index of total conflict (global average)	ICRG	775	808.9	2891	179.4
index of riot	ICRG	775	0.678	3.839	0.0323
(log) population	WEO	796	3.251	7.152	0.682
number of people affected by earthquakes (global)	WB	496	$6.103*10^{6}$	$4.672*10^{7}$	786,413
percentage of urban extent in coastal zone (km2) affected by sea level elevation	(CIESIN, 2013)	496	0.00731	0.0818	0
number of floods (global)	(EMDAT, 2020)	496	165.3	226	128
length of coastline	(World Research Institute, 2000)	496	28,511	265,523	0
frequency of major hurricanes in the North Atlantic	(NOAA, 2019)	496	3.188	7	0
distance from centroid of country to nearest coast (km)	(Portland State University, 2020)	496	250.2	1713	2.944
number of wildfires (global)	(EMDAT, 2020)	496	11.81	30	4
agricultural land (km2) per capita	(Nationmaster, 2007)	480	12.56	208.2	0.365

Table 2
The effect of EPS changes on popular support of the government.

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index	**−0.231*****	**−0.246*****	**−0.247*****	**−0.266*****	**−0.305*****	**−0.297*****
	(0.079)	**(0.079)**	**(0.080)**	**(0.086)**	**(0.091)**	**(0.092)**
public deficit %GDP	0.039***	0.028*	0.027*	0.025	0.016	0.015
	(0.014)	(0.015)	(0.015)	(0.017)	(0.014)	(0.015)
share of elderly	0.163***	0.177***	0.194***	0.172**	0.125	0.095
	(0.051)	(0.055)	(0.063)	(0.069)	(0.089)	(0.090)
GDP growth	0.063**	0.047	0.051	0.048	0.035	0.036
	(0.029)	(0.030)	(0.031)	(0.033)	(0.026)	(0.028)
unemployment rate		−0.035**	−0.033*	−0.028	−0.050**	−0.057**
		(0.017)	(0.018)	(0.022)	(0.020)	(0.025)
CPI			0.299	0.233	0.094	0.342
			(0.309)	(0.314)	(1.016)	(1.172)
gov. parties' vote share				0.010*	−0.003	−0.003
				(0.005)	(0.006)	(0.007)
average tax wedge					−0.071	−0.059
					(0.044)	(0.049)
financial reform index						0.806
						(1.929)
product reform index						−0.167
						(0.743)
current account reform index						−1.437
						(1.300)
capital account reform index						1.485
						(1.507)
labour reform index						0.854
						(1.732)
constant	0.659	0.724	0.281	0.628	3.050	1.868
	(0.624)	(0.673)	(0.924)	(1.010)	(1.910)	(2.846)
Observations	370	370	370	326	260	260
R-squared	0.455	0.466	0.468	0.464	0.548	0.554
Number of countries	30	30	30	26	23	23
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta\Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$. Columns differ with respect to control variable set. All estimations include country and year fixed effects. The main independent variable of interest is change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

share of the population as age tends to be positively associated with voting for the incumbent at the individual level (De Vries et al., 2014).

To test the robustness of these findings, we conduct three additional checks. First, we test whether our estimates are robust to alternative dependent variables. In particular, we re-estimate the baseline with three different indicators, namely the annual averages of the polls of the incumbent party (Table SI1), the vote share of all government parties (Table SI2), and the vote share of the largest government party (Table SI3). As the results show, the effect remains negative and significant for all dependent variables under consideration. Again, the magnitude of the impact is sizeable. For instance, increasing the EPS indicator by one-unit results in a 11.36% loss in vote share of all government parties (Table SI2).

Second, the baseline estimates may suffer from model uncertainty and misspecification. Here, one major source of model uncertainty originates from the covariate selection. While our baseline analysis shows that the inclusion or exclusion of a broad range of political, economic, and demographic control variables does not change the estimate of interest, we conduct two additional model averaging exercises using weighted-average least squares (WALS) (Magnus et al., 2010) and Bayesian model averaging (BMA) (Fernandez et al., 2001). As the results show (Table SI4), the change in the EPS index is one of the most robust variables among all covariates under consideration and the point estimates are very similar to the baseline results.

Third, it is possible that the political costs of CCPs have been declining over time as more frequent natural disasters, rising risk aversion about the consequences of climate change, and large-scale global protests have made the median voter greener over time. To test this argument, we re-estimate the baseline model by dropping observations after year k with $k = \{2006, 2008, 2010, 2012, 2014\}$. We use this sub-sampling approach instead of a rolling-window because restricting the dataset to only a few years would lead to extensive sampling uncertainty, and thus large standard errors. The results show that the political costs of CCPs have indeed decreased over time. While the coefficient based on observations with $k = 2006$ is −0.377, the estimate drops to −0.225 when including all observations up to 2014. In other words, even though both estimates are highly significant, the results indicate that the political costs of CCPs have declined over time (Table SI5). This result is consistent with the evidence that the support for green issues has been increasing in the recent decade (Spoon et al., 2014).

4.2. Endogeneity

Beyond measurement error of the dependent variable and covariate selection, the OLS estimates do not enable us to draw a causal interpretation due to two main forms of endogeneity. The first is omitted variable bias (OVB). While our model accounts for unobserved cross-country and -period heterogeneity and controls for numerous determinants of government support, we cannot exclude the possibility of OVB. A second issue is reverse causality, where the bias can go in either direction. On the one hand, governments might require political capital—proxied by popular support—to implement unpopular reforms. This selection bias implies a positive effect of the dependent variable on our policy variable and biases the OLS estimate towards zero. On the other hand, a government might implement CCPs because its unpopularity implies it has little to lose from implementing reforms. This would imply a negative effect of popular support on CCPs and thus the possibility that the OLS estimate could overestimate the true effect. In addition, despite the robustness of our results to alternative indicators of popular support, we cannot exclude measurement error. This is especially true when using policy indices. If the measurement error is uncorrelated with the error term, we lose precision. If there is correlation, attenuation bias leads to distorted coefficient estimates.

To address this issue, we adopt an IV approach. Following Nunn and Qian (2014), the approach consists of interacting a time-varying global term with a constant country-specific term. The global term we consider approximates the "policy pressure" that climate change induces at the supra-national level. To be more specific, we use indicators on the occurrence and consequences of extreme weather events each year such as the number of flood events, the number of people affected by earthquakes, the number of major hurricanes in the North Atlantic, and the number of wildfires around the globe per annum. This choice of instrument is consistent with previous evidence that preferences toward CCPs changes after major natural disasters (Bird, 2014; Welsch and Biermann, 2014; Latré et al., 2017). The country term we consider captures the vulnerability towards climate change which makes the adoption of CCPs more likely. For this purpose, geographical characteristics seem suitable measures since they can reasonably be assumed to be randomly distributed across countries and thus should not drive government support. We consider country-specific measures such as the length of the coastline, the minimum distance of a country's centroid to the coast, the share of urban population in coastal area, and agricultural land (in km[2]) per capita.

The theoretical rationale for our pressure-vulnerability instrument is based on the "war of attrition" model proposed by Alesina and Drazen, 1989. According to this model, reforms to correct unstainable long-term trends (such as persistent increases in debt or in emissions in our case) are often delayed when they have distributional implications. Reforms occur only when a given group concedes and is "forced" to bear the adjustment. In this model, a crisis (natural disaster) may induce reform "*because the relative cost of fighting the war tilts in favor of concession*" (Alesina et al., 2006, p.5). While both the global and country term can be assumed to be exogenous to popular support for the government, recent research has shown that one term being exogenous is enough for Bartik-like instruments to be valid (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022).

Overall, we have constructed five instruments consisting of interactions between a time-varying global term and a constant country term.[5] Our IV estimation reads as follows:

$$y_{i,t} = \beta\Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$$

$$\text{with } \Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}. \quad (3)$$

where S is the instrument. The analysis also controls for country and time fixed effects and can therefore be seen as a *difference-in-difference* approach. For all four instruments, the first stage estimates suggest that the instrument is "strong" and statistically significant. The Kleibergen–Paap rk Wald F statistic—which is equivalent to the F-effective statistic for non-homoskedastic error in case of one endogenous variable and one instrument (Andrews et al., 2019)—is higher than the associated Stock-Yogo critical value.

[5] The instruments are the interaction between: (i) the global number of flood events and the length of a country's coastline; (ii) the global number of people affected by earthquakes and the (log) share of urban extent in coastal zone affected by sea level elevation; (iii) the frequency of major hurricanes in the North Atlantic and distance from the centroid of a country to the nearest coast; and (iv) the global number of wildfires and a country's agricultural land per capita.

Our IV results support the findings obtained with OLS (Table 3): the IV estimates indicate a significant negative effect of EPS changes for all four instruments with similar-sized coefficients. However, the magnitude of the IV coefficient estimates is (more than) three times larger than the OLS estimate, which suggests that OLS estimates are biased towards zero. This is an informative outcome given that the direction of bias is ambiguous *ex-ante*.

To test the validity of our instruments, we run several checks. First, we test whether the instruments have a direct effect on popular support by including them stepwise as additional controls in the baseline model. If the coefficients turn out to be significant, one can argue that the instruments are part of the error term and thus do not satisfy the exclusion restriction. As the results show (Table SI6), this is not the case, since all five instruments turn out to be insignificant. Second, instead of regressing popular support on the instruments, we also directly test the association of the baseline residuals and the instrument. Again, the relationship is indistinguishable from zero (Table SI7), which supports the validity of our instruments.

Third, it might well be that our global term is associated with other global factors that affect political support. For instance, climate change pressure might be associated with global trends in political conflict or aversion towards globalization. These factors could affect popular support via country-specific factors spuriously related to countries' vulnerability to climate change. To test for this concern, we augment the baseline IV specification by including the interaction between length of the coastline with global factors such as: the overall globalization index from the KOF dataset (see Gygli et al., 2019), which is an aggregate of economic, social and political dimensions of globalization; the global average of the weighted conflict index from the ICRG dataset; and the global average of the riot index from the ICRG dataset (Table SI8).

Fourth, it is possible that our country specific term is associated with country characteristics (such as country size) that may affect political support through factors spuriously related to global pressure for climate

Table 3
The effect of EPS changes on popular support of the government using instrumental variable regressions.

Variables	(1)	(2)	(3)	(4)
change in EPS index	**−0.994*** (0.386)**	**−1.047** (0.533)**	**−0.635* (0.341)**	**−0.879** (0.377)**
Observations	370	370	370	361
Instrument: product of				
- global term (varying)	# of floods	# of people affected by earthquake	# of major hurricanes	# of wild fires
- country term (constant)	coast length	share of urban extent in coastal zone	distance from centroid to nearest coast	agricultural land per capita
1st-stage coef.	0.003*** (0.001)	0.016*** (0.004)	−7.552*** (1.165)	9.580*** (2.542)
1st-stage F-Stat	30.79	14.98	42.05	14.21
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta\Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the instrumental variable being used. All estimations include country and year fixed effects. The main independent variable of interest is the change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). Control variables have been partialled out. A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

change (such as globalization). For example, globalization may be more relevant for smaller countries and the popular support for globalization policies for smaller economies may be different than for larger economies. To address this concern, we further control for the interaction between the number of flood events and country size and the interaction of the globalization index and country size. The results are similar to, and not statistically different from, the baseline IV results (Table SI8).

4.3. Instrument types

Beyond the average effect of CCPs on popular support for the government, we test whether the political costs vary across instrument types. This is important as the literature portrays taxes on emissions as the most effective tool to reduce global greenhouse gases (Jenkins, 2014; Goulder and Parry, 2008; Goulder et al., 2019; Stiglitz, 2019; IMF, 2019). At the same time, carbon taxes play only a limited role in national environmental legislation (Beiser-McGrath and Bernauer, 2019; Carattini et al., 2018). Thus, we test whether governments rationally hesitate to adopt market-based measures to avoid political costs. To do so, we re-estimate the baseline specification for each sub-component of the EPS indicator to test whether increasing environmental policy stringency via market-based instruments (i.e., taxes on emission) has a larger negative effect on popular support than using non-market-based instruments (i.e., emission limits).

The results suggest important differences between market-based and nonmarket-based instruments (Table 4). Market-based measures, and especially taxes on emissions, are broadly consistent with the baseline results pointing to negative consequences on governmental support. In contrast, non-market-based measures are typically not statistically significant. One interpretation is that price mark-ups are more visible to consumers than supply limits. The fact that households are constantly confronted by fuel prices—for instance through household energy bills or at the gasoline station—makes it easy for them to trace the price mark-up back to tax rises. In contrast, non-market measures either do not spark nearly the same level of public attention, or limits translate into price changes only with a lag. Thus, using non-market-based measures—which overall are still efficient ways to reduce carbon emissions (IMF 2019)—seem to stand a reasonable chance of escaping political blame (Weaver, 1986; Pierson, 1994) and thus of overcoming the political cost of CCPs. An exception is the EPS on diesel. While diesel tax changes do not have a significant effect on popular support, nonmarket-based measures on diesel do (Table SI9).

Table 4
The effect of EPS changes on popular support of the government, market-based vs. non market-based EPS.

Variables	(1)	(2)	(3)	(4)
change in market-based EPS index	−0.153**			
	(0.074)			
change in tax-based EPS index		−0.199**		
		(0.092)		
change in non-market-based EPS index			−0.066	
			(0.048)	
change in limit-based EPS index				−0.004
				(0.065)
public deficit %GDP	0.038***	0.039***	0.039***	0.039***
	(0.013)	(0.013)	(0.014)	(0.013)
share of elderly	0.166***	0.161***	0.149***	0.149***
	(0.053)	(0.051)	(0.050)	(0.051)
GDP growth	0.064**	0.058**	0.061**	0.061**
	(0.028)	(0.027)	(0.027)	(0.027)
Constant	0.621	0.684	0.835	0.819
	(0.647)	(0.628)	(0.600)	(0.611)
Observations	370	373	378	378
R-squared	0.452	0.445	0.444	0.441
Number of countries	30	30	30	30
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$. Columns differ with respect to the choice of the sub-index of EPS (each ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

5. Heterogeneity of political costs of CCP

5.1. Empirical analysis

Beyond the direct effects of CCPs, we test five factors that might mitigate their political costs. First, global energy prices (Federal Reserve Bank of St, 2020a; Federal Reserve Bank of St, 2020b) might play an essential role in how citizens perceive stricter climate change legislation. This is because environmental policy measures create economic costs through price or quantity changes which consumers have to absorb. Second, the political consequences of CCPs might be larger when their adoption is more visible to voters. Thus, we test whether adopting CCPs is politically more costly towards the end of the legislative term when governmental decisions are more in the public eye during campaign periods. Third, because CCPs impact especially emission-intensive sectors (Frankhauser et al., 2008), we expect those countries with a large input share of non-green (dirty), a heavy presence of such sectors may generate more political headwinds. Here, we proxy sectoral vulnerability to CCPs by the value-added share of dirty-energy mining industries (OECD, 2018). Fourth, the political costs of CCPs may also depend on the initial status-quo of environmental protection compared to other countries. To test for these potential non-linearities, we use the value of the EPS index for each country to examine whether governments with strict environmental standards in place experience larger political costs when adopting CCPs. Fifth, we look at whether CCPs are politically more costly at times when income inequality is rising and in the absence of social insurance mechanisms. To proxy the shape of the income distribution, we use (market- and net-based) Gini coefficient as well as different income shares (OECD, 2017; WID, 2020). To investigate the mediating role of social benefits, we use indicators capturing social benefits to households (% GDP) as well as social expenditure on active labor market policies and unemployment benefits (% GDP) based on data from Adema (2011).

To test these hypotheses, we depart from the following specification:

$$y_{i,t} = F(z_{i,t}) * \left[\beta^L \Delta EPS_{i,t} + \theta^L X_{i,t}\right] + (1 - F(z_{i,t})) * \left[\beta^H \Delta EPS_{i,t} + \theta^H X_{i,t}\right] + \alpha_i + \gamma_t + \varepsilon_{i,t}$$

$$\text{with } F(z_{i,t}) = \frac{e^{-1.5 * z_{i,t}}}{1 + e^{-1.5 * z_{i,t}}} \tag{2}$$

where z is the z-score (normalized to have zero mean and unit variance) of the following variables (M): (i) oil and gas price; (ii) value added share of dirty-energy mining industries (mining and oil); (iii) income shares and inequality measures; and (iv) social expenditure in percent of GDP. $z_{i,t} = \left(\frac{\overline{M_{it}} - \overline{M_i}}{\sigma_{Mi}}\right)$.[6] $F(z_{it})$ is the smooth transition function of the variable z. The coefficients β^L and β^H capture the impact of changes in EPS in cases of low M ($F(z_{it}) \approx 1$ when z goes to minus infinity) and high

[6] Thus, we look at within-country variation over time. Since the importance of the dirty energy mining industry and the initial levels of EPS is influenced by CCPs, we address this endogeneity issue by looking at *cross-country* variation—defining $z_{i,t} = \left(\frac{M_{i_{first}} - \overline{M}}{\sigma_M}\right)$. In other words, we use the first value that is available in our dataset.

M ($(1-F(z_{it})\approx 1$ when z goes to plus infinity), respectively.[7]

The use of the smooth transition function is equivalent to the smooth transition autoregressive (STAR) model developed by Granger and Terasvirta (1993) to assess non-linear effects above/below a given threshold or regime. The main advantage of this approach relative to estimating SVARs for each regime is that it uses a larger number of observations to compute the effects, improving the stability and precision of the estimates. In addition, this estimation strategy can handle the potential correlation of the standard errors within countries more easily by clustering at the country level.

5.2. Period characteristics

Starting with the results for fuel prices, we find that the effect of CCPs on governmental support depends on gasoline and oil market conditions, consistent with both sources of energy being the main sources of household energy consumption in OECD countries (Eurostat, 2020). In times of high oil and gasoline prices, the political damage from CCPs is statistically significantly negative—with coefficients 1.5 to 2 times as large as the direct effect in our baseline (Table 5). In contrast, the effect is not statistically different from zero when EPS rises in times of low global fuel prices. This implies that timing plays a substantial role from a political standpoint: governments can mitigate the political costs to a large extend by passing new legislation when fuel market conditions are favourable. This said, our estimation strategy only allows us to draw conclusions about fuel prices in the year of CCP implementation. Thus, it could be that governments are penalized in subsequent years should fuel prices rise again.

Overall, one can argue that adopting new measures when fuel prices are low gives an offsetting effect from more stringent environmental regulation on domestic budget constraints. In addition, there is a well-established literature on an inverted U-shaped relation between incomes and support for CCPs—a kind of environmental Kuznets curve (Dinda, 2004): environmental damage increases initially with rising levels of income per capita, but then diminishes. There is also micro evidence that support for CCPs rises with incomes, and so a greater willingness-to-pay for green policies (Carlsson and Johansson-Stenman, 2000; Franzen, 2003; Kotchen et al., 2013; see Torgler and Schneider, 2007). So, it is plausible that relaxing budget constraints through real income gains in times of low fuel prices could increase support for CCPs among voters.

Beyond contemporaneous fuel prices, our estimates indicate that political costs are only present when they are adopted towards the end of the legislative term (Table 5). In other words, governments can mitigate some of the political backlash when they adopt CCPs directly after elections. This finding is in line with the economic voting literature on voters' recency bias indicating that individuals base their voting decision more strongly on current policy outcomes prior to elections, while disregarding earlier legislative changes at the beginning of the term (see Alesina et al., 2021). In addition to electoral timing, we also test whether the partisanship of the government alters the political costs of CCPs. Indeed, the results show that left-wing and centre-governments are punished more strongly for increasing environmental policy stringency, while the coefficient is not significant for right-wing incumbents (Table SI10). These ideological differences are in line with the related literature on welfare state retrenchment showing that the governments are punished to different extents along partisan lines (Horn, 2021): as workers in non-green (dirty) energy-dependent sectors are traditionally

[7] $F(z_{it}) = 0.5$ is the cut-off between the weak and strong regimes. The approach is similar to considering a dummy variable that takes value 1 when the variable is about the country-specific mean—that is, $F(z_{it}) >=0.5$, and zero otherwise. The difference is that instead of considering two discrete values (0 and 1), the smooth transition approaches allow the regimes to continuously vary between 0 and 1.

Table 5
The effect of EPS changes on popular support of the government mediated by fuel prices, electoral timing, dirty energy dependence, and initial level of EPS.

Variables	(1)	(2)	(3)	(4)	(5)
change in EPS index x low oil price	−0.029 (0.272)				
change in EPS index x high oil price	−0.320** (0.129)				
change in EPS index x low gas price		0.222 (0.301)			
change in EPS index x high gas price		−0.493*** (0.170)			
change in EPS index x low # of years until election			−0.322** (0.153)		
change in EPS index x high # of years until election			−0.204 (0.136)		
change in EPS index x low share of dirty energy				−0.200 (0.119)	
change in EPS index x high share of dirty energy				−0.264** (0.119)	
change in EPS index xlow initial EPS level					−0.145 (0.113)
change in EPS index x high initial EPS level					−0.321*** (0.076)
public deficit %GDP	0.039*** (0.014)	0.040*** (0.013)	0.037*** (0.014)	0.040*** (0.009)	0.039*** (0.009)
share of elderly	0.165*** (0.052)	0.165*** (0.050)	0.111 (0.095)	0.163*** (0.054)	0.160*** (0.051)
GDP growth	0.063** (0.028)	0.061** (0.028)	0.055** (0.026)	0.063*** (0.019)	0.065*** (0.019)
Constant	0.634 (0.633)	0.628 (0.614)	1.343 (1.170)	−0.681 (0.896)	0.000 (0.000)
Observations	370	370	329	370	366
R-squared	0.456	0.460	0.466	0.337	0.338
Number of countries	30	30	30	30	29
Year Fixed Effects	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES
p-value of coefficient equality	0.428	0.105	0.604	0.751	0.234

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta\Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

tied to left-wing parties, the political costs are particularly large when they adopt stricter environmental measures.

5.3. Country characteristics

With respect to industrial composition, our results show that in countries with a large share of dirty-energy sectors such as Norway and Indonesia, more stringent CCPs garner larger political costs (Table 5). In contrast, the effect is not significant for countries where dirty-energy sectors have a very low weight in national input shares such as in Slovenia and Ireland. As employees in dirty-energy sectors are adversely affected by more stringent EPS, governments in countries with large employment shares in such industries are expected to experience stronger resistance against CCPs among the electorate. While CCPs would also create new employment opportunities, transition costs could be substantial, and matching skills in declining sectors to new job opportunities difficult.

In addition to industrial composition, we also find that political costs are higher in countries which started from stricter levels of environmental legislation in the early 2000s (Table 5). This finding points towards potential non-linear effects from a cross-sectional perspective: voters from environmental-friendly countries penalize the government for stricter CCPs when they feel that their economy is losing out against others which do not undertake necessary action against global warming. This finding highlights that international coordination of climate-related policymaking facilitates the adoption of necessary measures also at the national level. Finally, we test whether left-wing parties – which have historically a strong commitment to industrial workers (Kono, 2020) – face greater political costs when adopting stricter CCPs. Indeed, our results provide evidence in favor of this hypothesis (Table SI10).

5.4. Inequality and social insurance

Our results show that CCPs decrease the popular support for the government when income inequality is increasing, while stricter environmental legislation has no effect when inequality is declining (Tables 6 and 7). This finding is robust to various measures of income inequality, including Gini coefficients and income shares. The only exception concerns the bottom 1%-income share, for which we do not find significant effects. However, overall, the results are in line with our hypothesis: since stricter EPS impacts prices of basic goods which by their nature are not easily substitutable and have a low-price elasticity of demand (Eitches and Crain, 2016), a price jump for such products affects particularly households at the lower end of the income distribution (Rao, 2013).[8]

To test whether the provision of social insurance can counteract potential concerns related to economic hardship among adversely affected groups, we examine the role of social benefits to households as well as social expenditure on ALMPs and unemployment benefits (Table 8). In line with our hypothesis, our results show that increasing social benefits to households on active labor market policies and unemployment benefits reduces (and in some cases eliminate) the political costs of CCPs.

5.5. Endogeneity

We extend the IV estimation to include the mediating factors described previously. The setup is similar to the regime-dependent IV approach of Ramey and Zubairy (2018):

Table 6
The effect of EPS changes on popular support of the government mediated by inequality (Gini coefficients).

Variables	(1)	(2)
change in EPS index x lower market-based GINI coefficient	−0.066	
	(0.279)	
change in EPS index x higher market-based GINI coefficient	−0.380**	
	(0.170)	
change in EPS index x lower net-based GINI coefficient		−0.006
		(0.264)
change in EPS index x higher net-based GINI coefficient		−0.402**
		(0.162)
public deficit %GDP	0.038***	0.039***
	(0.014)	(0.014)
share of elderly	0.099	0.103
	(0.094)	(0.093)
GDP growth	0.057**	0.057**
	(0.026)	(0.026)
Constant	1.487	1.445
	(1.149)	(1.139)
Observations	326	326
R-squared	0.470	0.470
Number of countries	30	30
Year Fixed Effects	YES	YES
Country Fixed Effects	YES	YES
p-value of coefficient equality	0.442	0.301

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta\Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. ALMP and UB refers to active labor market programs and unemployment benefits, respectively. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

$$y_{i,t} = \beta\Delta\widehat{EPS}^{D}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$$

$$\text{with } \Delta\widehat{EPS}^{D}_{i,t} = \vartheta\left[F^{D}(Z) * S_{i,t}\right] + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t} \tag{4}$$

while $D \in \{H, L\}$.

The IV results in the extended model support our previous OLS estimates. Adopting CCPs in times of high oil and gas prices (Table SI11), as well as closer to elections, raises political costs (Table SI12). Likewise, a higher dirty-energy input share raises political costs, which are three times larger compared to low-dirty-energy-input-share economies (Table SI12). We also find causal evidence that the political costs of CCPs are higher in countries which had already higher levels of environmental protection in the early 2000s (Table SI12). With respect to inequality, the IV estimates show that CCPs are larger when inequality is higher, and across the different inequality measures (Table SI13, SI14, and SI15). Finally, the findings on the mediating effect of social benefits to households as well as social expenditure on ALMPs and unemployment benefits are also in line with the baseline estimates (Table SI16 and SI.17).

6. Conclusion and policy implications

While our results confirm that CCPs on average may undercut the popularity of governments on average, we show that there are effective mitigating strategies that limit or even remove the detrimental political consequences.

First, the type of CCP matters. While market-based measures (i.e. emission taxes) lower government popularity substantially, the political cost is not significant for non-market-based instruments (e.g., emission limits). Since economists consider market-based instruments to be the

[8] This is in line with a range of empirical studies showing that preferences towards environmental policies varies across income groups (Carlsson and Johansson-Stenman, 2000; Franzen, 2003; Kotchen et al., 2013; see Torgler and Schneider, 2007).

Table 7
The effect of EPS changes on popular support of the government mediated by inequality (income shares).

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index x low top 1% income share	0.023					
	(0.192)					
change in EPS index x higher top 1% income share	−0.300*					
	(0.156)					
change in EPS index x lower top 10% income share		0.033				
		(0.182)				
change in EPS index x higher top 10% income share		−0.320**				
		(0.155)				
change in EPS index x lower top 20% income share			0.022			
			(0.151)			
change in EPS index x higher top 20% income share			−0.320**			
			(0.146)			
change in EPS index x lower bottom 1% income share				0.010		
				(0.264)		
change in EPS index x higher bottom 1% income share				−0.254		
				(0.314)		
change in EPS index x lower bottom 10% income share					−0.304*	
					(0.156)	
change in EPS index x higher bottom 10% income share					0.021	
					(0.210)	
change in EPS index x higher bottom 20% income share						−0.352**
						(0.150)
change in EPS index x higher bottom 20% income share						0.082
						(0.198)
public deficit %GDP	0.040***	0.040***	0.040***	0.034***	0.039***	0.040***
	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
share of elderly	0.142**	0.144**	0.145**	0.126**	0.143**	0.147**
	(0.058)	(0.059)	(0.059)	(0.059)	(0.058)	(0.058)
GDP growth	0.071**	0.071**	0.071**	0.108***	0.070**	0.070**
	(0.035)	(0.035)	(0.035)	(0.034)	(0.035)	(0.035)
Constant	0.278	0.252	0.227	0.442	0.269	0.208
	(0.907)	(0.912)	(0.918)	(0.928)	(0.912)	(0.907)
Observations	301	301	301	259	301	301
R-squared	0.513	0.513	0.513	0.524	0.513	0.514
Number of countries	24	24	24	21	24	24
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES
p-value of coefficient equality	0.297	0.235	0.176	0.633	0.331	0.164

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta \widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ with $\Delta \widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

most efficient, it is important to internalize that such measures might also be relatively costly politically. Yet, as non-market-based measures remain viable instruments to reduce carbon emissions (IMF, 2019), such second-best options are efficient alternatives to tackle global warming (Stiglitz, 2019).

Second, the consequences of tightening environmental regulation seem to be more visible when the changes adjust energy prices – especially energy and fuel prices for households. Thus, adopting CCPs when world energy prices are low can provide an effective avenue for overcoming political-economy challenges. We have also shown that adopting stricter environmental legislation prior to elections entails greater political costs. This finding suggests that visibility and salience of reforms – which is higher in campaign periods – condition the political costs of legislative changes.

Third, inequality concerns play a key role for the feasibility of CCPs since the economic burden from CCPs are concentrated among groups with weaker initial conditions and less resilience (Känzig, 2021). Our results show that when CCPs are adopted in times of increasing inequality, political costs are magnified. However, redistributive instruments targeted at the individuals experiencing higher economic insecurity are viable strategies to overcome the political fallout from CCPs and support governments to take necessary action without risking losing office.

While our empirical analysis is based on 30 advanced economies with democratic systems, our results can also speak to less developed countries with similar political institutions. In particular, our findings imply that that the political costs of, and the public resistance against, stricter climate change policies are high when countries do not provide sufficient social insurance that support the ones that are economically negatively affected by these policies in the short term. This is likely to be the case for less developed countries with more limited in size and less developed social insurance schemes.

Climate change will be on the global policy agenda for years to come. As with all policies that generate winners and losers, CCPs require political support to be viable. Rational governments will continue to hesitate and delay because the political damage is palpable. Overcoming this bad equilibrium of inaction is urgent. Our hope is that the evidence and strategies identified in this paper may provide some guidance on ways forward.

List of funding sources

This work was supported by the European Social Research Council [ESRC Grant number: ES/P000649/1].

CRediT authorship contribution statement

Davide Furceri: Conceptualization, Methodology, Formal analysis,

Table 8
The effect of EPS changes on popular support of the government mediated by social benefits to households as well as social expenditure on ALMP and unemployment benefits.

Variables	(1)	(2)	(3)	(4)
change in EPS index x lower in-cash social benefits	−0.463***			
	(0.164)			
change in EPS index x higher in-cash social benefits	0.006			
	(0.155)			
change in EPS index x lower in-kind social benefits		−0.512**		
		(0.247)		
change in EPS index x higher in-kind social benefits		−0.053		
		(0.146)		
change in EPS index x lower social expend. on ALMP			−0.483***	
			(0.158)	
change in EPS index x higher social expend. on ALMP			0.084	
			(0.153)	
change in EPS index x lower social expenditure on UB				−0.544***
				(0.170)
change in EPS index x higher social expenditure on UB				0.155
				(0.218)
public deficit %GDP	0.041***	0.039***	0.033**	0.034***
	(0.014)	(0.012)	(0.014)	(0.013)
share of elderly	0.165***	0.144***	0.156**	0.171**
	(0.051)	(0.048)	(0.062)	(0.070)
GDP growth	0.065**	0.061**	0.047**	0.050*
	(0.029)	(0.028)	(0.023)	(0.027)
Constant	0.650	0.939	0.832	0.666
	(0.612)	(0.576)	(0.743)	(0.827)
Observations	370	340	334	321
R-squared	0.459	0.470	0.462	0.498
Number of countries	30	28	27	26
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES
p-value of coefficient equality	0.0947	0.187	0.0405	0.0544

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta\Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of −0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. ALMP and UB refers to active labor market programs and unemployment benefits, respectively. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

Writing, Visualization. **Michael Ganslmeier:** Conceptualization, Methodology, Formal analysis, Visualization. **Jonathan Ostry:** Conceptualization, Methodology, Formal analysis, Visualization.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Michael Ganslmeier reports financial support was provided by University of Oxford.

Data availability

Data will be made available on request.

Acknowledgements

We thank Tarik Abou-Chadi, Jean Chateau, Mark Fransham, Ian Perry, Lukas Lehner, Orkun Saka, Gregor Schwerhoff, Tim Vlandas and Jeromin Zettelmeyer as well as participants and discussants from the IMF Surveillance Meeting and the DSPI Research Group Meeting at Oxford for their valuable comments and suggestions. We are grateful to Connor Kinsella for excellent editorial assistance.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.enpol.2023.113575.

References

Adams, James, Clark, Michael, Lawrence, Ezrow, Glasgow, Garrett, 2006. Are niche parties fundamentally different from mainstream parties? The causes and the electoral consequences of Western European parties' policy shifts, 1976–1998. Am. J. Polit. Sci. 50 (3), 513–529.
Adema, Willem, 2011. Pauline Fron, and Maxime Ladaique. "Is the European welfare state really more expensive?: indicators on social spending. In: And a Manual to the OECD Social Expenditure Database. SOCX, pp. 1980–2012.
Akerlof, George, Aumann, Robert, Deaton, A., Diamond, P., Engle, R., Fama, E., Hansen, L., Hart, O., Holmström, B., Kahneman, D., 2019. Economists' statement on carbon dividends. Wall St. J. A13.
Aldy, Joseph E., Scott, Barrett, Stavins, Robert N., 2003. Thirteen plus one: a comparison of global climate policy architectures. Clim. Pol. 3 (4), 373–397.
Alesina, Alberto, Furceri, Davide, Ostry, Jonathan D., Papageorgiou, Chris, Quinn, Dennis P., 2020. Structural Reforms and Elections: Evidence from a World-wide New Dataset. No. W26720. National Bureau of Economic Research.
Alesina, Alberto, Ardagna, Silvia, Trebbi, Francesco, 2006. Who adjusts and when? The political economy of reforms. IMF Staff Pap. 53 (1), 1–29.
Alesina, Alberto, Drazen, Allan, 1989. Why Are Stabilizations Delayed?. No. W3053. National Bureau of Economic Research.
Alesina, Alberto, Furceri, Davide, Ciminelli, Gabriele, Saponaro, Giorgio, 2021. Austerity and Elections. No. 2021/121. International Monetary Fund.
Andrews, Isaiah, Stock, James H., Sun, Liyang, 2019. Weak instruments in instrumental variables regression: theory and practice. Annual Review of Economics 11, 727–753.
Beiser-McGrath, Liam F., Bernauer, Thomas, 2019. Could revenue recycling make effective carbon taxation politically feasible? Sci. Adv. 5 (9), eaax3323.
Bird, Deanne K., 2014. Katharine Haynes, Rob van den Honert, John McAneney, and Wouter Poortinga. "Nuclear power in Australia: a comparative analysis of public opinion regarding climate change and the Fukushima disaster. Energy Pol. 65, 644–653.
Borusyak, Kirill, Hull, Peter, Jaravel, Xavier, 2022. Quasi-experimental shift-share research designs. Rev. Econ. Stud. 89 (1), 181–213.
Botta, Enrico, Koźluk, Tomasz, 2014. Measuring Environmental Policy Stringency in OECD Countries: A Composite Index Approach.
Burke, Marshall, Hsiang, Solomon M., Miguel, Edward, 2015. Global non-linear effect of temperature on economic production. Nature 527 (7577), 235–239.
Carattini, Stefano, Carvalho, Maria, Fankhauser, Sam, 2018. Overcoming public resistance to carbon taxes. Wiley Interdisciplinary Reviews: Clim. Change 9 (5), e531.
Carl, Jeremy, Fedor, David, 2016. Tracking global carbon revenues: a survey of carbon taxes versus cap-and-trade in the real world. Energy Pol. 96, 50–77.
Carlsson, Fredrik, Johansson-Stenman, Olof, 2000. Willingness to pay for improved air quality in Sweden. Appl. Econ. 32 (6), 661–669.
Center for Global Development, 2015. Developed countries are responsible for 79 percent of historical carbon emissions. Retrieved September 01, 2020, from. https://www.cgdev.org/media/who-caused-climate-change-historically.
Chan, Ho Fai, Frey, Bruno S., 2019. Ahmed skali, and benno torgler. In: Political Entrenchment and GDP Misreporting.
CIESIN, 2013. "Sea Level Rise Impacts on Ramsar Wetlands of International Importance". NASA Socioeconomic Data and Applications Center (SEDAC), Palisades, NY. http://sedac.ciesin.columbia.edu/data/set/lecz-slr-impacts-ramsar-wetlands.
Cotoc, Ionut, Johri, Alok, Sosa-Padilla, César, 2021. Sovereign Spreads and the Political Leaning of Nations. No. W29197. National Bureau of Economic Research.
De Vries, Catherine, E., Giger, Nathalie, 2014. Holding governments accountable? Individual heterogeneity in performance voting. Eur. J. Polit. Res. 53 (2), 345–362.
Dell, Melissa, Jones, Benjamin F., Olken, Benjamin A., 2012. Temperature shocks and economic growth: evidence from the last half century. Am. Econ. J. Macroecon. 4 (3), 66–95.
Diffenbaugh, Noah S., Burke, Marshall, 2019. Global warming has increased global economic inequality. Proc. Natl. Acad. Sci. USA 116 (20), 9808–9813.
Dinda, Soumyananda, 2004. Environmental Kuznets curve hypothesis: a survey. Ecol. Econ. 49 (4), 431–455.
Dolezal, Martin, 2010. Exploring the stabilization of a political force: the social and attitudinal basis of green parties in the age of globalization. W. Eur. Polit. 33 (3), 534–552.

Drazen, Allan, Grilli, Vittorio, 1993. The Benefit of Crisis for Reform. American Economic Review, pp. 588–608.
Eckstein, David, Künzel, Vera, Schäfer, Laura, Winges, Maik, 2019. Global climate risk index 2020. Germanwatch. https://germanwatch.org/sites/germanwatch.org/files/20-2-01e%20Global.
Eitches, Eliana, Crain, Vera, 2016. Using gasoline data to explain inelasticity. Beyond the Numbers: Prices and Spending 5 (5).
EMDAT, 2020. OFDA/CRED International Disaster Database. Université catholique de Louvain –, Brussels – Belgium.
Eurostat, 2020. Energy consumption and use by households. Retrieved September 01, 2020, from. https://ec.europa.eu/eurostat/web/products-eurostat-news/-/DDN-20170328-1.
Ezrow, Lawrence, De Vries, Catherine, Steenbergen, Marco, Edwards, Erica, 2011. Mean voter representation and partisan constituency representation: do parties respond to the mean voter position or to their supporters? Party Polit. 17 (3), 275–301.
FRED, 2020a. Louis and US. In: Global Price for Brent Crude [POILBREUSDA]. Retrieved September 01, 2020, from. https://fred.stlouisfed.org/series/POILBREUSDA.
Federal Reserve Bank of St, 2020b. Louis and US. "Global price of natural gas, US henry hub gas [PNGASUSUSDA]". Retrieved September 01, 2020, from. https://fred.stlouisfed.org/series/PNGASUSUSDA.
Fernandez, Carmen, Ley, Eduardo, Steel, Mark FJ., 2001. Model uncertainty in cross-country growth regressions. J. Appl. Econom. 16 (5), 563–576.
Fernandez, Raquel, Rodrik, Dani, 1991. Resistance to Reform: Status Quo Bias in the Presence of Individual-specific Uncertainty. The American economic review, pp. 1146–1155.
Frankhauser, Samuel, Sehlleier, Friedel, Stern, Nicholas, 2008. Climate change, innovation and jobs. Clim. Pol. 8 (4), 421–429.
Franzen, Axel, 2003. Environmental attitudes in international comparison: an analysis of the ISSP surveys 1993 and 2000. Soc. Sci. Q. 84 (2), 297–308.
Goldsmith-Pinkham, Paul, Isaac, Sorkin, Henry, Swift, 2020. Bartik instruments: what, when, why, and how. Am. Econ. Rev. 110 (8), 2586–2624.
Goulder, Lawrence H., Hafstead, Marc AC., Kim, GyuRim, Long, Xianling, 2019. Impacts of a carbon tax across US household income groups: what are the equity-efficiency trade-offs? J. Publ. Econ. 175, 44–64.
Goulder, Lawrence H., Parry, Ian, 2008. Instrument choice in environmental policy. Rev. Environ. Econ. Pol. 2 (2), 152–174.
Granger, Clive WJ., Terasvirta, Timo, 1993. Modelling Non-linear Economic Relationships. OUP Catalogue.
Green-Pedersen, Christoffer, Mortensen, Peter B., 2010. Who sets the agenda and who responds to it in the Danish parliament? A new model of issue competition and agenda-setting. Eur. J. Polit. Res. 49 (2), 257–281.
Guivarch, Céline, Renaud, Crassous, Olivier, Sassi, Hallegatte, Stéphane, 2011. The costs of climate policies in a second-best world with labour market imperfections. Clim. Pol. 11 (1), 768–788.
Gygli, Savina, Haelg, Florian, Potrafke, Niklas, Sturm, Jan-Egbert, 2019. The KOF globalisation index–revisited. The Review of International Organizations 14 (3), 543–574.
Habla, Wolfgang, Roeder, Kerstin, 2017. The political economy of mitigation and adaptation. Eur. Econ. Rev. 92, 239–257.
Herrnstadt, Evan, Muehlegger, Erich, 2014. Weather, salience of climate change and congressional voting. J. Environ. Econ. Manag. 68 (3), 435–448.
Horn, Alexander, 2021. The asymmetric long-term electoral consequences of unpopular reforms: why retrenchment really is a losing game for left parties. J. Eur. Publ. Pol. 28 (9), 1494–1517.
Hsiang, Solomon, Kopp, Robert, Amir, Jina, James, Rising, Delgado, Michael, Mohan, Shashank, Rasmussen, D.J., et al., 2017. Estimating economic damage from climate change in the United States. Science 356 (6345), 1362–1369.
Hsiang, Solomon M., Marshall, Burke, 2014. Climate, conflict, and social stability: what does the evidence say? Climatic Change 123 (1), 39–55.
ICRG, 2020. International Country Risk Guide (ICRG) Researchers Dataset.
IMF, 2019. Fiscal Monitor: How to Mitigate Climate Change (Washington, October).
IMF, 2020. World Economic Outlook: Growth and Distribution Friendly Climate Change Mitigation Strategies" (Washington, DC, October).
Jenkins, Jesse D., 2014. Political economy constraints on carbon pricing policies: what are the implications for economic efficiency, environmental efficacy, and climate policy design? Energy Pol. 69, 467–477.
Jennings, Will, Wlezien, Christopher, 2018. Election polling errors across time and space. Nat. Human Behav. 2 (4), 276–283.
Kahn, Matthew E., Mohaddes, Kamiar, Ryan, NC Ng, Hashem Pesaran, M., Raissi, Mehdi, Yang, Jui-Chung, 2019. Long-term Macroeconomic Effects of Climate Change: A Cross-Country Analysis. National Bureau of Economic Research. No. w26167.
Känzig, D.R., 2021. The Unequal Economic Consequences of Carbon Pricing.
Klenert, David, Gregor Schwerhoff, Edenhofer, Ottmar, Mattauch, Linus, 2018. Environmental taxation, inequality and Engel's law: the double dividend of redistribution. Environ. Resour. Econ. 71 (3), 605–624.
Klenert, David, Mattauch, Linus, Combet, Emmanuel, Edenhofer, Ottmar, Hepburn, Cameron, Ryan, Rafaty, Stern, Nicholas, 2017. Making Carbon Pricing Work.
Koch, Nicolas, Mama, Houdou Basse, 2019. Does the EU Emissions Trading System induce investment leakage? Evidence from German multinational firms. Energy Econ. 81, 479–492.
Kono, Daniel Yuichi, 2020. Compensating for the climate: unemployment insurance and climate change votes. Polit. Stud. 68 (1), 167–186.
Kotchen, Matthew J., Boyle, Kevin J., Leiserowitz, Anthony A., 2013. Willingness-to-pay and policy-instrument choice for climate-change policy in the United States. Energy Pol. 55, 617–625.
Lambsdorff, Johann Graf, 2003. How corruption affects productivity. Kyklos 56 (4), 457–474.
Latré, Edwin, Perko, Tanja, Peter, Thijssen, 2017. Public opinion change after the Fukushima nuclear accident: the role of national context revisited. Energy Pol. 104, 124–133.
Li, Aijun, Lin, Boqiang, 2013. Comparing climate policies to reduce carbon emissions in China. Energy Pol. 60, 667–674.
Liu, Xinsheng, Lindquist, Eric, Arnold, Vedlitz, 2011. Explaining media and congressional attention to global climate change, 1969-2005: an empirical test of agenda-setting theory. Polit. Res. Q. 64 (2), 405–419.
Magnus, Jan R., Powell, Owen, Prüfer, Patricia, 2010. A comparison of two model averaging techniques with an application to growth empirics. J. Econom. 154 (2), 139–153.
Markkanen, Sanna, Anger-Kraavi, Annela, 2019. Social impacts of climate change mitigation policies and their implications for inequality. Clim. Pol. 19 (7), 827–844.
McKibbin, Warwick J., Wilcoxen, Peter J., 2002. The role of economics in climate change policy. J. Econ. Perspect. 16 (2), 107–129.
Meguid, Bonnie M., 2005. Competition between unequals: the role of mainstream party strategy in niche party success. Am. Polit. Sci. Rev. 347–359.
Metcalf, Gilbert E., 2009. Designing a carbon tax to reduce US greenhouse gas emissions. Rev. Environ. Econ. Pol. 3 (1), 63–83.
Nationmaster, 2007. Countries compared by agriculture agricultural land sq. km per capita. In: International Statistics. Retrieved October 15, 2020, from. https://www.nationmaster.com/country-info/stats/Agriculture/Agricultural-land/Sq.-km/Per-capita. Retrieved October 15, 2020, from.
NOAA, 2019. HURDAT comparison table. Retrieved October 15, 2020, from. https://www.aoml.noaa.gov/hrd/hurdat/comparison_table.html.
Nunn, Nathan, Qian, Nancy, 2014. US food aid and civil conflict. Am. Econ. Rev. 104 (6), 1630–1666.
OECD, 2018. Input-Output Tables. and Development, Paris. For download at. http://oe.cd/i-o. Organisation for Economic Co-operation.
OECD, 2017. OECD Income Distribution Database (IDD): Gini, Poverty, Income, Methods and Concepts. Social and welfare issues.
OECD, 2012. The Jobs Potential of a Shift towards a Low-Carbon Economy". OECD Publishing.
Ostry, Jonathan D., 2021. Andrew Berg, and Siddharth Kothari. "Growth-equity trade-offs in structural reforms. Scot. J. Polit. Econ. 68 (2), 209–237.
Ostry, Jonathan D., Prakash, Loungani, Berg, Andrew, 2019. Confronting Inequality. Columbia University Press.
Persson, Torsten, Tabellini, Guido, 2002. Political Economics: Explaining Economic Policy. MIT press.
Pew Research Center (PEW), 2019. Climate Change Still Seen as the Top Global Threat, but Cyberattacks a Rising Concern".
Pierson, Paul, 1994. Dismantling the Welfare State?: Reagan, Thatcher and the Politics of Retrenchment. Cambridge University Press.
Portland State University. "Country Geography Data". Retrieved August 17, 2020, from https://www.pdx.edu/econ/country-geography-data.
Ramey, Valerie A., Zubairy, Sarah, 2018. Government spending multipliers in good times and in bad: evidence from US historical data. J. Polit. Econ. 126 (2), 850–901.
Rao, Narasimha D., 2013. Distributional impacts of climate change mitigation in Indian electricity: the influence of governance. Energy Pol. 61, 1344–1356.
Rojas-Vallejos, Jorge, Amy, Lastuka, 2020. The income inequality and carbon emissions trade-off revisited. Energy Pol. 139, 111302.
Samuelson, William, Zeckhauser, Richard, 1988. Status quo bias in decision making. J. Risk Uncertain. 1 (1), 7–59.
Sovacool, Benjamin K., Linnér, Björn-Ola, Goodsite, Michael E., 2015. The political economy of climate adaptation. Nat. Clim. Change 5 (7), 616–618.
Spoon, Jae Jae, Hobolt, Sara B., De Vries, Catherine E., 2014. Going green: explaining issue competition on the environment. Eur. J. Polit. Res. 53 (2), 363–380.
Stern, Nicholas, 2008. The economics of climate change. Am. Econ. Rev. 98 (no. 2), 1–37.
Steves, Franklin, Teytelboym, Alexander, 2013. Political Economy of Climate Change Policy.
Stiglitz, Joseph E., 2019. Addressing climate change through price and non-price interventions. Eur. Econ. Rev. 119, 594–612.
Stokes, Leah C., 2016. Electoral backlash against climate policy: a natural experiment on retrospective voting and local resistance to public policy. Am. J. Polit. Sci. 60 (4), 958–974.
Torgler, Benno, Schneider, Friedrich, 2007. Shadow Economy, Tax Morale, Governance and Institutional Quality: a Panel Analysis.
Torgler, Benno, Garcia-Valiñas, Maria A., 2007. The determinants of individuals' attitudes towards preventing environmental damage. Ecol. Econ. 63 (2–3), 536–552.
Tsebelis, George, 2002. Veto Players: How Political Institutions Work. Princeton University Press, Princeton, NJ.
Weaver, R. Kent, 1986. The politics of blame avoidance. J. Publ. Pol. 371–398.
Weitzman, Martin L., 1974. Prices vs. quantities. Rev. Econ. Stud. 41 (4), 477–491.
Welsch, Heinz, Biermann, Philipp, 2014. Fukushima and the preference for nuclear power in Europe: evidence from subjective well-being data. Ecol. Econ. 108, 171–179.

WID, 2020. World Inequality Database. Retrieved September 01, 2020, from https://wid.world/.

World Research Institute (WRI), 2000. Coastal and marine ecosystems — marine jurisdictions: coastline length units: kilometers. Retrieved October 15, 2020, from. https://web.archive.org/web/20120419075053/http://earthtrends.wri.org/text/coastal-marine/variable-61.html.

Yabré, Tobignaré, Semedo, Gervasio, 2021. Political stability and fiscal consolidation in sub-Saharan African countries. World Econ. 44 (4), 1077–1109.