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Essays in Political Economy

Fabian Ruthardt

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Preface

This thesis examines the reciprocity between the economy and elections, politicians, and policies. To inform voters about the consequences of their ballot choices, understanding the triangular relationship between the economy, political actors, and policies is essential. In representative democracies, people do not elect policies. They elect politicians, who serve as imperfect intermediaries to carry out the people's will. The four chapters aim to improve transparency about the origins and effects of economic policies. They each address certain aspects of policy preferences, formulation, timing, and impact. While each chapter constitutes a self-contained study, I encourage the reader to follow the given structure: Chapter 1 studies the consequences of a specific tax reform, a tariff increase, for GDP growth, the balance of trade, and state finances. Chapter 2 investigates how politicians time tax policies around elections. Chapter 3 examines the influence of extraordinary politicians on global economic expectations. Chapter 4 connects local exogenous shocks to voters' policy preferences and electoral outcomes. Unanticipated events and electoral uncertainty are common themes of all papers. Arguably the most surprising of all events considered in this thesis happened in Sweden in 1887, starting Chapter 1, *Protectionism and economic growth: Causal evidence from the first era of globalization*.

Swedish trade policies had been liberal for decades in the 19th century. The people supported free trade and kept electing free-trade majorities in the national parliament. However, an extraordinary tax scandal disqualified a free-trade candidate from Stockholm in the aftermath of the 1887 election. Based on the election statutes of 1866, the election committee declared all free-trade candidates from Stockholm illegitimate candidates and instated only protectionist candidates as representatives for the Stockholm electoral district. The supreme court confirmed the decision and the protectionists unexpectedly gained a comfortable majority in the national parliament. A protectionist government took office in 1888 and immediately increased tariffs by around 30%. Together with my co-authors Niklas Potrafke and Kaspar Wüthrich, I exploit this unexpected change of government to investigate how protectionist policies influence economic growth. We employ the synthetic control method to select control countries against which economic growth in Sweden can be compared. We do not find evidence that protectionist policies influenced economic growth and examine channels why. A mechanism through which changes in import tariffs may influence economic growth operates through fiscal policies. We show that the new tariff laws increased government revenue. However, the results do not suggest that the protectionist government stimulated the economy by increasing government expenditure.

Imposing tariffs on goods crossing the border is one of the earliest forms of taxation. More than a century before the surprising events in Sweden, protesters boarded ships of the East India Company and threw chests of tea into the Boston Harbor. The Sons of Liberty strongly opposed the British taxation policy on imports and excises. “No taxation without representation” was their slogan. But even with representation, taxation is painful – then and now. Politicians face a dilemma: They have to raise revenue to finance government spending for, e.g., the military, infrastructure, social welfare, and education. However, generating tax revenue is highly unpopular and voters regularly punish politicians for tax increases at the polls.

Chapter 2, *Read my lips? Taxes and elections*, continues the examination of tax policies. Clemens Fuest, Klaus Gründler, Niklas Potrafke, and I explore how electoral motives influence tax reforms. Research on many important questions on taxation is impeded by a lack of cross-nationally comparable data. We compile a new dataset that includes quantitative harmonized indices of tax reforms based on qualitative information from about 900 Economic Surveys from the OECD and 37,000 tax-related news from the IBFD archives collected by the IMF. Our dataset provides indicators on tax reforms for tax rates and tax bases, along with detailed sub-indices for six types of taxes (23 countries, 1960–2014). Relating tax reforms to the timing of elections, we provide first empirical evidence on electoral cycles in tax reforms at the national level. Our results show that politicians postpone tax rate increases until after the elections. The argument of “tax salience” suggests that voters are rationally ignorant about most tax measures and pay attention mostly to policies that directly affect them and are easily noticeable. Examining heterogeneity across tax types, we find that electoral cycles are particularly pronounced for salient taxes, such as value added tax rates and personal income tax rates.

The overarching theme of elections also permeates into Chapter 3 and politicians remain in the spotlight. The timing of elections is important for exploiting electoral uncertainty, meaning that electoral outcomes are unpredictable ex ante. Levels of electoral uncertainty vary depending on the election. Dorine Boumans, Klaus Gründler, Niklas Potrafke, and I focus on a particular election with high electoral uncertainty: The 2020 US presidential election. We investigate whether extraordinary politicians can influence macroeconomic expectations on a global scale. New political movements transform the political system of many advanced democracies. Nationalists and social conservatives increasingly exhibit political cleavage with cosmopolitans and social progressives. New parties and politicians campaign on anti-establishment and anti-elite platforms. The 2020 US presidential election between Donald Trump and Joe Biden is a prime example of a polarized election. It makes an ideally-suited quasi-natural experiment for identifying the causal effect of an exceptional politician on macroeconomic expectations because (i) the United States wields significant economic and political influence in the world, (ii) the election garnered high levels of public attention, and (iii) concurrent elections provided little scope for confounding. In *Political leaders and macroeconomic expectations: Evidence from a global survey experiment*, we provide first evidence on political spillover effects in the formation of macroeconomic expectations. The study's core is a large-scale survey experiment among 837 influential economic experts working in more than 100 countries. The survey was distributed in two waves, where each wave consisted of a randomly selected subset of participants. We gathered the results of the first wave shortly before the election (control group). The second wave was collected five days later after electoral uncertainty had been resolved by major US media outlets calling Joe Biden president-elect (treatment group). We find large effects of Joe Biden's election on national growth expectations of international experts, working through more positive expectations about trade. The electoral outcome particularly affected the expectations of Western allies. Our findings suggest important political spillover effects in the formation of macroeconomic expectations.

In the last chapter of this thesis, I remain in the United States and turn to local events and US gubernatorial election outcomes. While uncertainty persists as a theme, it no longer pertains to electoral ambiguity, but rather to unanticipated adverse shocks. The previous chapters studied the propositions, timing, and effects of policies. I now return to the origins of economic policy and explore the formation of policy preferences. The economic and political science literature has long acknowledged that natural disasters influence electoral outcomes. Yet, the focus has been primarily limited to incumbent vs. challenger races, with little attention to the political affiliation of the candidates. Results are mixed on whether incumbents benefit from natural disasters. Depending on the sign of the effect, studies identified irrational retrospective voting, disaster relief effort, and information updating as possible links between the negative shock and a change in voter behavior. I provide evidence for another reason: Changing policy preferences due to the shock itself.

In *Democratic tornadoes? County-level evidence on the influence of local disasters on US gubernatorial elections*, I study how deadly tornadoes influenced electoral outcomes from

1960 to 2020. I combine county-level election data with geospatial information on the incidence of tornadoes. Tornadoes make useful natural experiments because (i) they vary over space and time, (ii) are limited in their geographic scope, and (iii) are short-lived and unpredictable. My main result is that Democratic candidates benefit from deadly tornadoes. The Democratic vote share is 0.43 percentage points higher in counties that experienced a deadly tornado since the last gubernatorial election. The effect increases with treatment intensity. Deadly tornadoes within 12 months before the election and tornadoes causing more fatalities trigger a stronger response by the electorate. Based on a spatial model of voting with valence, I present suggestive evidence that voters' political preferences shift toward the policy platform offered by Democratic candidates after the crisis experience.

Keywords: Economic growth, elections, electoral cycles, government revenue, international trade, natural disasters, tax reforms, voter behavior

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Fabian Ruthardt, March 2023

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1 Protectionism and economic growth: Causal evidence from the first era of globalization*

1.1 Introduction

How trade policies influence economic growth has been examined for a long time. Empirical evidence based on data for the late 20th and the early 21st century suggests that protectionist policies decrease economic growth.[1] The empirical evidence from the late 19th and the early 20th century is less conclusive; most studies report positive correlations between tariffs and economic growth ('tariff-growth paradox').[2] However, such positive correlations do not provide causal evidence on how protectionism influences growth because most policy changes are endogenous. Reverse causality and anticipation effects give rise to biases when applying, for example, standard panel data approaches based on international cross-sections.

We provide causal evidence on the tariff-growth paradox by investigating a rare case of a plausibly exogenous change in trade policy. We exploit that an extraordinary tax scandal in the fall of 1887 gave rise to an unexpected change of government in Sweden. Swedish trade policies had been liberal for decades in the 19th century. Advocates of free trade (free-traders) also won the Swedish national elections in 1887 by a large margin. Two weeks after the election, a newspaper editor appealed the election results, claiming that a free-trade candidate from Stockholm was an illegitimate candidate because of outstanding tax liabilities. To the surprise of many, the election committee discarded all ballots with votes for the free-trade candidate and instated protectionist candidates as representatives for the Stockholm electoral district in the *Riksdag*. In January 1888, the supreme court confirmed the decision of the election committee. The free-trade majority in the second chamber of parliament was overturned by a comfortable protectionist majority, and the free-trade government resigned. A protectionist government took office in February 1888 and increased tariffs by around 30% (Persarvet, 2019).

The unanticipated change of government provides an ideal case for investigating how

* This chapter is based on joint work with Niklas Potrafke and Kaspar Wüthrich.

[1] See, for example, Sachs and Warner (1995), Frankel and Romer (1999), Rodríguez and Rodrik (2000), Dreher (2006), Gygli et al. (2019), and Furceri et al. (2020).

[2] See, for example, Bairoch (1972), Irwin (1998), Irwin (2002), O'Rourke (2000), Lehmann and O'Rourke (2011), and Schularick and Solomou (2011).

protectionist policies influence short-run economic growth. First, because the change of government was unanticipated and eventually decided by a court, anticipation effects and reverse causality are unlikely to bias our estimates. Second, the tariff increase was large. Overall, tariffs increased by around 30%, and all industries were affected by the tariff laws. Third, customs revenue was the most important revenue stream for state finances at the time and made up 42% of total government revenue in 1888/89 (Häggqvist, 2018). Fourth, trade policy was the central topic defining political competition, and the new tariff laws were the only major policy change implemented by the protectionist government. Finally, changes in tariff laws were quickly perceived by merchants in the first era of globalization. The telegraph, for example, was used frequently in the late 19th century and reduced information frictions in international trade (Steinwender, 2018).

We employ the synthetic control (SC) method (Abadie and Gardeazabal, 2003) to select control countries against which economic growth in Sweden can be compared. We do not find evidence suggesting that the protectionist policies influenced short-run economic growth in 19th century Sweden. The results corroborate that the short-run effects of protectionism are likely to be context-specific (Eichengreen, 2019). Our study shows that focusing on exogenous variation is essential to better understand the 'tariff-growth paradox' in the first era of globalization.

A channel through which changes in import tariffs may influence short-run economic growth operates through changes in government revenue and fiscal policies. How changes in import tariffs affect imports depends on the tariff rate and the elasticity of import demand. We find that imports did not decrease and government revenue increased. Consequently, the protectionist government needed to decide how to spend the additional revenue. While increases in government expenditure translate quickly into higher GDP (Ramey and Zubairy, 2018; Ramey, 2019), our results do not show that the Swedish government increased government expenditure. The Swedish government used the additional revenue to consolidate budgets and repay public debt. Consistent with our empirical results, budget consolidation is unlikely to increase short-run economic growth.

Methodologically, our paper is most closely related to Billmeier and Nannicini (2013), who employ the SC method to examine how trade policy reforms influenced economic growth in the 20th century. Using the SC method in the literature on trade policies and economic growth has been a major innovation (Irwin, 2019). However, the variation in the trade policy reforms investigated by Billmeier and Nannicini (2013) is not exogenous. While SC can accommodate some forms of selection on unobservables (e.g., Ferman, 2021), it is unlikely to completely eliminate the endogeneity bias in such settings. We use the SC method and examine a change of government that induced exogenous variation in Swedish trade policy.

We also contribute to the literature exploiting quasi-exogenous variation to examine how protectionism influenced economic development in the 19th century (e.g., Juhasz, 2018). We provide well-identified reduced-form evidence on the effect of protectionism on short-run growth in late 19th century Sweden.

More broadly, we contribute to studies investigating the relationship between trade, trade policy, and political outcomes.[3] Trade increases the power of merchants and enables them to implement institutional change (e.g., Acemoglu et al., 2005; Puga and Trefler, 2014). However, evidence suggests that in the context of the first era of globalization, trade increased the support for protectionist policies (e.g., *'puzzle of America's 19th century protectionism'* (Gourevitch, 1977; Epstein and O'Halloran, 1996; Scheve and Serlin, 2023)), the welfare state (Scheve and Serlin, 2022), and progressive taxation (Barnes, 2020). Similarly, evidence for the 20th and 21st century suggests that trade exposure changes political preferences (e.g., see Colantone et al. (2022) for a survey on the backlash against globalization and Ballard-Rosa et al. (2021) for a case study on trade shocks and their influence on the 2016 Brexit referendum).

1.2 Change in government and protectionism

1.2.1 The 1887/1888 change in government

Sweden pursued a liberal trade policy since the late 1850s (Rustow, 1955).[4] In 1885, members of both chambers of the Swedish parliament started to organize themselves according to their stance on trade policy (Rustow, 1955; Lewin, 1988). The result was a face-off between free-traders and protectionists. The free-traders won the election in fall 1887 by a large margin (Andersson, 1950).[5] Thus, it was very likely that the liberal trade policy would have been continued.

Shortly after the fall election, an unexpected event took place, which was characterized as "sensational" (Lewin, 1988), "preposterous" (Carlsson and Rosén, 1961), and "scandalous"

[3] See, for example, Becker (1983), Rogowski (1987), Gowa and Mansfield (1993), Milner (1999), Hiscox (2002), Milner and Kubota (2005), Feigenbaum and Hall (2015), and Kim (2017).
[4] See, for example, Schonhardt-Bailey (1991), Lake (1988), Lazer (1999), Coutain (2009), Morrison (2012), and Fajgelbaum and Redding (2022) for studies on the origin of and the political environment during the first era of globalization.
[5] See Lehmann and Volckart (2011) for a description of the electorates of free-traders and protectionists.

(Esaiasson, 1990). Stockholm's electoral district was entitled to 22 seats in the second chamber of parliament (Rustow, 1955). Citizens in Stockholm elected only free-traders into parliament by large vote margins.[6] The election's appeal period lasted until October 4, 1887. Two citizens filed appeals against the election results in Stockholm's electoral district (Stockholms Dagblad, 1887). The appeal by Wilhelm Alexander Bergstrand, the publisher of the newspaper *Nya Dagligt Allehanda*, induced political turmoil in Stockholm and soon after in the whole country.

On October 4, 1887, shortly before the appeal period ended, Bergstrand submitted his appeal and published it in *Nya Dagligt Allehanda* on the same day (Bergstrand, 1887). In his appeal, Bergstrand claimed that Olof Larsson, one of the 22 free-trade candidates, owed a small amount of crown and municipal taxes for 1881 and 1882. According to paragraphs 25 and 26 of the Parliament Act of 1866, a candidate with tax debt is disqualified, and all ballots with votes for the respective candidate are invalid (Lagerbjelke et al., 1866). Bergstrand demanded that all ballots with votes for Larsson must be declared invalid. He further demanded a recount of all valid votes. On October 5, 1887, Bergstrand published proof for Larsson's tax liabilities: the tax collection commissioner for Adolf Fredriks and Kungsholms (two districts in Stockholm) had issued a certificate confirming Larsson's tax liabilities on October 4, 1887 (Geete, 1887).

Events unraveled during the following days. Many newspapers published opinions about the legitimacy of the appeal. Larsson's statement in *Aftonbladet*, one of the most influential newspapers at the time, disputed any tax liabilities but remained without the intended effect (Larsson, 1887). On October 12, 1887, the election committee accepted Bergstrand's appeal and invalidated all ballot papers with votes for Larsson (Lindorm, 1936). It ordered a recount of the votes and declared the 22 protectionist candidates winners of the election. Disputes followed and the decision of the election committee was challenged. On January 25, 1888, the supreme court ruled that the 6,585 ballot papers with votes for Larsson are indeed invalid and officially instated the 22 protectionist candidates as legitimate representatives of the electoral district of Stockholm in the *Riksdag* (Lewin, 1988). The free-trade majority in the second chamber of parliament (125 free-traders, 97 protectionists) was overturned by a comfortable protectionist majority (103 free-traders, 119 protectionists).[7] As a result, the free-trade government resigned on February 6, 1888, and the experienced protectionist Gillis Bildt became Prime Minister (Lindorm, 1936).[8] In February 1888, Bildt's government issued

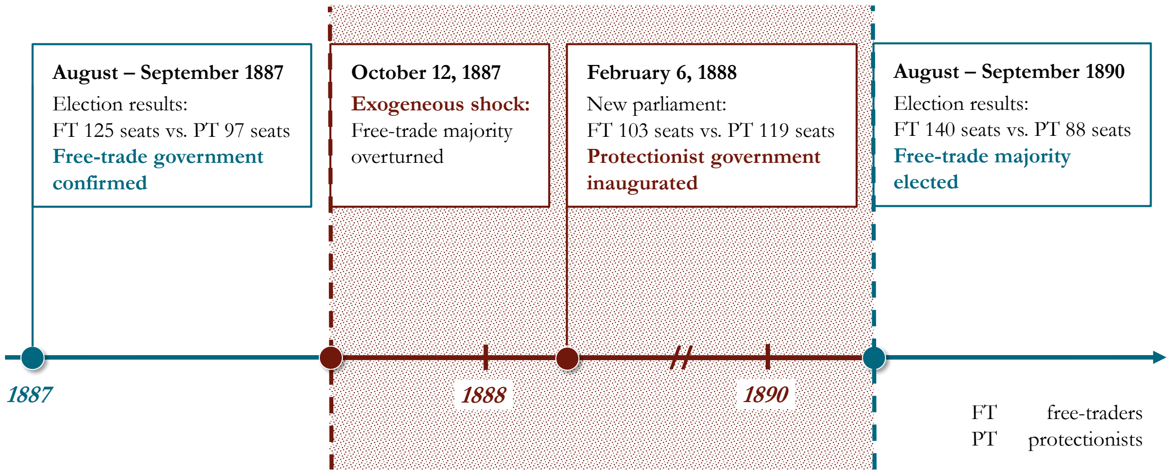
[6] Stockholm was the main stronghold of free-trade sentiment at the time. See Appendix A for the fall election results for the electoral district of Stockholm.

[7] Both chambers of parliament decide on trade policy, and each representative has one vote.

[8] Bildt served as Swedish ambassador in Berlin when the *Reichstag* under Bismarck introduced the agrarian protectionist system in 1879.

the first tariff laws. See Section 1.2.2 for details on the new tariff laws. Figure 1.1 shows the timeline of the main events.

Figure 1.1 : The 1887/1888 change in government



Source: Own illustration.

The change in parliamentary majorities in the aftermath of the 1887 fall election occurred unexpectedly. We reviewed hundreds of articles from regional and national Swedish newspapers from before the September 1887 election up to January 25, 1888.[9] We found no indication that the tax debt was publicly known before the election.

1.2.2 Swedish protectionist policies

The protectionist government increased overall tariffs by around 30% in 1888 (Persarvet, 2019).[10] The tariff increase was heterogeneous across product classes.

We follow Persarvet (2019) and classify the goods of the Swedish historical trade statistics according to the Standard International Trade Classification (SITC) framework. Tariffs on food and beverages increased substantially (SITC sections 0–1). The protectionist government raised food tariffs on average by six percentage points. The increase affected 36% of total imports. The largest tariff increase was on grain (from 2% to 27%).

Tariffs on raw materials and fuels increased only to a small extent (SITC sections 2–4). The

[9] We used a search algorithm with keywords and time periods for Swedish newspaper articles provided by the National Library of Sweden (*Kungliga biblioteket, KB*).

[10] We contribute to the long-standing debate on the effect of the Swedish tariff increases on growth by providing first causal evidence (e.g., Heckscher, 1941; Montgomery, 1966; Hammarström, 1970; Schön, 1989; Bohlin, 2005; Häggqvist, 2018; Persarvet, 2019). We review this debate in more detail in Appendix B.

tariff increase on scrap metal increased the average tariff on ores and metal scrap. New tariffs on lard increased the average tariff on animal and vegetable fats. Coal, coke, and crude oil remained duty-free.

Tariffs on manufactured products increased slightly (SITC sections 5–9). Most of the industrial tariffs were still bound by the Franco-Swedish trade agreement.[11] Tariffs increased on iron and steel products through the introduction of new tariffs on sheet metal, steel beams, cast steel, and metal wire.

1.3 Data and empirical strategy

1.3.1 Data

We use data from the Jordà-Schularick-Taylor Macrohistory Database (Jordà et al., 2017).[12] The Jordà-Schularick-Taylor Database includes annual data for 17 advanced economies since 1870. It encompasses measures of GDP[13], imports, central government revenue, and central government expenditure. Data comes from a broad range of historical sources and various publications of governments, statistical offices, central banks, and private banks. For some countries, the authors extended data series from university databases and international organizations. The main source for our GDP measure is the Macroeconomic Data Set (Barro and Ursúa, 2010). Most trade and national account data come from Mitchell (2007), Flora et al. (1983), IMF international financial statistics, OECD national accounts statistics, and national statistics offices.

We examine data until 1890 because the next election took place in the fall of 1890. The free-traders won this election. An advantage of using a short post-treatment period is that other potential confounding events are unlikely to affect our analysis.

1.3.2 The synthetic control method

We employ the SC method (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015); see Abadie (2021) for a review.[14] SC approximates what would have happened to Sweden with

[11] In 1860, France and Great Britain signed the Cobden-Chevalier treaty. This triggered a large number of most favored nation treaties on the European continent and contributed to a period of relatively free trade (Lampe, 2009; Tena-Junguito et al., 2012). France and Sweden signed a trade agreement in 1865. When this agreement expired in 1892, Sweden regained tariff autonomy and substantially increased tariffs on industrial products (Persarvet, 2019).
[12] The data are available here: `http://www.macrohistory.net/data/`.
[13] We use real GDP per capita (index, 2005=100).
[14] There is a growing body of work using SC to make causal inference in aggregate panel datasettings (e.g., Billmeier and Nannicini, 2013; Bohn et al., 2014; Pinotti, 2015; Cunningham and Shah, 2018; Asatryan et al., 2018;

a free-trade government using a weighted average of control countries. We perform the empirical analyses in `Stata` (StataCorp., 2019) and `R` (R Core Team, 2020).

To describe the SC method formally, we use the potential outcomes framework (Rubin, 1974). We denote by Y_{jt}^F and Y_{jt}^P the potential outcome of country j in period t with a free-trade (F) and a protectionist (P) government. Our main outcome of interest is real GDP per capita, and we also investigate imports, government revenue, and government expenditure. Let $j = 1$ index Sweden and $j = 2, \ldots, J+1$ index the J control countries. We discuss the choice of the J control countries, our *donor pool*, in Section 1.3.3.

Our purpose is to estimate the causal effect of protectionism between 1888 and 1890 (the year of the next election),

$$\tau_t = Y_{1t}^P - Y_{1t}^F, \quad t \in \{1888, 1889, 1890\} \ . \tag{1.1}$$

For Sweden, we observe Y_{1t}^F until 1887 and Y_{1t}^P afterwards. For the control countries, we observe Y_{jt}^F for all periods. Thus, to estimate τ_t, we need to estimate the unobserved potential outcome Y_{1t}^F. We use the following estimator

$$\hat{Y}_{1t}^F = \sum_{j=2}^{J+1} \hat{w}_j Y_{jt}^F \ . \tag{1.2}$$

We refer to the weighted average in Equation 1.2 as *synthetic Sweden*. The SC weights $\left(\hat{w}_2, \ldots, \hat{w}_{J+1}\right)$ are estimated by minimizing the discrepancy between the pre-treatment outcomes for Sweden and synthetic Sweden using the Stata package `synth` (Abadie et al., 2011). To avoid concerns about specification search, we do not include additional predictors. The weights are restricted to be positive and add up to one, which ensures transparency and precludes extrapolation (see Section 3.2 in Abadie, 2021).

SC generalizes difference-in-differences (DID). To approximate Y_{1t}^F, DID employs simple averages of control units chosen by the researcher. By contrast, SC chooses controls in an automatic data-driven way, employing a weighted average (Equation 1.2) to approximate

Eliason and Lutz, 2018; Andersson, 2019; Born et al., 2019; Potrafke and Wüthrich, 2020).

Y_{1t}^{F}. As a result, SC is less susceptible to specification search and often provides a better counterfactual approximation. See Section 4 in Abadie (2021) for further discussions of the advantages of SC.

To make inferences, we employ the permutation method proposed by Abadie et al. (2010). See Firpo and Possebom (2018) and Abadie (2021) for further discussions. In Section 1.6, we apply the conformal inference procedure of Chernozhukov et al. (2021) as an additional robustness check.[15]

1.3.3 Choice of donor pool

We restrict our donor pool of control units to countries that had free-trade governments from 1870 to 1890. From the 17 countries available in the Jordà-Schularick-Taylor Database, we exclude France, Germany, Italy, Spain, and Portugal because of protectionist trade policies.[16] Data is missing for Australia and Japan. Therefore, our donor pool includes Belgium, Canada, Denmark, Finland, the Netherlands, Norway, Switzerland, the United Kingdom, and the United States. In Section 1.6, we present results for a restricted donor pool with only European countries.

An important requirement for SC analyses is that the donor pool of control countries is homogeneous enough (Abadie, 2021). All countries in our donor pool were industrializing during the 1870s and 1880s. Citizens or elected representatives of the citizens possessed substantial political power and influenced national policies.

1.4 Results: Protectionism and growth

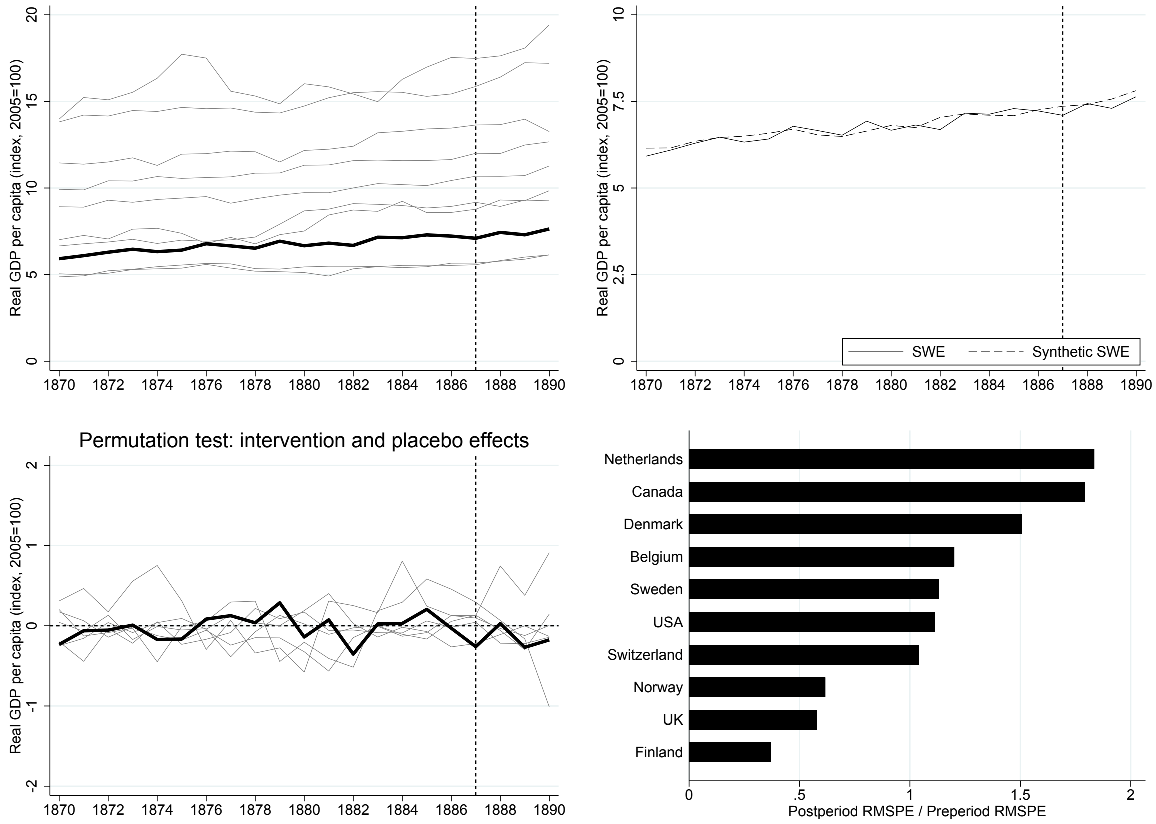
The upper left panel of Figure 1.2 shows real GDP per capita for each donor pool country and Sweden from 1870 to 1890. Sweden's GDP is shown in thick black; the control countries' GDPs are shown in grey. The upper right panel shows how real GDP per capita developed in Sweden and synthetic Sweden over the period 1870–1890. The synthetic Sweden consists of 21.7% Denmark, 43.6% Finland, 17.3% Norway, 0.3% United Kingdom, and 17.0% United States (Table A.3 in Appendix E).

[15] We implement the conformal inference procedure using the `R`-package `scinference`.

[16] We use country classifications of previous studies (e.g., O'Rourke et al., 1996; O'Rourke, 2000; Irwin, 1998, 2002; Rodríguez and Rodrik, 2000; Clemens and Williamson, 2004; Williamson, 2006; Schularick and Solomou, 2011) and classify countries either as "protectionist"/"tariff hikers" or "free-trade"/"non-tariff hikers".

We find no evidence suggesting that protectionism influenced real GDP per capita. From 1870 to 1887, Sweden's average real GDP per capita grew from 5.92 to 7.10 (average annual growth rate (AAGR): 1.07%), and synthetic Sweden's average real GDP per capita grew from 6.15 to 7.36 (AAGR: 1.06%). After the change in government, from 1887 to 1890, Sweden's average real GDP per capita grew from 7.10 to 7.64 (AAGR: 2.47%), and synthetic Sweden's average real GDP per capita grew from 7.36 to 7.81 (AAGR: 2.01%).

Figure 1.2 : Real GDP per capita



Notes: Real GDP per capita is shown as an index (2005 = 100). The lower left panel excludes countries for which the pre-treatment MSPE is at least 10 times larger than Sweden's pre-treatment MSPE. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

To make inferences, following Abadie et al. (2010), we iteratively re-assign the treatment of having a protectionist government to each country in the donor pool. Because SC does not yield good pre-treatment fits for some control countries, we exclude countries for which the pre-treatment mean squared prediction error (MSPE) is more than 10 times larger than the pre-treatment MSPE for Sweden (lower left panel of Figure 1.2). The results do not suggest that the effect of protectionism on GDP in Sweden was large relative to the distribution of placebo effects. Since the cutoff of 10 is arbitrary, we also report the ratios of post-treatment root MSPE (RMSPE) to pre-treatment RMSPE, as suggested by Abadie et al. (2015). A large RMSPE ratio indicates a rejection of the null hypothesis that protectionism had no effect. The lower right panel of Figure 1.2 suggests that Sweden's ratio was not large compared to the other countries in the donor pool.

In Section 1.6, we show that our results are robust to potential spillover effects from Sweden's tariff policy on its trading partners and to restricting the donor pool to European countries.

1.5 Channels

We examine channels for why there is no evidence suggesting that protectionism influenced short-run economic growth. We focus on outcomes of international trade and fiscal policies that are likely to influence short-run economic growth.[17]

1.5.1 Imports

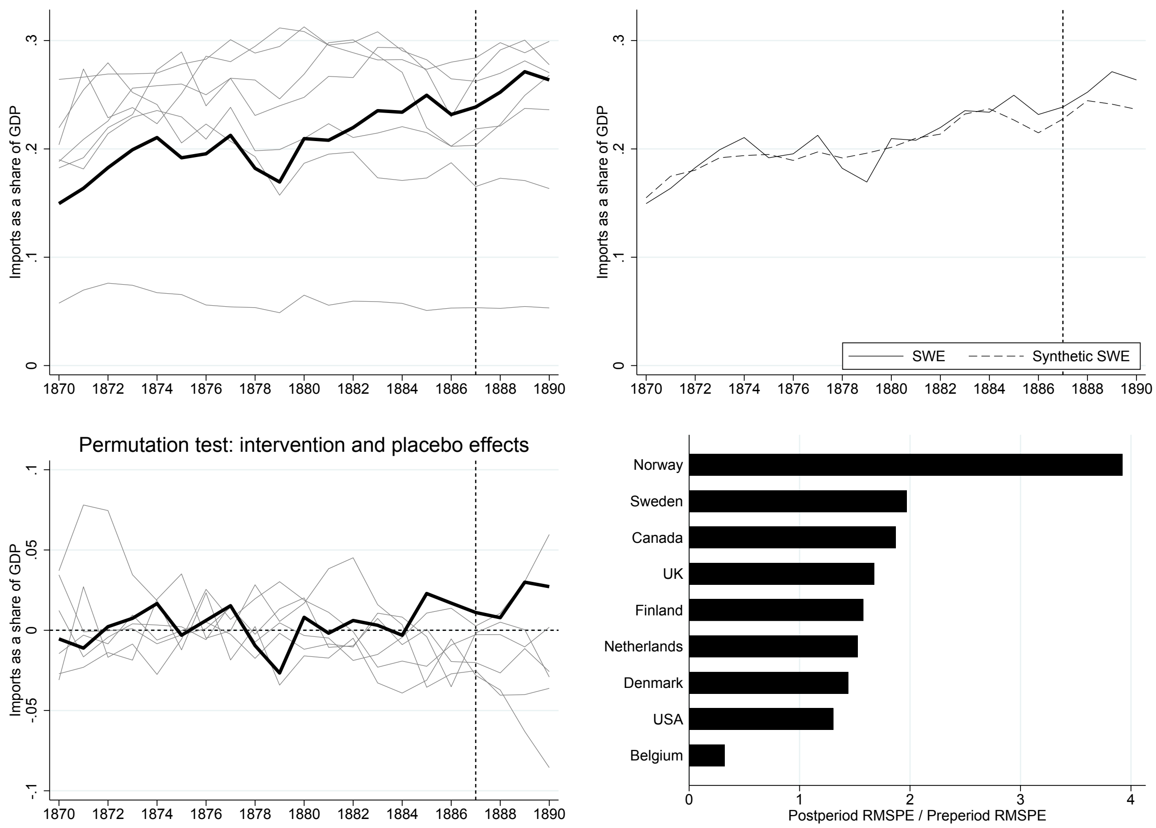
It is conceivable that protectionism decreased imports, especially from those countries from which Sweden imported a substantial fraction of its goods. However, the empirical results in Figure 1.3 do not suggest that the introduction of tariffs decreased imports. The total value of imports increased from 297.41 million SEK in 1887 to 324.71 million SEK in 1888. The protectionist tariff policy implemented in early 1888 did not reverse the steady growth of imports. The total value of imports as a share of GDP increased from 14.95% in 1870 to 23.87% in 1887. In 1888, imports as a share of GDP increased to 25.23% and reached 26.37% in 1890.

We do not find evidence that aggregate import levels masked heterogeneous effects of the Swedish tariffs on individual trading partners. See Appendix D.1 for how Swedish imports from individual countries developed between 1870 and 1890.

Bildt's government increased tariffs to different extents across sectors (see Section 1.2.2). Appendix D.2 shows how the composition of Swedish imports across sectors developed between 1870–1890. Agricultural imports remained stable at a high level, and manufactured imports continued their growth path after 1888. Based on our data, we cannot disentangle the effects of tariffs on agricultural imports and tariffs on manufactured imports. However, we do not observe that the composition of imports changed substantially after 1887.

[17] Clearly, we would have liked to also examine the extent to which short-run economic growth was influenced by changes in firms' productivity. However, there is no data on firms' productivity or TFP across countries in the late 19th century available, which we could use in our SC analyses.

Figure 1.3 : Imports as a share of GDP



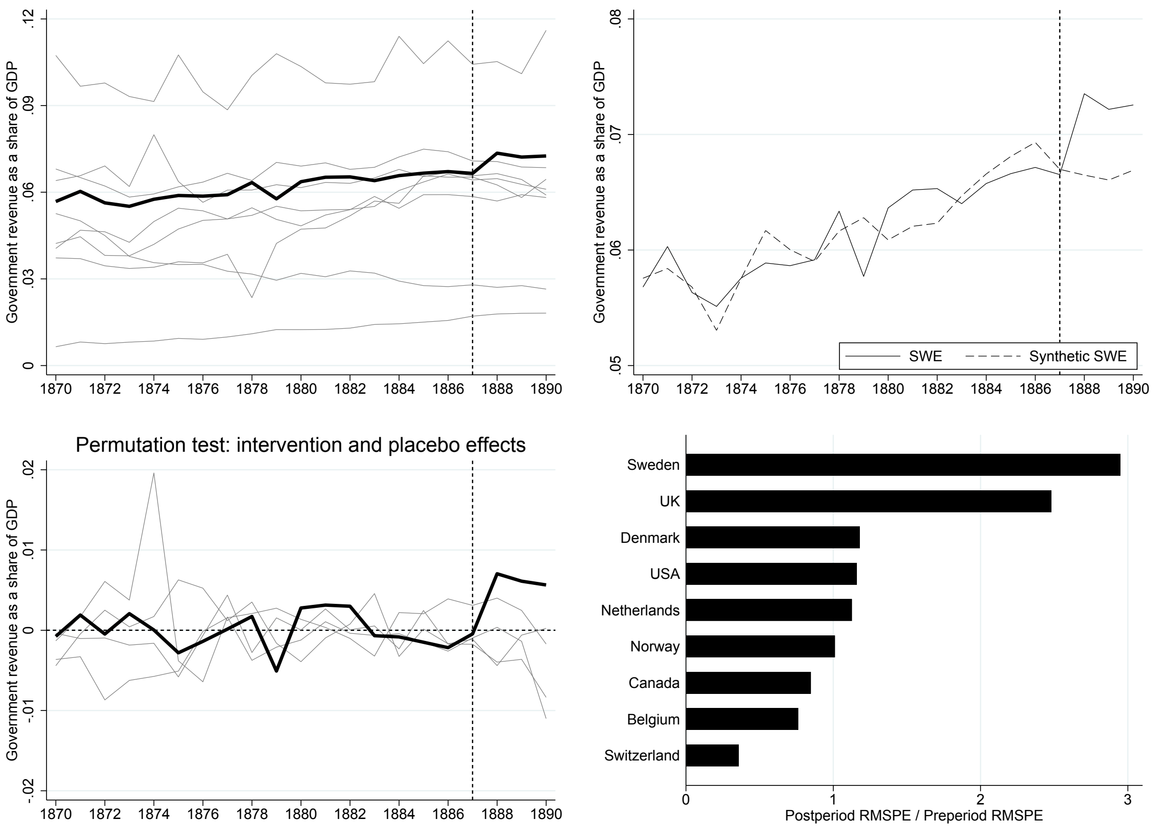
Notes: The graphs (upper left panel) do not include the Netherlands' imports. The Netherlands' imports as a share of GDP were between 54.43% and 107.95% (1870–1890). Data is missing for Switzerland. The lower left panel excludes countries for which the pre-treatment MSPE is at least 10 times larger than Sweden's pre-treatment MSPE. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

1.5.2 Government revenue

We examine how the protectionist policies influenced government revenue. Whether the higher tariffs decreased or increased government revenue depends on the tariff rate and the elasticity of import demand. As shown in Section 1.5.1, imports did not decrease when tariffs were increased. Hence, higher tariffs may well have increased government revenue.

Figure 1.4 shows that the protectionist policies enacted after the change of government increased government revenue. The ratio of the post-treatment to the pre-treatment RMSPE is the largest for Sweden. If one were to select a country at random, the probability of obtaining a ratio as high as Sweden's is $1/9$ (see Abadie et al., 2015, for a further discussion of this interpretation). This result is robust to using the conformal inference procedure of Chernozhukov et al. (2021), see Section 1.6.

Figure 1.4 : Government revenue as a share of GDP



Notes: Data is missing for Finland. The lower left panel excludes countries for which the pre-treatment MSPE is at least 10 times larger than Sweden's pre-treatment MSPE. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

Government revenue was 81.11 million SEK in 1887. It increased by 16.02% to 94.11 million SEK in 1888. As a share of GDP, government revenue increased from 6.65% to 7.35% and remained relatively stable until 1890 (1889: 7.22%, 1890: 7.26%). Meanwhile, synthetic Sweden's government revenue as a share of GDP decreased from 6.70% in 1887 to 6.65% in 1888. It remained relatively stable until 1890 (1889: 6.61%, 1890: 6.69%). Customs revenue was responsible for the increase in government revenue (see Appendix C for a description of Swedish fiscal policies 1888–1890). Because imports did not decrease when the protectionist policies were introduced, it is unlikely that tariffs were systematically circumvented.[18]

Our results are in line with empirical evidence from the United States in the 1880s (Irwin, 1998). The United States enjoyed high fiscal surpluses in the early 1880s, and the political parties

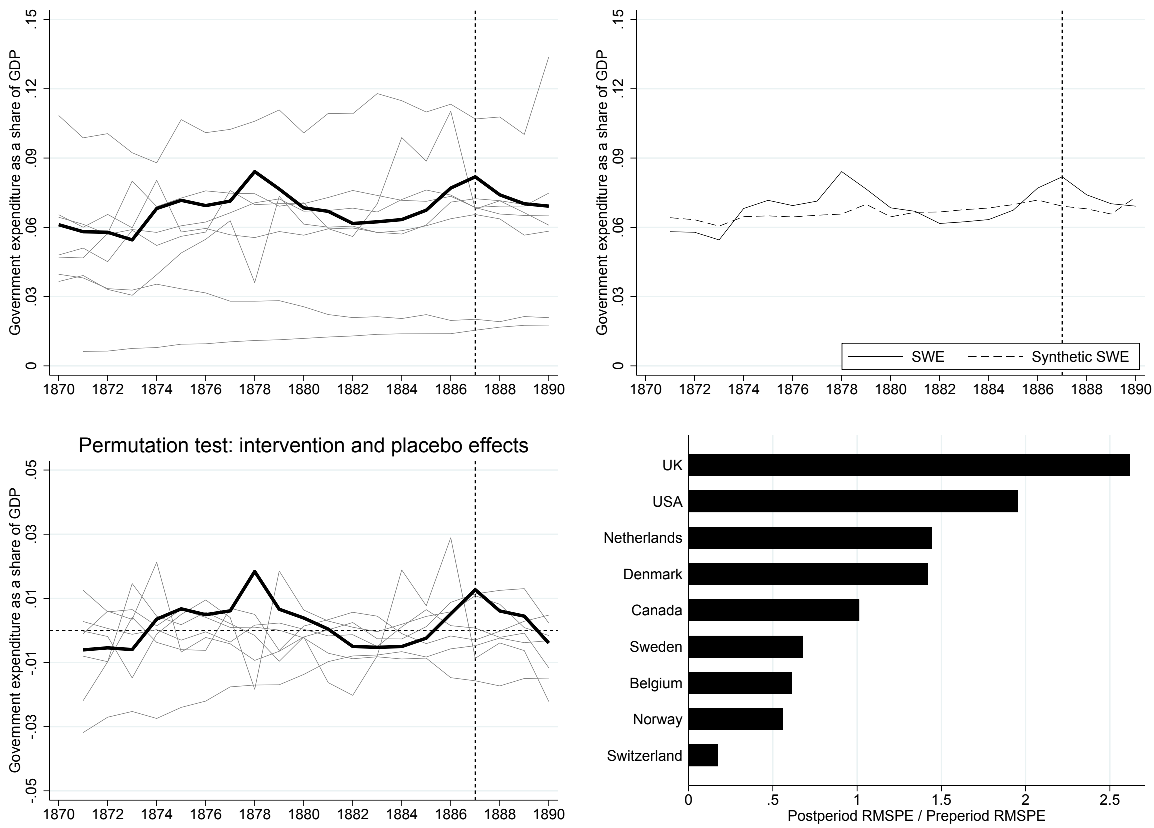
[18] Further, given the development of Swedish imports from Norway after 1887, it is unlikely that goods destined for Sweden were shipped to Norway and then crossed country borders by rail; see Appendix D.1.

were discussing how changing import tariffs would influence customs revenues. Irwin (1998) estimates the revenue effects of the proposed tariff changes and concludes that lower import tariffs would also have reduced customs revenues.

1.5.3 Government expenditure

Figure 1.5 shows the SC estimates for government expenditure. The results do not suggest that the protectionist government influenced government expenditure. Swedish government expenditure as a share of GDP decreased from 8.19% in 1887 to 6.92% in 1890. Synthetic Sweden's government expenditure as a share of GDP increased from 6.92% to 7.31% over the same period.

Figure 1.5 : Government expenditure as a share of GDP



Notes: Data is missing for Switzerland's government expenditure in 1870. Therefore, we calculate the synthetic Sweden based on the best pre-treatment fit from 1871 to 1887. Data is missing for Finland. The lower left panel excludes countries for which the pre-treatment MSPE is at least 10 times larger than Sweden's pre-treatment MSPE. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

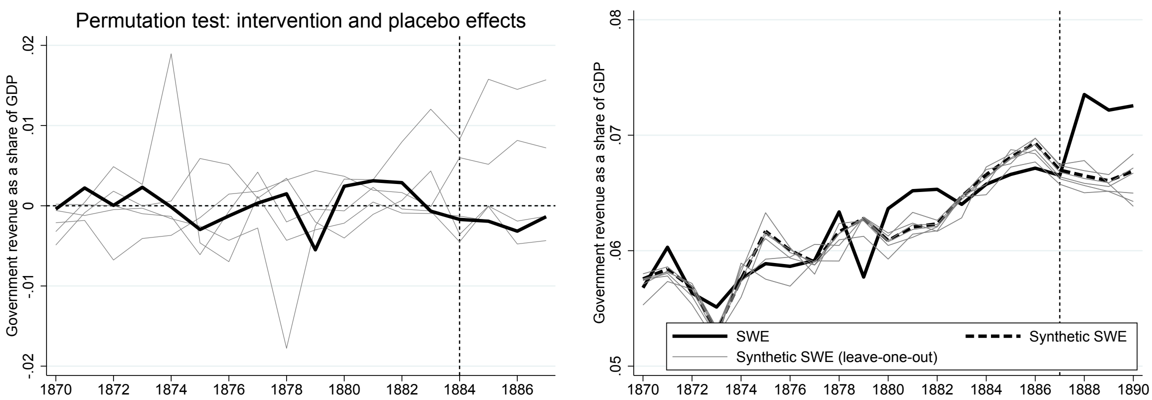
Sweden went from a large primary budget deficit in 1887 to a small primary budget surplus in 1888. The surplus increased in 1889 and 1890. In both years, Sweden had a total budget surplus and total government debt decreased.

1.6 Robustness checks

We submit the estimated effect of protectionism on government revenue to four robustness checks and also apply the conformal inference procedure of Chernozhukov et al. (2021).

First, following Abadie et al. (2015), we backdate the treatment and consider a placebo change of government in the previous election year (1884). A significant effect of this placebo treatment would threaten the credibility of our findings. The results from the permutation inference procedure do not indicate an effect of the placebo treatment on government revenue (left panel of Figure 1.6). The ratio of post-treatment to pre-treatment RMSPE for Sweden is smaller than one and only the sixth highest among all countries (not shown).

Figure 1.6 : Placebo treatment in 1884 and leave-one-out sensitivity

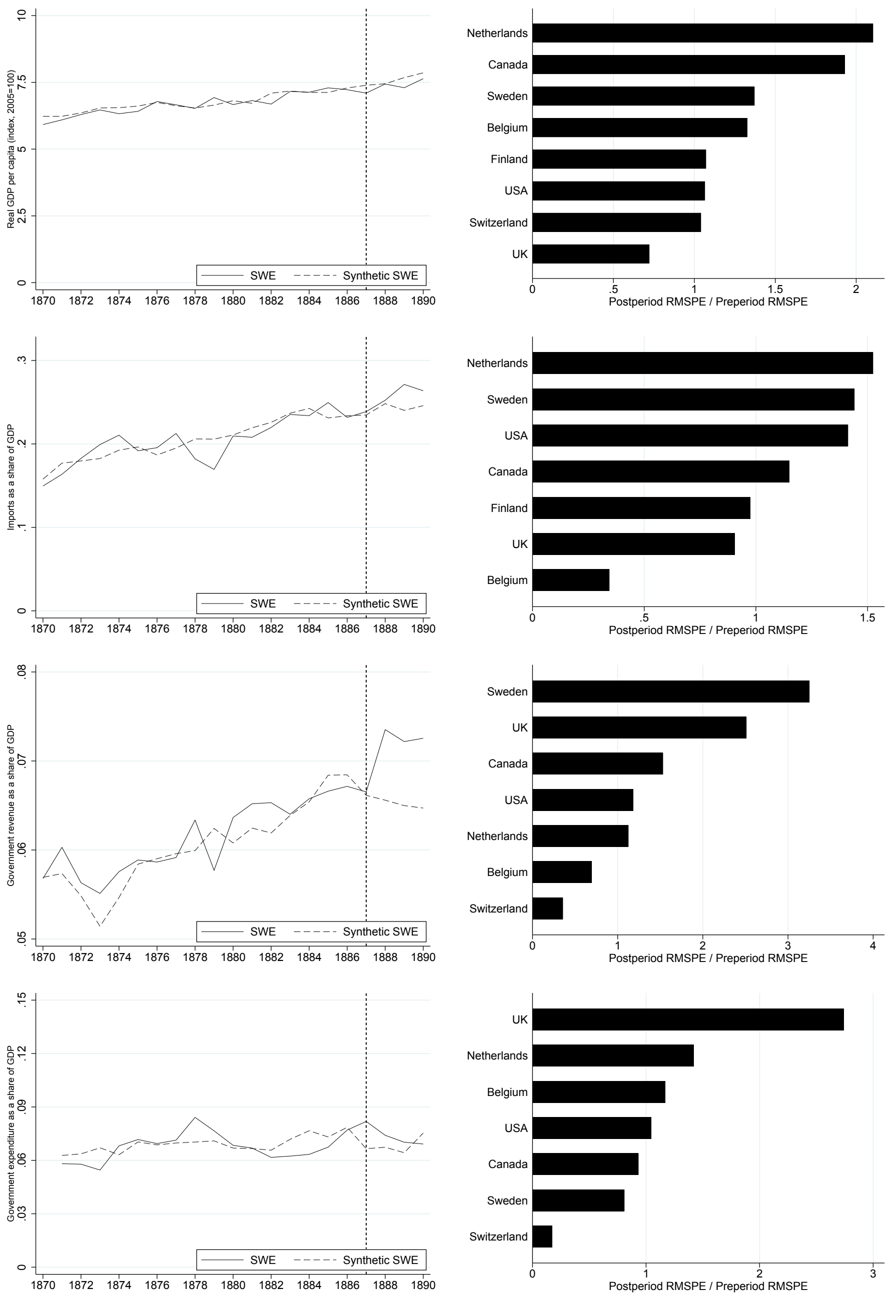


Notes: The left panel shows the results for government revenue for the placebo treatment in 1884 and excludes countries for which the pre-treatment MSPE is at least 10 times larger than Sweden's pre-treatment MSPE. The right panel shows the Swedish counterfactuals for government revenue iteratively excluding each country in the donor pool with positive weights. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

Second, we investigate whether an influential control country drives the estimated effect. Following Abadie et al. (2015), we re-estimate the effect by iteratively excluding from the donor pool countries with a positive SC weight. The right panel of Figure 1.6 shows the results. We find that the effect of protectionism on government revenue is not driven by any individual control country.

Third, we check for potential spillover effects from Sweden's tariff policy on trading partners. We exclude Denmark and Norway from the donor pool because they exported more than 10% of their exports to Sweden in 1887. Figure 1.7 shows that restricting the donor pool does not change the results.

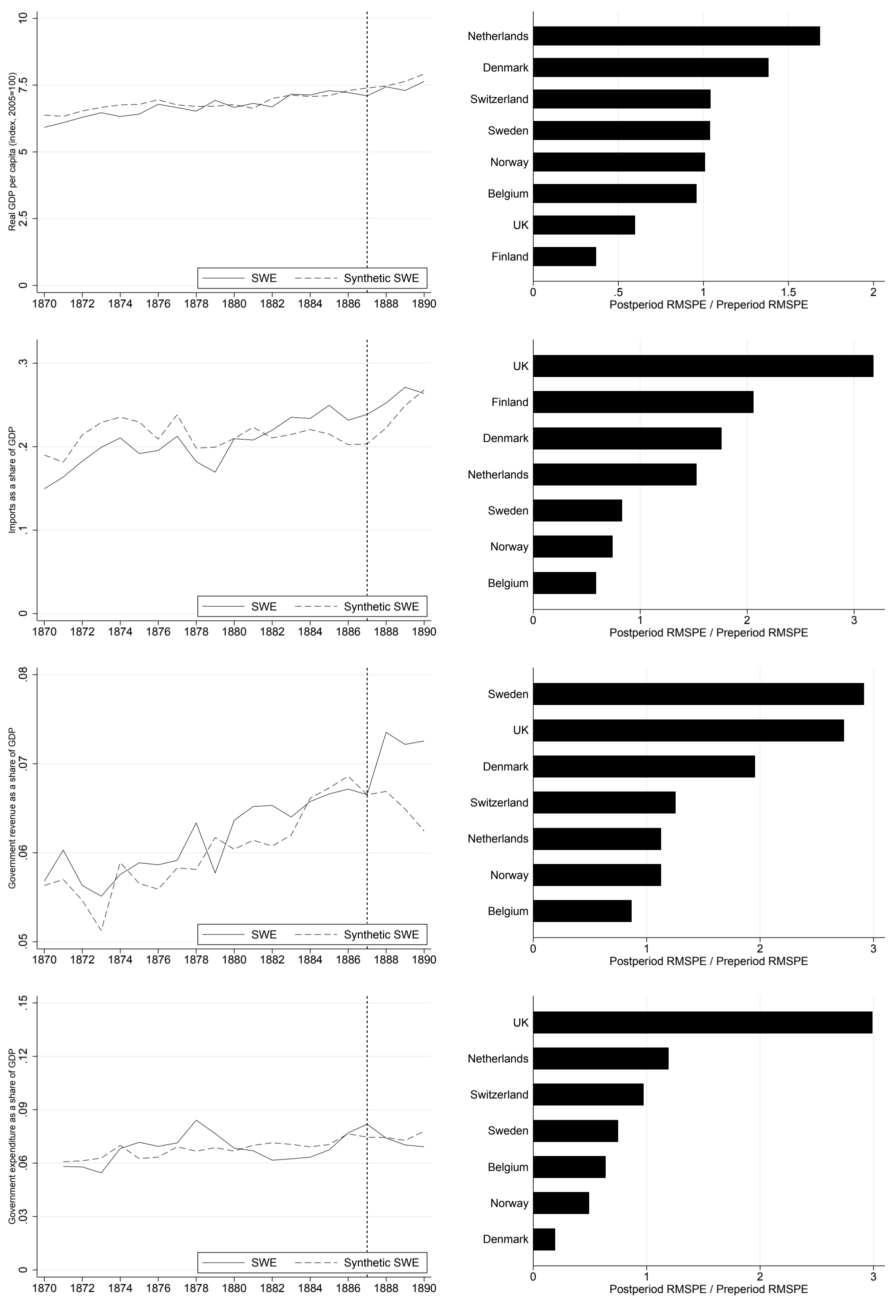
Figure 1.7 : Robustness: Excluding countries with >10% exports to Sweden in 1887



Notes: We exclude Denmark and Norway from the original donor pool. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

Fourth, we restrict the donor pool to European countries. Average tariffs between 1870 and 1890 were substantially higher in the labor-scarce, land-abundant United States and Canada than in the European countries (Lake, 1988; Irwin, 2002). Figure 1.8 shows that excluding Canada and the United States does not change inferences.

Figure 1.8 : Robustness: European countries only



Notes: We exclude Canada and the United States from the original donor pool. The data are from the Jordà-Schularick-Taylor Macrohistory Database.

Finally, we apply the conformal inference procedure of Chernozhukov et al. (2021) to test the null hypothesis that the protectionist policies had no effect on government revenue. This approach computes p-values by permuting SC residuals. We employ the two types of permutations recommended by Chernozhukov et al. (2021): iid and moving block permutations. The p-values are 0.01 for the iid permutations and 0.05 for the moving block permutations (the smallest possible p-value given the number of time periods). Thus, our results are robust to the choice of the inference procedure.

1.7 Conclusion

Previous studies did not explain the 'tariff-growth paradox' in the first era of globalization: protectionism has been shown to decrease economic growth in the second half of the 20th and 21st century, but tariffs and growth were positively correlated in the late 19th century and the early 20th century. We provide causal evidence on how protectionist policies influenced short-run economic growth in the late 19th century. We exploit an exogenous shock, unique in Sweden's history, that changed the parliamentary majority from free-trade to protectionist. The new protectionist government increased tariffs.

Using the SC method, we do not find evidence suggesting that the protectionist policies influenced short-run economic growth in late 19th century Sweden. An interesting question is why. The results suggest that the increased tariffs did not deter Sweden's trading partners from exporting goods to Sweden. The protectionist government increased revenue but refrained from stimulating the economy in the short run by increasing government expenditure. Instead, it used the increased government revenue to balance the budget.

More causal evidence is needed to better understand the 'tariff-growth paradox' in the first era of globalization. The short-run effects of protectionism are likely to be context-specific (Eichengreen, 2019). Empirical strategies to identify causal effects should also be employed to examine how individual tariffs (e.g., agricultural tariffs, industrial tariffs) influenced government revenue and growth (e.g., Lehmann and O'Rourke, 2011).

We examine how protectionism influences short-run economic growth. Our post-treatment period encompasses three years. A relatively short post-treatment period is well-suited for our purpose because other confounding events after treatment are unlikely to bias our results. However, our research design is not suitable to identify and estimate the long-run effects of protectionism on economic growth, and future research should investigate such long-run effects in the first era of globalization.

Our identification strategy to examine how protectionism influences economic growth is based on exploiting exogenous variation in a change of government which gave rise to a drastic change in trade policies. Future research investigating how economic policies influence economic outcomes might also use exogenous variation based on political events or shocks that influenced economic policies.

2 Read my lips? Taxes and elections*

2.1 Introduction

The theories of political business cycles describe that politicians implement expansionary fiscal policies before elections and postpone unpopular fiscal measures until after elections (Nordhaus, 1975; Rogoff and Sibert, 1988; Rogoff, 1990). Empirical evidence shows that election-motivated politicians increase public expenditure before elections[1], but little is known regarding electoral cycles in taxation. Previous studies have examined how electoral motives influence individual tax types at the local and sub-national level. There is no evidence, however, about heterogeneity in electoral cycles across tax types and, more generally, on electoral cycles in taxation at the national level. This lack of evidence is striking, as the key tax policy decisions are made on the national level in most countries and electoral motives are likely to affect tax decisions differently across tax types.

An important reason why there is little evidence on electoral cycles in taxation on the national level and across tax types is a lack of cross-nationally comparable data on tax changes. We introduce new harmonized indices on reforms of tax rates and bases for 23 advanced and emerging market economies, including granular sub-indices on six tax types between 1960 and 2014 based on extensive qualitative information collected by Amaglobeli et al. (2018). Our indices provide an encompassing overview of tax reforms and international trends in taxation over the past six decades. We use our indices to investigate electoral cycles in tax reforms and examine how these cycles change the composition of national tax systems.

Our main findings are as follows: Governments postpone tax rate increases until after elections. The overall tax reform index was around 0.24 standard deviations larger in post-election years than in other years. We also find substantial heterogeneity across tax types, indicating that electoral cycles influence the composition of national tax systems. Our main result is driven by post-election increases in value added and sales tax (VAT) rates and personal income tax (PIT) rates, which are particularly unpopular among voters. The heterogeneity in electoral cycles across tax types is in line with the argument of "tax salience", suggesting that voters are rationally ignorant about most tax measures and pay attention mostly to policies that directly

* This chapter is based on joint work with Clemens Fuest, Klaus Gründler, and Niklas Potrafke.

1 See, for example, Alesina et al. (1997), Alt and Lassen (2006), Shi and Svensson (2006), Potrafke (2010), Katsimi and Sarantidis (2012), Herwartz and Theilen (2014), and Castro and Martins (2018).

affect them (e.g., Finkelstein, 2009; Cabral and Hoxby, 2012).

Measuring tax reforms across countries and over time is a challenging endeavor because tax systems vary greatly across countries (Koester, 2009). While the general types of taxes are similar in the group of industrialized countries – including, for instance, PIT, corporate income taxes (CIT), VAT, and property taxes – tax rates, tax bases, and tax exemptions are subject to multifaceted provisions in the legal framework of countries. Hence, a simple comparison of tax rates gives rise to biased assessments when comparing cross-national differences in taxation. In an attempt to provide more granular information on national tax systems and to facilitate the comparison of tax systems across countries, the International Monetary Fund (IMF) compiled qualitative information on tax reforms in OECD countries (Amaglobeli et al., 2018). This information, provided in the form of text entries for countries and years, is based on more than 900 OECD Economic Surveys and 37,000 tax-related news from the International Bureau of Fiscal Documentation. The qualitative nature of the data allows scholars to learn about tax reforms since 1960 in detail, but it does not provide quantitative measures that are readily available for empirical research. In the first part of our paper, we use the qualitative data of the IMF to compile a new quantitative and cross-nationally harmonized dataset of tax reforms, the "Tax Reform Index" (TRI). Our new dataset covers indicators on tax reforms for tax rates and tax bases, along with detailed sub-indices for six types of taxes. These indicators are available for 23 advanced and emerging market economies over the period 1960–2014.

Based on our indices, we document cross-national trends in taxation since early 1960, allowing us to uncover a more detailed picture of international trends in taxation than based on previous datasets, which typically include data only for individual types of taxation. Our data points to a decline in CIT and PIT rates across countries since the early 1980s, and a stark increase in VAT rates and excise tax rates since the mid-1970s. We also observe an increase in social security contributions (SSC) since the early 1970s, but no distinct patterns regarding property tax rates. Similarly, our tax reform indicators show that tax bases of the CIT and PIT have been narrowed, while tax bases of the VAT and excises have been broadened. Examining trends in tax burdens via our aggregated measures, we find a hump-shape development for tax rates, which started to increase in the early 1980s and decreased since the millennium. Tax bases, in contrast, have been steadily narrowed over time since the 1970s.

In the second part of our paper, we relate our indicators on tax reforms to the timing of elections. Our main result, which is stable across alternative specifications, shows that politicians increased tax rates after elections. Our new indicators also allow us to provide first evidence on heterogeneity in electoral cycles across individual types of taxes. We find that electoral cycles are particularly pronounced for VAT rates and PIT rates. Against the background that these types of taxes directly influence voters' disposable income, the results for individual tax types corroborate the political business cycle theories.

Our results are unaffected by government changes and other national fiscal policy measures, e.g., changes in expenditure or debt around elections. Inferences also do not change when we disentangle post-electoral tax reforms of re-elected incumbents and newly elected governments. A key question is whether tax increases have been announced prior to elections, or whether they caught voters by surprise. The theory of rational political business cycles predicts that incumbents signal their fiscal competence prior to elections, which would go against electoral promises to increase taxes after elections. An announcement of tax increases would most likely also reduce parties' electoral success. In any event, to address possible anticipation effects, we collect data on tax queries via Google in the countries included in our sample and compare the popularity of tax issues in election years with other years. While tax issues are less popular in election years, the differences in means do not turn out to be statistically significant. Taken together, Google Trends data provides suggestive evidence that goes against pre-election hikes in public attention to tax policies. This result suggests that electoral cycles in taxation are driven by strategic tax reforms of election-motivated politicians.

Contribution to the literature: Our main contribution is to provide harmonized tax reform indicators for six tax types that allow us to study trends in taxation over almost six decades. Relating tax reforms to national elections, we find pronounced political cycles in tax reforms. Our results relate to the empirical literature on political business and budget cycles.[2] Some studies specifically focus on electoral motives in tax policies, examining electoral cycles in tax revenue at the national level and tax rates on the local level (Erhart, 2013; Foremny and Riedel, 2014; Alesina and Paradisi, 2017; Sances, 2017; Lami and Imami, 2019). Little is known, however, about heterogeneity in electoral cycles across types of taxes and, more broadly, on electoral cycles in overall tax reforms on the national level. Our study fills this gap and contributes to the literature in two ways. First, we show that postponing tax increases until after elections is a global phenomenon and not confined to individual federal states. Given

[2] (e.g., Ben-Porath, 1975; Schuknecht, 1996; Bloomberg and Hess, 2003; Shi and Svensson, 2006; Desai et al., 2007; Potrafke, 2010; Aidt et al., 2011; Brender and Drazen, 2013; De Haan and Klomp, 2013; Aidt and Mooney, 2014; Foremny and Riedel, 2014; Klomp and De Haan, 2016; Baskaran et al., 2015; Dubois, 2016; Bostashvili and Ujhelyi, 2019; Aidt et al., 2020; Potrafke, 2020; Bohn and Sturm, 2021).

that the most important tax choices in many countries are made on the national level, our results show that electoral motives have a substantial impact on national tax systems. Second, we provide first evidence on heterogeneity in electoral cycles across individual types of taxes. The analysis uncovers substantial heterogeneity in electoral cycles across tax types suggesting that studies focusing on single tax types are not suitable to describe how electoral motives influence tax policies overall.

We also relate to the literature showing that voters tend to discount the past. When evaluating politicians based on their economic performance, voters seem to consider only the previous one or two years (Fair, 1978, 1982, 1988, 2009). Our results suggest that election-motivated politicians tend to take strategic account of voters' ignorance of the past by postponing tax increases until after elections so that voters are likely to have forgotten the reform when the next election approaches. There is also evidence that this strategic behavior pays off. Previous work has shown that tax reforms impact re-election probabilities of incumbent governments (Chen et al., 2019). Focusing on tax reforms intended to consolidate the budget, the results show that reforms of indirect taxes tend to have higher electoral costs than reforms of direct taxes. Electoral costs also tend to be lower for leftwing than rightwing governments.

We also contribute to the literature on trends in taxation in industrialized countries. So far, most studies investigated reforms in CIT and tax competition (Devereux and Griffith, 1998; Devereux et al., 2002; Devereux and Griffith, 2003; Devereux et al., 2008; Arulampalam et al., 2012; Becker et al., 2012; Clausing, 2013; Kawano and Slemrod, 2016; Fuest et al., 2018). Comparable data on tax reforms, particularly for individual tax types and tax bases, has been heavily limited across countries and years. Our new indices on tax reforms are well suited to examine how tax systems have been reformed over the past six decades in industrialized countries and may also be useful for scholars to address other research questions on the causes and consequences of tax reforms.

2.2 Measuring tax reforms

Several researchers have compiled datasets on taxes and tax reforms. The OECD Tax Database provides comparative information on major taxes for OECD and inclusive framework countries (OECD, 2021). However, the database has two shortcomings: (i) the tax base is missing for most taxes and (ii) coverage is limited regarding the time dimension, including information only for the post-2000 period. The University of Michigan World Tax Database collects mostly

PIT and CIT rates for OECD countries from 1960 (PIT) and 1980 (CIT) until the 2000s.[3] The Oxford University Centre for Business Taxation (CBT) Tax Database covers CIT bases and rates for all OECD countries from 1980 to 2017. Although these datasets have set milestones in the analyses of taxes and tax reforms, we are not aware of any tax database that provides comparable data on tax rates and bases for a large sample of countries, tax types, and years.

2.2.1 Measuring tax reforms across countries

Comparing tax systems across countries is notoriously difficult. Tax systems are complex and their economic impact depends on tax rates, tax bases, administrative practices, fines for tax evasion, and many other institutional details. Empirical research often needs summary measures, which allow for a simple comparison of tax burdens across countries. Which type of summary measure is appropriate depends on the question asked. One example is the international comparison of the corporate tax burden on investment, where King and Fullerton (1984) introduced the concept of the effective marginal tax rate. It measures how the combination of tax rates and tax bases distorts marginal investment in a country. Other examples with a different focus include the concept of effective average tax rate introduced by Devereux and Griffith (2003) or the labor income tax wedge published regularly by the OECD (see e.g., OECD, 2020).

Our analysis focuses on tax policy changes before and after elections. Here the challenge is that we cannot focus on individual taxes only or on particular economic distortions caused by the tax system. We are interested in changes rather than levels of taxation and in principle all taxes are relevant. Hence, our key objective is to represent the entire tax system and its composition at the national level. We are also interested in the magnitude of the tax changes and their significance. At the same time, we need a measure that is simple enough to be suitable for the empirical analysis in a panel of countries. To strike a balance between these objectives we develop a new index for the international comparison of tax reforms based on qualitative information on tax reforms provided by the IMF (Amaglobeli et al., 2018).

2.2.2 Collecting qualitative information on tax reforms

The key requirement of our approach is collecting detailed qualitative information about reforms of tax rates and tax bases for as many countries, years, and tax types as possible. Until recently, detailed cross-country information on the nature of tax reforms was difficult to access. The availability of qualitative information on tax reforms was drastically improved

[3] The data are no longer updated. All previously collected data are available here: `https://www.bus.umich.edu/otpr/otpr/default.asp`.

by the launch of the IMF's Tax Policy Reform Database (TPRD), a comprehensive database of tax policy measures, which includes 23 advanced and emerging market economies observed between 1930 and 2014 (Amaglobeli et al., 2018). The 23 countries are Australia, Austria, Brazil, Canada, China, the Czech Republic, Denmark, France, Germany, Greece, India, Ireland, Italy, Japan, Luxembourg, Mexico, Poland, Portugal, Spain, South Korea, Turkey, the United Kingdom, and the United States. The dataset covers detailed qualitative information on tax reforms using more than 900 OECD Economic Surveys and 37,000 tax-related news from the International Bureau of Fiscal Documentation.

The innovation of the TPRD is the systematic and granular documentation of the direction of tax rate and tax base changes, along with a qualitative assessment of the IMF on whether a reform has been "major" or "minor". Information is separately available for six tax types: personal (PIT) and corporate (CIT) income taxes, value added and sales taxes (VAT), social security contributions (SSC), excises (EXE), and property taxes (PRO).[4] The dataset also includes information on the announcement and implementation dates of tax reforms. The major advantage of the TPRD is its broad coverage, including qualitative information in areas (e.g., countries, years, tax types, tax bases) for which no time-varying policy indices existed before.

2.2.3 Quantifying tax reforms: The Tax Reform Index (TRI)

While the IMF's Tax Reform Database is unparalleled in coverage and detail, its broad qualitative information cannot be readily used in empirical analyses that seek to quantify the causes and consequences of tax reforms. To provide a readily available index, we transfer the qualitative information into a quantitative index of tax reforms. Our index, the "Tax Reform Index" (TRI), provides cross-nationally harmonized measures of tax reforms that are available for 23 countries over the period 1960–2014.[5] We introduce 14 indices for each country, reflecting reforms of tax rates and tax bases for each of the six tax types included in the IMF's Tax Reform Database. We also combine these sub-indices into an aggregate index of tax rate and tax base reforms that measure the extent of reform of the overall tax system.

Let Δs^r_{it} be the change in the tax rate for tax type r, adopted and announced in country i at time t.[6] Consider further that $|\tilde{s}_{it}|$ is the qualitative information included in the IMF's Tax

[4] We include SSC when discussing tax types because of convenience and clarity.
[5] While the TPRD includes observations since 1930, data on pre-1960 periods is available only for few countries and with large gaps.
[6] A key question is to what time period a tax reform should be assigned. The IMF Tax Reform Database includes information on both the announcement year and the implementation year. In most cases, these years are

Reform Database about the strength of a reform (“major” or “minor”). Our tax reform index $\mathfrak{S}^r_{it} \in [-2, +2]$ is defined as

$$\mathfrak{S}^r_{it} = \begin{cases} -2, & \text{if } \Delta s^r_{it} < 0 \text{ and } |\tilde{s}_{it}| = \text{“major”} \\ -1, & \text{if } \Delta s^r_{it} < 0 \text{ and } |\tilde{s}_{it}| = \text{“minor”} \\ \pm 0, & \text{if } \Delta s^r_{it} = 0 \\ +1, & \text{if } \Delta s^r_{it} > 0 \text{ and } |\tilde{s}_{it}| = \text{“minor”} \\ +2, & \text{if } \Delta s^r_{it} > 0 \text{ and } |\tilde{s}_{it}| = \text{“major”} \end{cases} . \tag{2.1}$$

This index provides an intuitive interpretation. The index assumes a value of +1 when the tax reform led to a minor increase in tax rates, and a value of +2 when there has been a major increase in tax rates. For tax reforms that decreased tax rates, the index assumes the value of -1 for a minor decrease and -2 for a major decrease. The advantage of this coding is that it makes tax reforms more comparable than simply comparing tax rates. Under a given legal tax framework, an increase of e.g., two percentage points may either be a minor increase or a major increase. The classification of the extent of the reform requires additional information on legal provisions of a tax system, which are accounted for by the IMF’s qualitative assessment.

We also code two minor increases as being equivalent to one major increase. The rationale for this strategy is that two minor increases of the same tax type in the same year have the same reform character as one major increase. Clearly, such a coding rule reflects views on the nature of tax reforms, and researchers may have diverging views about the relative importance of several minor increases compared to one major increase. To address this concern, our indices come in three variants, which differ in the normalization rule applied to measure the extent of tax reforms. Depending on their research question, scholars will find either the normalized, the reduced or the non-normalized version better suited to match their purposes. The reduced variant codes each reform as a single tax change with equal weights, regardless of the scope of the reform or whether there are multiple reforms in a given year. The scale of this version is $[-1, +1]$. The non-normalized versions put no constraints on the upper and lower bounds, adding multiple minor (+1) and major (+2) increases that occurred in the same period. Hence, compared with our normalized benchmark index $\mathfrak{S}^r_{it} \in [-2, +2]$ defined in Equation 2.1, the

identical, but we also observe differences between the two. We base our analysis on the announcement year to avoid distorting anticipation effects. Also, the announcement data is useful for examining how election-motivated politicians influence tax policies.

non-normalized variant $\mathfrak{S}_{it}^{r} \in \mathbb{Z}$ is more volatile. While this version offers greater scope to account for extreme reforms, it bears the risk that results in empirical estimations are driven by extreme outliers. The versions also differ in their definition of the very nature of a tax reform. When the research question is whether or not there has been a (major) tax reform in a given year, the normalized version is better suited for empirical work. Instead, when the research question requires having estimates for the extent of multiple reforms, the non-normalized version is the better-suited alternative.

In principle, there may be reforms of several tax types in a given year, and these reforms may not be independent. Hence, for some empirical analyses, researchers might be interested in the extent to which the overall tax system has been reformed. We built our composite tax reform index $\mathfrak{S}_{it}^{A} \in [-2, +2]$ for country i in year t as a simple average of the six sub-indices via

$$\mathfrak{S}_{it}^{A} = \frac{1}{6}\sum_{r=1}^{6} \mathfrak{S}_{it}^{r} \quad , \tag{2.2}$$

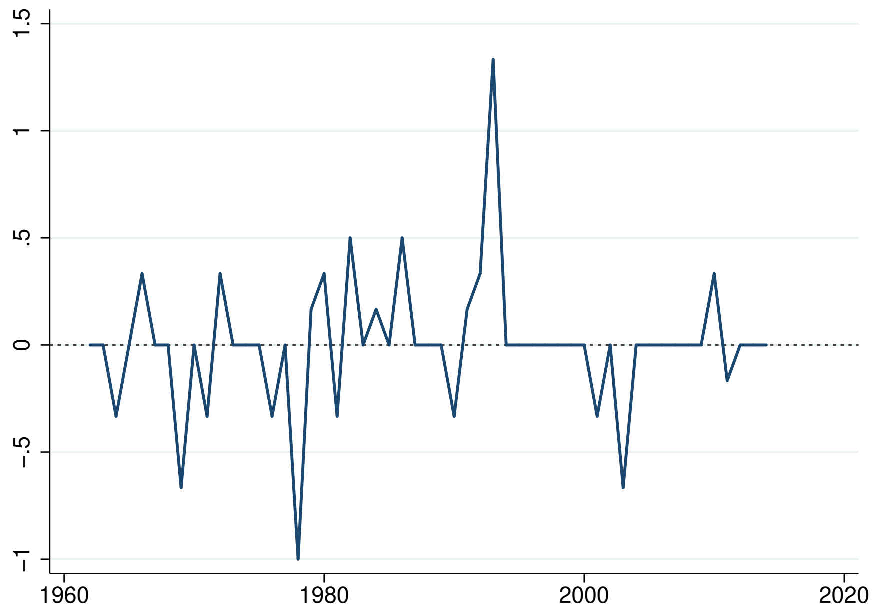
where $r = 1, \ldots, 6$ denote the six tax types. Again, there might be good arguments to scale the final index on the interval $[-2, +2]$, to code a reduced version irrespective of the scope of reforms, or to allow the index not to be confined by a pre-defined interval. We provide the two versions of the aggregate index, but we believe that the constrained index is more suitable for our research question. We apply the same logic to obtain indices to measure changes in tax bases.

Assessing trends in taxation: Our indices reflect reforms of the tax system, and may hence be interpreted as the *change* of the tax burden at a given point in time. For many analyses, however, researchers might be interested in international trends in taxation. Such trends can be obtained by accumulating the index values $\mathfrak{S}_{it}^{r}$ over time

$$\mathfrak{S}_{iT'}^{r} = \sum_{t=1960}^{T'} \mathfrak{S}_{it}^{r} \quad , \tag{2.3}$$

where $\mathfrak{S}_{iT'}^{r}$ is the accumulated index value at time T'. This index value shows the tax burden of a tax system relative to the base year 1960. The accumulated version also allows comparisons of trends between countries, as all accumulated indices are, by construction, indexed to $1960 = 0$.

Figure 2.1 : Illustration of the Tax Reform Index (TRI): Tax reforms and trends in taxation in the United States, tax rates, 1960–2014



(a) Aggregate TRI for the United States

(b) Aggregate TRI for the United States, accumulated over time

Notes: The figure illustrates the aggregate Tax Reform Index $\mathfrak{S}^{A}_{it}$ (tax rates) for the United States (Panel A) and the accumulated version of this index $\mathfrak{S}^{r}_{iT'}$ (Panel B) to assess trends over time. For the accumulated version, each point in time $T^{'}$ represents the sum of the Tax Reform Index $\mathfrak{S}^{A}_{it}$ over all available periods prior to $T^{'}$.

Example – Tax reforms and trends in the United States, 1960–2014: Figure 2.1 shows the Tax Reform Index, plotting the aggregate Tax Reform Index $\mathfrak{S}_{it}^{A}$ for the United States (upper panel) and the accumulated version of the index $\mathfrak{S}_{iT'}^{r}$ (lower panel). This example shows the logic of our index and compares the Tax Reform Index to the time-accumulated version of the index that allows for trends in taxation over time.

By construction, the Tax Reform Index oscillates around the zero line, pointing to reforms in the US tax system (upper panel). Cumulating these changes over time, we arrive at trends in taxation (lower panel). We observe that the Tax Reform Index decreases at the beginning of the observation period, which indicates a negative trend in the tax burden during the 1970s. This trend reversed during the 1990s when the US tax system became more contractionary than in 1960.

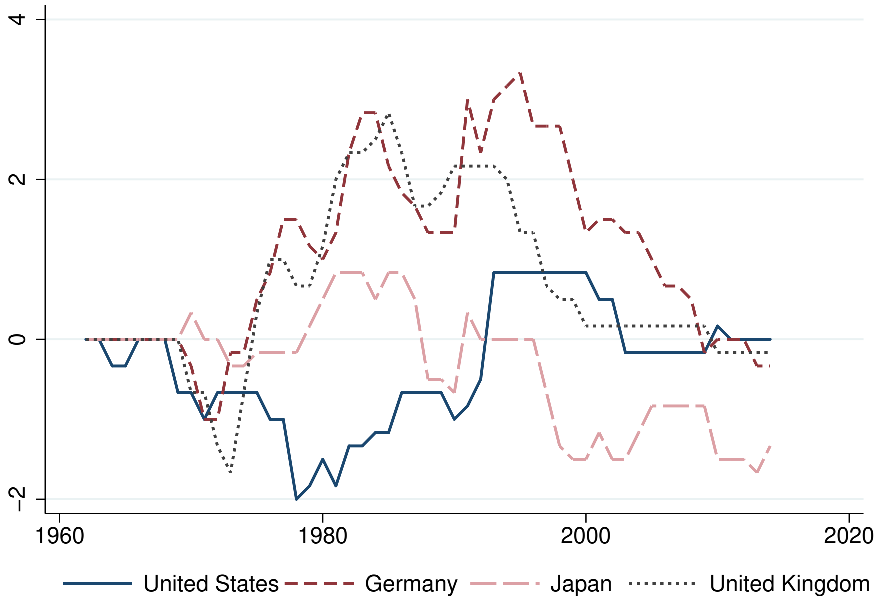
2.2.4 International trends in taxation: A comprehensive picture

Previous tax indices were mainly confined to information on tax rates for PIT and CIT, and were heavily limited in the included number of countries and years. Using our new indices on tax reforms, we portray cross-national trends in taxation from the early 1960s to the mid-2010s.

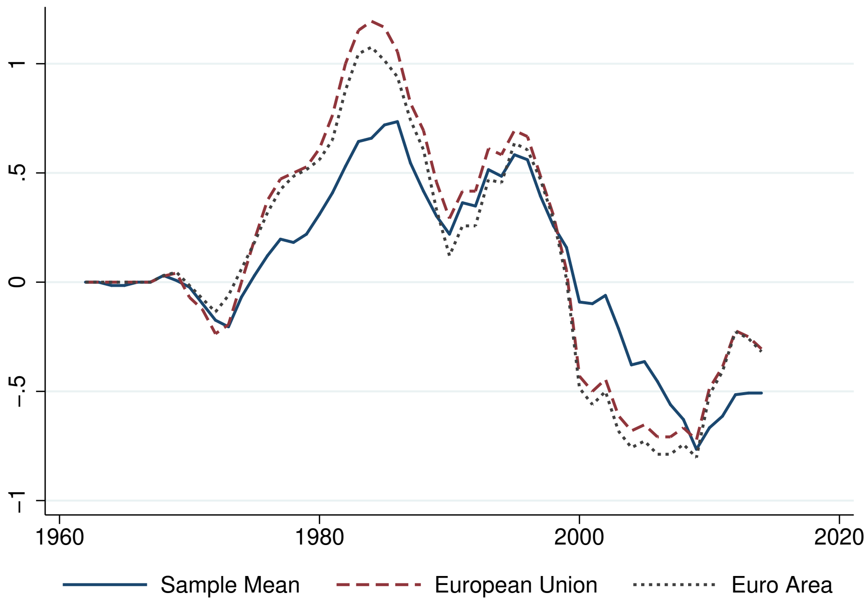
Trends in aggregate tax rates: Figure 2.2 compares trends in taxation in individual countries (Panel A) and for groups of countries (Panel B). The figure shows that there is heterogeneity in how tax systems have developed across the United States, Germany, Japan, and the United Kingdom. The average Tax Reform Index for tax rates in the United States decreased during the 1970s, remained at a low level compared to other advanced economies during the Reagan administration in the 1980s, and approximated its initial level in the 1990s during the presidency of Bill Clinton. The average Tax Reform Index for tax rates started to decrease again under George Bush and remained on a quite constant level since. In contrast, we observe a positive trend in the average Tax Reform Index for tax rates in Germany and the United Kingdom from the 1960s until the mid-1990s, and a substantial decline that lasts until the end of our sample period. Trends in Japan are similar, but increases and decreases are less pronounced.

Despite substantial heterogeneity in the development of tax rates across countries, we observe distinct trends in taxation when examining sample means and averages of the Tax Reform Indices across the whole sample, the European Union, and the Euro Area (Panel B). After a slight decline in the 1960s, the average Tax Reform Index for tax rates increased during the oil crises in the 1970s and reached its peak in the mid-1980s. After a substantial decline in the

Figure 2.2 : Trends in tax rates, selected countries and samples, 1960–2014



(a) Trends in taxation: The United States, Germany, Japan, the United Kingdom



(b) Trends in taxation: Sample mean, the European Union, and the Euro Area

Notes: The figure illustrates the accumulated version of the aggregate Tax Reform Index ($\mathfrak{S}^{A}_{iT'}$) for tax rates to compare trends in taxation between the United States, Germany, Japan, and the United Kingdom (Panel A), and the sample mean, the European Union, and the Euro Area (Panel B) over time. For the accumulated version, each point in time $T^{'}$ represents the sum of the Tax Reform Index $\mathfrak{S}^{A}_{it}$ over all available periods prior to $T^{'}$.

second half of the 1980s, the average Tax Reform Index for tax rates increased again during the early 1990s. From the late 1990s until the Financial Crises of 2007–2008, the average Tax Reform Index for tax rates decreased substantially. The average Tax Reform Index for tax rates started increasing again after the Great Recession initiated by the Financial Crisis.

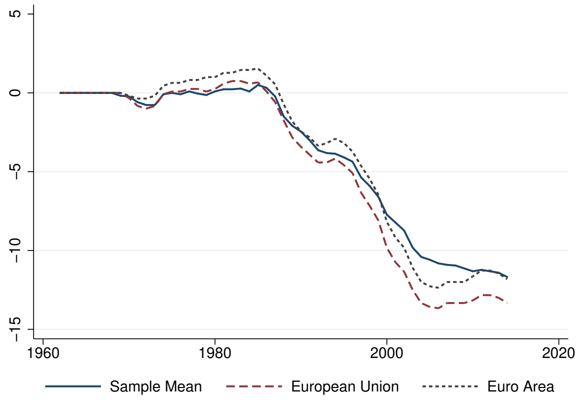
Trends in the composition of tax systems: Our aggregate tax index adds sub-indices for tax types using equal weights. A major advantage of our dataset is that it includes separate indices for six major types of taxation. There may well be heterogeneity in how tax rates for individual tax types have developed. Figure 2.3 shows trends in taxation for individual tax types considering the sample mean, the European Union, and the Euro Area. The figures indicate that there has been a major change in the composition of tax systems and clear differential trends between tax types. Starting in the 1980s, there has been a substantial decline in the Tax Reform Index for CIT and PIT rates. This decline is consistent with trends reported for CIT rates in prior studies (e.g., Devereux et al., 2008; Heinemann et al., 2010). The United States is a prime example of the decrease in PIT rates during the 1980s. President Ronald Reagan reduced the top income-tax rate from about 70% when he entered office in the early 1980s to 28% in 1986 (e.g., Souleles, 2002).

The decrease in income tax rates was compensated by a stark increase in tax rates for VAT and excises. While there is a substantial increase in the Tax Reform Index for both tax types observable for the whole sample and for European countries, the rise in VAT rates after the Financial Crises was particularly strong in the European Union and the Euro Area. In a similar vein, SSC increased between the early 1980s and the early 2000s, but there are diverging trends between the sample mean and European countries since the turn of the millennium. There are no distinct trends regarding property tax rates.

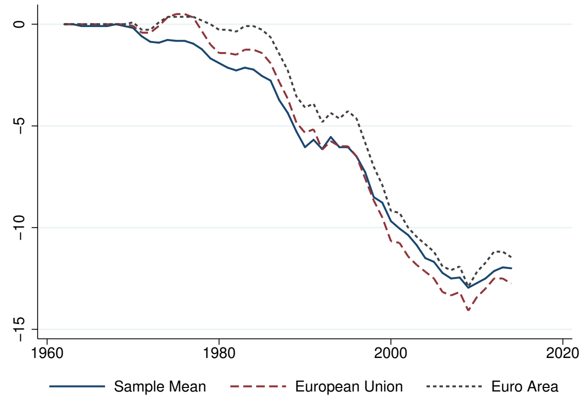
Trends in tax bases: Figure B.1 in the Appendix shows trends in taxation for tax bases. Panel A shows that tax bases have become smaller in the United States, Germany, Japan, and the United Kingdom. This development was particularly pronounced in the United Kingdom and less so in Japan. Panel B considers trends of the whole sample, the European Union, and the Euro Area. For all groups of countries, we observe clear trends toward more narrow tax bases.

Similar to the results for tax rates, the composition of tax systems regarding tax bases changed over time. While tax bases became smaller, particularly for PIT and CIT, tax bases broadened for VAT and excises, mirroring the development of tax rates. Again, we observe no distinct trends for property taxes. Tax bases underlying SSC narrowed, constituting the only tax type for which tax bases and tax rates have developed differently.

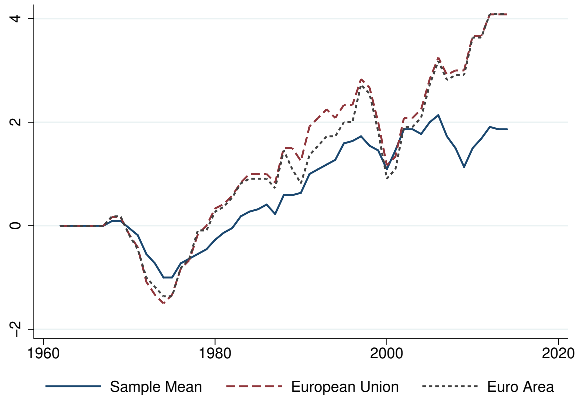
Figure 2.3 : Trends in individual tax types, tax rates, 1960–2014



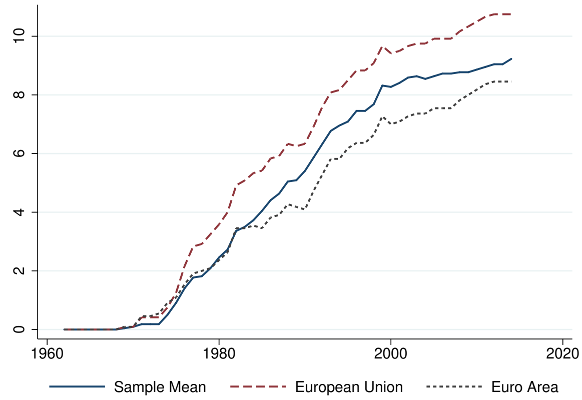
(a) **Corporate tax rates**



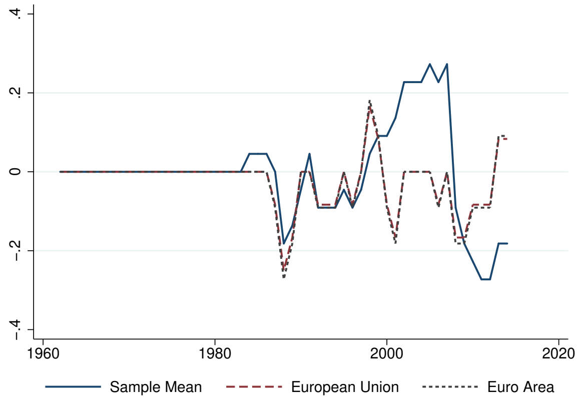
(b) **Personal income tax rates**



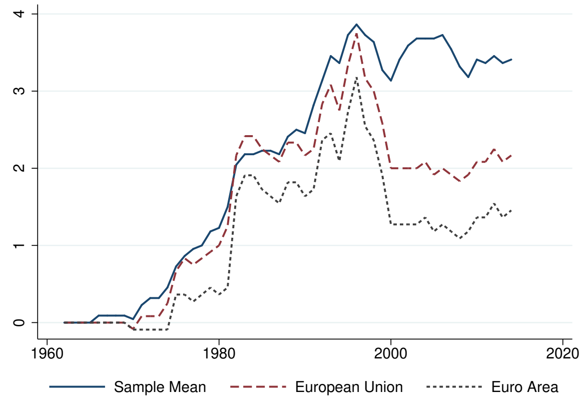
(c) **Value added tax rates**



(d) **Excise tax rates**



(e) **Property tax rates**



(f) **Social security contributions**

Notes: The figure illustrates the accumulated version of the Tax Reform Index ($\mathfrak{S}^{A}_{iT'}$) for individual tax types to compare trends in taxation between the sample mean, the European Union, and the Euro Area over time. For the accumulated version, each point in time $T^{'}$ represents the sum of the Tax Reform Index $\mathfrak{S}^{A}_{it}$ over all available periods prior to $T^{'}$.

2.3 Changes in taxation after elections

2.3.1 Hypotheses

The theories on political business cycles describe that politicians implement expansionary policies such as increasing public expenditure before elections to maximize electoral success (Nordhaus, 1975; Rogoff and Sibert, 1988; Rogoff, 1990). The traditional theories on political business cycles focused on demand-increasing policy measures that are conducted prior to elections to boost the economy, affecting macroeconomic variables such as GDP growth, unemployment, and inflation (Nordhaus, 1975; Lindbeck, 1976). Evidence for political business cycles in macroeconomic variables is, however, mixed (Alesina et al., 1997; Potrafke, 2012; Drazen, 2018). In a similar vein, the traditional models of political business cycles have been criticized for the assumption of non-rational and myopic voters. A subsequent generation of political business cycle models abandoned the assumption of irrationality and focused on information asymmetry between politicians and voters instead (rational political business cycle). These models examine fiscal choices in a model in which politicians signal their competence via fiscal policies, resulting in a distortion of fiscal policies around elections (e.g., Rogoff and Sibert, 1988; Rogoff, 1990; Persson and Tabellini, 2002). The key difference compared to the traditional political business cycle models is that the rational political business cycle theories predict distortions in fiscal policies (e.g., spending, revenues, and deficits) rather than in macroeconomic indicators around elections. Signaling competence in administering the production of public goods, as in the model of Rogoff (1990), requires supplying a higher level of public goods at constant levels of taxation or by implementing low-tax policies for a given public good provision.[7] The latter strategy is however costly for incumbents because it reduces their fiscal scope when they get re-elected.

Beyond signaling, a key motivation of fiscal policy before elections is to take measures that are valued by the electorate to increase the incumbents' likelihood of re-election. Increases in taxes are particularly unpopular with voters because tax increases affect citizens' after-tax income. Politicians are hence most likely to conduct tax reforms when they are least likely to pay the electoral cost of such reforms. The literature on "political opportunity" is based on the observation that unpopular actions in non-election years are heavily discounted by voters over time (Fair, 1978, 1982, 1988, 2009; Berry and Berry, 1994; Nelson, 2000) and argues that tax reforms are most likely to be conducted directly after elections.

If electoral incentives are relevant for political decision-making, we expect incumbents to signal their competence to the electorate prior to elections by keeping taxes low, for a given

[7] On the signaling process see, for example, Garcia and Hayo (2021).

amount of public good provision. Following this line of argument, we expect a reduced probability of tax increases prior to elections and a higher probability of tax increases after elections. This electoral cycle in taxation is reinforced if the opposition party wins the election. The newly elected government can justify raising taxes by claiming that the preceding government did some window dressing about the state of public finances. Such claims may be used strategically but they may also be objectively true when the preceding government, aware of a high probability of losing the election, conducted expansionary fiscal policies to reduce fiscal space for the next government (e.g., Persson and Svensson, 1989).

Taken together, our hypotheses to be tested empirically are:

Hypothesis 1. *Tax rates increase after elections.*

Hypothesis 2. *Tax bases expand after elections.*

Some authors argue that salience plays a role, suggesting that voters are rationally ignorant about most tax policy issues and pay attention only to policies that affect them strongly or are easy to understand (e.g., Cabral and Hoxby, 2012; Finkelstein, 2009). Following the argument of “salience”, we expect that electoral cycles in taxation are more pronounced regarding tax types that many voters directly notice. Such tax types include the VAT, which directly increases consumer prices, and the PIT, which influences the disposable incomes of a large fraction of the population. We expect such electoral cycles to be less pronounced regarding tax types that affect a lower fraction of the electorate, such as CIT or property taxes.

2.3.2 Stylized facts and case studies

We discuss case studies of three tax reforms that are in line with our theoretical hypotheses and describe how we coded the reforms in our harmonized index. We then investigate the degree to which the aforementioned case studies are representative of tax reforms around elections by comparing sample means of tax reforms in election years, pre-election years, and post-election years.

Italy 1977: The Italian center-right minority government announced a major tax reform on 1 February 1977. Only seven days later, on 8 February 1977, the standard VAT rate was increased by two percentage points from 12% to 14%. An extra bracket was introduced for the reduced VAT rate which was taxable at 12%.[8] The increased tax rate was further increased by five

[8] The total number of tax brackets for the reduced VAT rate was four before the reform. Tax rates of the brackets

percentage points from 30% to 35% (European Commission, 2020b).

The IMF Tax Reform Database records the announced change in the Italian VAT rate as a major increase. Therefore, the index for the VAT assumes the value two for Italy for the year 1977.

United States 1993: The 103rd US Congress enacted the Tax Reform Act of 1993 and President Bill Clinton signed it into law. The tax reform contained major provisions for individuals and companies. The most substantial changes concerned the PIT and CIT rates. The Tax Reform Act of 1993 increased the PIT rate by five percentage points from 31% to 36% for taxable income between 140,000 and 250,000 USD and by 8.6 percentage points to 39.6% for income over 250,000 USD (Feldstein, 1995). It also increased the CIT rate for corporate income over 10 million USD: the tax rate on income between 10 and 15 million USD increased by one percentage point from 34% to 35%, the tax rate on income between 15 and 18.33 million USD increased by three percentage points from 34% to 38%, and the tax rate on income over 18.33 million USD increased by one percentage point from 34% to 35% (Taylor, 2003).

The IMF Tax Reform Dataset records the announced changes in the US PIT and CIT rates as major increases. Therefore, the tax reform indices for the PIT and the CIT assume the value two for the United States for the year 1993.

Germany 2006: On 19 May 2006, the German parliament (*Deutscher Bundestag*) voted for the largest tax increase since 1949 (Spiegel, 2006). The Christian conservatives (*CDU & CSU*) and the social democrats (*SPD*) jointly agreed to increase the VAT rate by three percentage points from 16% to 19% starting 1 January 2007.[9]

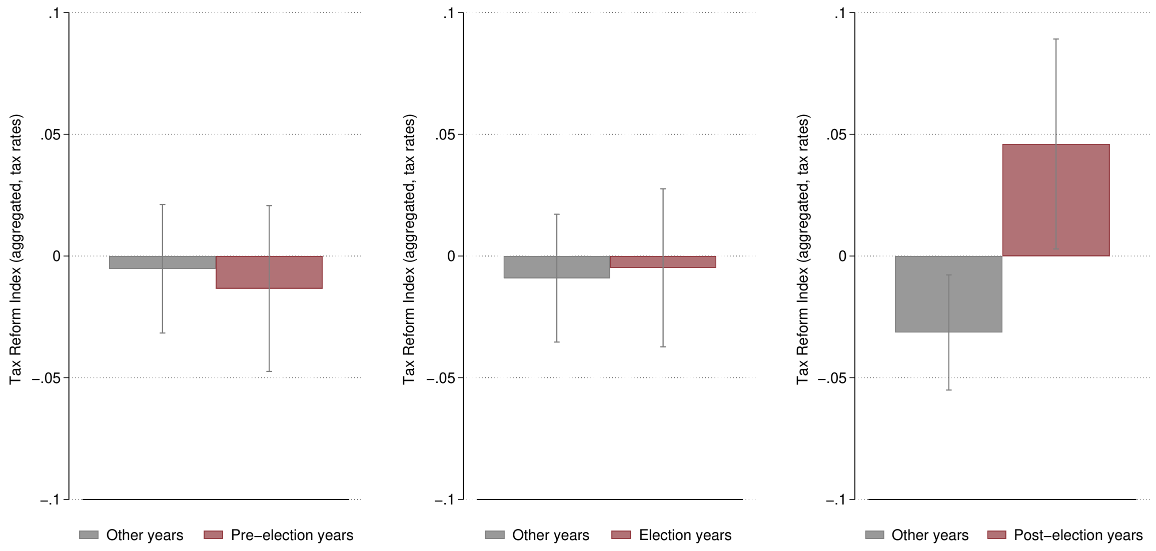
During the 2005 election campaign, CDU candidate Angela Merkel proposed a two percentage points increase in the VAT rate. The SPD promised that they would not support any VAT rate increase and campaigned with the now infamous slogan: *“Mehrwertsteuer, das wird teuer.” (Engl.: “VAT rate [increase] will be expensive”)*. The “compromise” was a VAT rate increase by three percentage points.

The IMF Tax Reform Database records the announced change in the German VAT rate as a major increase. Therefore, the tax reform index for the VAT assumes the value two for Germany for the year 2006.

were 1%, 3%, 6%, and 9%.

[9] The reduced VAT rate increased by two percentage points from 5% to 7%.

Figure 2.4 : Changes in tax rates, aggregate TRI, OECD countries, 1960–2014



(a) **Year before the election** **(b)** **Election year** **(c)** **Year after the election**

Notes: The figure shows changes in tax rates implied by the aggregate Tax Reform Index ($\mathfrak{S}^{A}_{iT'}$) in election years and non-election years (see Section 2.2 for a detailed description of the index). When comparing changes in tax rates in the pre-election year (Panel A), the label “Pre-election years” refers to the year prior to an election, while “Other years” refers to all other years. When comparing changes in tax rates in the election year (Panel B), the label “Election years” refers to the election year, while “Other years” refers to non-election years. When comparing changes in tax rates in the post-election year (Panel C), the label “Post-election years” refers to the year after an election, while “Other years” refers to all other years. Vertical lines indicate 95% confidence intervals. All figures use the full set of observations for countries and years included in our dataset.

Comparing sample means: The case studies are expository examples of tax changes after elections. To provide a more general overview of the unconditional correlation between tax changes and election dates, Figure 2.4 shows the overall Tax Reform Index for tax rates in election and non-election years (left panel), post-election and no post-election years (center panel), and pre-election and no pre-election years (right panel). The figure uses data for our baseline sample of OECD counties with established democracies. The difference in the overall Tax Reform Index for tax rates in election years (-0.005) and non-election years (-0.009) lacks statistical significance.

The results suggest that increases in tax rates were postponed until after elections: in post-election years, the overall Tax Reform Index for tax rates was 0.046 points. In all other years, the overall Tax Reform Index for tax rates was -0.031 points on average. This difference is statistically significant at the 5% level. Overall tax rates seemed to decrease in pre-election years: The mean of the overall Tax Reform Index for tax rates is -0.013 (right panel), whereas the mean of the overall Tax Reform Index for tax rates is -0.004 in all other years. The difference in means does, however, not turn out to be statistically significant for pre-election years. The patterns do not change when we compare election dates and tax reforms using the full sample of countries, which also covers emerging market economies (Figure B.3 in the Appendix).

2.3.3 Empirical strategy

The unconditional correlations reported in Figure 2.4 suggest that tax rates, on average, increase after elections, implying that policymakers postpone tax increases until after elections to avoid unfavorable effects on electoral outcomes. However, these unconditional correlations may be influenced by the large cross-country heterogeneity in national tax systems documented in Section 2.2.4. Also, our descriptive statistics revealed pronounced trends in taxation over time. Our empirical strategy addresses the confounding influence of cross-country heterogeneity in tax policies and trends in taxation over time. We follow Foremny and Riedel (2014), estimating variants of the model

$$\mathfrak{S}_{it}^{A} = \gamma E_{it+\tau} + \mathbf{X}'_{it}\boldsymbol{\beta} + \eta_i + \zeta_t + \varepsilon_{it} \quad , \tag{2.4}$$

where the Tax Reform Index $\mathfrak{S}_{it}^{A}$ of country i at time t is modeled to be a function of the election date $E_{it+\tau}$. We explore temporal dynamics in tax changes around election dates by examining election years ($\tau = 0$), pre-election years ($\tau = -1$), and post-election years ($\tau = 1$). To account for cross-country heterogeneity in taxation, we include fixed effects for countries η_i. These effects also account for all other time-invariant unobserved factors that may correlate with election dates and tax reforms (e.g., cultural norms and socialization, political history, dominant schools of thought, geography, and institutional frameworks). To account for cross-country trends in taxation over time, we include year fixed effects ζ_t. These effects also account for exogenous shocks that similarly affect all countries in the dataset and which may influence taxation (e.g., economic crises, see Fuest et al., 2022). All unobserved time-varying shocks to taxation are absorbed by the idiosyncratic error term ε_{it}.

Identification: Our parsimonious models of Equation 2.4 consider tax policies separately for election years, pre-election years, and post-election years. In our full model specification, we simultaneously take into account the years around elections

$$\mathfrak{S}_{it}^{A} = \mathbf{E}'_{i}\boldsymbol{\delta} + \mathbf{X}'_{it}\boldsymbol{\beta} + \eta_i + \zeta_t + \varepsilon_{it} \quad , \tag{2.5}$$

where dummy variables for election years, pre-election years, and post-election years are included in the vector $\mathbf{E}'$ via

$$\mathbf{E}' = \begin{bmatrix} E_t \\ E_{t-1} \\ E_{t+1} \end{bmatrix} \quad \text{and} \quad \begin{cases} = 1 & \text{in the election year (0 otherwise)} \\ = 1 & \text{in the pre-election year (0 otherwise)} \\ = 1 & \text{in the post-election year (0 otherwise)} \end{cases} . \tag{2.6}$$

As election dates vary across countries, our approach resembles a "difference-in-differences" framework in which countries with no election in a given year serve as a control group to identify the effect of elections on tax policies in the treatment group of countries with an election (and on those countries in a pre- and post-election year). Threats to the identification come from country-specific omitted factors that are correlated simultaneously with tax policies and the timing of elections. To the extent that these factors are time-invariant over our observation period (such as, for instance, institutional, cultural, historical, geographic or political factors), they are included in the set of country-fixed effects η_i. To the extent that these factors correlate with trends in taxation and period-specific shocks, they are included in the set of period-specific effects ζ_t.

Biases may arise from factors that determine national tax decisions and are correlated with election dates. In variants of Equation 2.4 and Equation 2.5, we account for observable time-varying factors that may confound the relationship between election dates and taxation. These factors are stacked in the matrix $\mathbf{X}'_{it}$ and include government ideology, the growth rate of real per capita GDP, globalization, and the quality of political institutions.

The partisan theories describe that leftwing and rightwing governments implement fiscal policies to gratify the needs of their constituencies. Leftwing governments are expected to cater to low-income citizens and favor income redistribution; rightwing governments are expected to cater to high-income citizens and favor less income redistribution than leftwing governments (Hibbs, 1977; Chappell and Keech, 1986; Alesina, 1987; Schmidt, 1996; Potrafke, 2017, 2018). Leftwing governments have been shown, for example, to set higher CIT rates than rightwing governments (Osterloh and Debus, 2012).

Tax reforms may also be driven by a country's past macroeconomic performance (see, e.g., Castanheira et al., 2012). On the one hand, tax increases may face fewer political headwinds in times when the macroeconomic performance is favorable. On the other hand, tax increases may be inevitable if spending increases in recessions have raised the budget deficit. To account for these mechanisms, our model includes the growth rate of real per capita GDP.

Globalization is likely to influence taxation policies (Jha and Gozgor, 2019). The question is how. The race-to-the-bottom hypothesis describes that globalization puts pressure on national governments: systems competition gives rise to decreasing tax rates (Sinn, 1997, 2003). By contrast, when citizens demand higher social insurance during globalization, governments need to increase public expenditure and may want to increase tax revenues. On the globalization-welfare state nexus, see, for example, Schulze and Ursprung (1999) and Potrafke (2015).

The main hypothesis underlying the electoral cycle theory is that self-interested politicians have incentives to pursue expansionary fiscal policies before elections and to postpone contractionary policies until after the elections. Leeway for such practices is higher in countries with weaker political institutions.

Our Tax Reform Index is available for a total of 23 advanced and emerging market economies. Data on elections is missing for China among these 23 countries. Our benchmark estimates are based on the 16 democratic OECD and/or EU-member countries included in Armingeon et al. (2020) to compare our results with previous studies on electoral cycles that were based on OECD countries with established political institutions. Next, we enlarge our analysis to cover the full sample of 22 countries.

Equation 2.4 and Equation 2.5 test a reduced form of electoral cycle models by examining whether tax reforms are determined by election dates. Following many previous studies, we do not aim to explicitly test the (rational) political business cycle theories, as such a test is difficult and would require having a measure for (unobserved) government competency (e.g., Kneebone and McKenzie, 2001).

2.3.4 Data description

Election data: Our main election data comes from the "Comparative Political Data Set 1960-2018" compiled by Armingeon et al. (2020). This dataset includes information for 16 of the 22 countries included in our sample. For the analysis of our full sample, we enlarge the election dataset with the information provided in the Database of Political Institutions 2020 (Scartascini et al., 2021).

Data for control variables: Data on real per capita GDP comes from the Penn World Tables version 9.1 (Feenstra et al., 2015). To measure government ideology, we update the index of

Potrafke (2009) that assumes values between one (strong rightwing government) and five (strong leftwing government). For globalization, we employ the KOF Globalisation Index compiled by Dreher (2006) and Gygli et al. (2019). Political institutions are measured using the continuous democracy indicator of Gründler and Krieger (2021, 2022). We also code government changes using the information provided in the Database of Political Institutions 2020 (Scartascini et al., 2021). Data on additional fiscal measures (expenditure and public debt) is taken from the IMF's International Financial Statistics database.

Sample of established democracies: We distinguish between two samples of countries. Our benchmark sample includes "established democracies", defined as countries that have been OECD members before the fall of the Iron Curtain. Incentives to adjust the policy mix around elections to improve re-election prospects should increase with the political power of the electorate. We compare our results with the full sample that also includes countries with less developed democratic institutions.

2.3.5 Baseline results

Table 2.1 presents our baseline results. The main result is that tax rates increase after elections, but not prior to elections or during the election year. The parameter estimates of the election year and pre-election year variables are not statistically distinguishable from zero – both when we include them individually (Columns I and III) and when we estimate the full model specification (Column IV). By contrast, the parameter estimate of the post-election year variable is statistically significant at the 1% level ($t = 3.30$) in Column II and when estimating the full model specification (Column IV). Numerically, the parameter estimate on the post-election dummy in the parsimonious model suggests that the overall Tax Reform Index for tax rates was 0.0717 points higher in post-election years than in other years included in our sample (Column II). In the full model specification, the parameter estimate slightly increases, suggesting that the Tax Reform Index was 0.0874 points higher in post-election years than in other years that were no election or pre-election years (Column IV). Considering the distribution of the variables, the estimated coefficients suggest that the increase in the overall Tax Reform Index for tax rates was around 0.24 standard deviations larger in post-election years than in other years.

In Table B.2 in the Appendix, we provide complementary results for the overall Tax Reform Index for tax bases. The results do not provide any evidence for a political business cycle in tax bases.

Table 2.1 : Baseline results, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.000926			0.0384
	(0.03)			(1.04)
Post-Election		0.0717***		0.0874***
		(3.30)		(3.25)
Pre-Election			-0.0151	0.00824
			(-0.43)	(0.19)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.138	0.146	0.138	0.147
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level

Our baseline results suggest that election-motivated politicians influence tax policies around elections. The results also show that it is important to distinguish between changes in tax rates and bases. While we find an economically and statistically significant increase in the overall Tax Reform Index for tax rates after elections, similar inferences cannot be drawn regarding the overall Tax Reform Index for tax bases.

2.3.6 Robustness

Observable confounding factors: A concern may be that the results are driven by time-varying confounding factors. In Table B.3, we control for observable factors that may be simultaneously correlated with tax policies and elections. Potential confounders include the growth rate of real per capita GDP, government ideology, globalization, and political institutions (see Section 2.3.3). Including these controls results in fewer observations. We observe that (i) estimating the baseline model using the reduced number of observations does not change the inferences and (ii) including the controls also does not change the results.

US midterm elections: Our benchmark election dummy follows the definition of Armingeon et al. (2020) and includes mid-term elections for the United States. The results are basically identical when we exclude US midterm elections (see Table B.4 in the Appendix).

Jack-knife analysis: A concern about our benchmark estimates is that the results may be driven by individual countries. To examine whether outliers influence our results, Table B.5 in the Appendix reports results from a jack-knife analysis which re-estimates the baseline model while gradually excluding observations for each country. Inferences do not change.

Non-normalized index of tax reforms: Our estimates are obtained based on the tax reform indices that are normalized on the interval $[-2, +2]$. The motivation for this choice is two-fold. First, our research question is whether we observe tax reforms before and after election dates. Hence, we are interested in the occurrence of any reform and not in the occurrence of multiple reforms. Second, we want to rule out that the results are driven by outliers. For instance, a series of reforms led the sub-index for PIT to assume values of +6 in Canada (1985) and the United Kingdom (1976), and the sub-index for VAT to be -10 in France (2000) and -5 in Spain (1999). The total number of observations outside the interval $[-2, +2]$ is, however, small for each sub-index.[10] Inferences regarding the electoral cycle do not change when we replace the non-normalized index with the normalized index.

Reduced version of our tax reform index: Another methodological concern regarding the coding scheme of our indices may refer to the IMF's classification of "minor" and "major" reforms. To alleviate concerns about misclassifications in the scope of the reform, our reduced variant of the tax reform index codes each year with any decrease in tax rates as -1, each year with any increase in tax rates as $+1$, and zero otherwise. Using this reduced variant as the dependent variable does not change the inferences (see Table B.6 in the Appendix), but as expected, we observe a change in the size of the estimated parameters.

Electoral cycles in emerging market economies: Our tax reform indices are available for a total of 23 countries, including 16 democratic OECD and/or EU-member countries covered by Armingeon et al. (2020), as well as Brazil, China, the Czech Republic, India, Mexico, South Korea, and Turkey. Our baseline results are obtained using the 16 countries included in Armingeon et al. (2020) to examine electoral cycles in established democracies with strong political institutions. In Table B.7, we re-estimate our baseline model for the full sample of

[10] Out of 1,166 observations, the following number of observations is outside the normalized interval $[-2, +2]$: CIT (48), PIT (59), excises (27), VAT (31), property tax (2), SSC (23). The share of observations outside the normalized interval is less than 5% for each index.

countries. This analysis excludes China, where the Database on Political Institutions does not report a single election during the observation period. Exploiting the full sample of countries and years does not change the inferences. Using the broader set of countries, however, yields larger estimated parameters and lower standard errors.

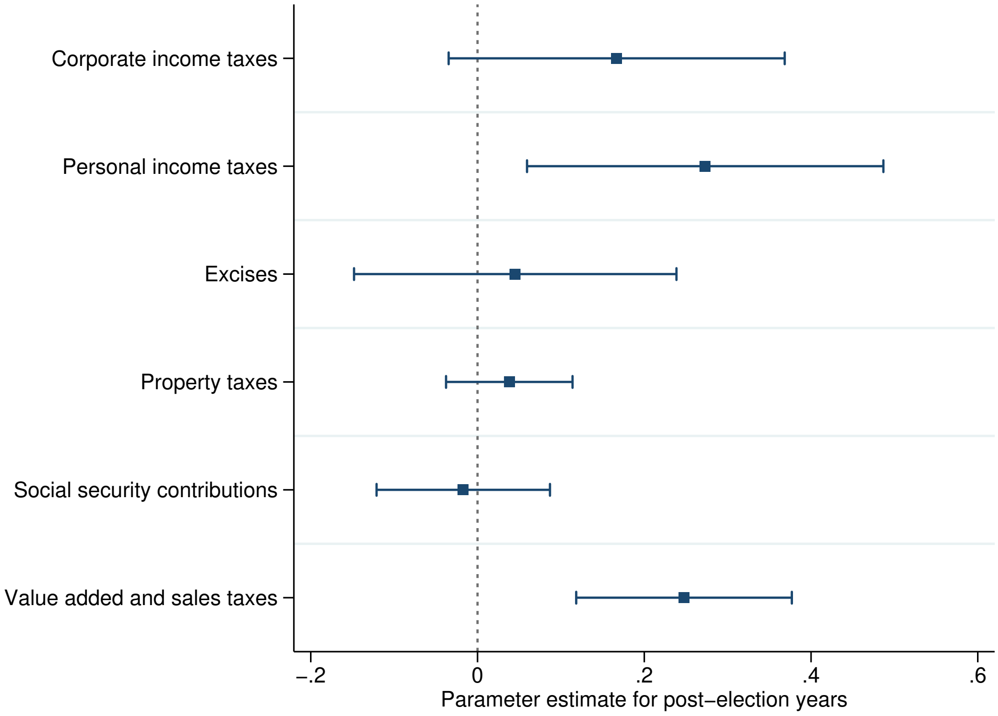
Early elections: Election-motivated politicians may well have less time to manipulate economic policies and outcomes when elections are called early than when elections take place on their scheduled date. Scholars, therefore, disentangle the effects of regular and early elections (Shi and Svensson, 2006; Potrafke, 2010). Our dependent variable is based on announcements of tax reforms and not the reforms' implementation dates. It is therefore conceivable that election-motivated politicians influence tax reforms also before early elections. We use official government documents to code a dummy variable that is 1 if an election took place early (and zero otherwise), building on the early election data by Potrafke (2010, 2020). Our baseline sample includes 232 elections, 93 of them were called early (about 40%). We disentangle regular and early elections in Table B.8 in the Appendix. The results indicate that election-motivated politicians postponed tax increases until after elections, regardless of whether elections take place regularly or were called early. The estimated parameters for both types of elections are similar in size (0.0866 for regular elections and 0.0721 for early elections).

2.3.7 Heterogeneity across tax types

In Tables B.9–B.14 in the Appendix, we re-estimate our benchmark model of Table 2.1 for the individual tax types. The key results are shown in Figure 2.5, plotting parameter estimates for post-election years and 90% confidence intervals for the extended specification that includes pre-election years, election years, and post-election years (Column IV of the Tables in the Appendix). The results show that the overall postponement effect is driven by VAT and, to a lesser extent, by PIT. Regarding the VAT rate, the parameter estimate of the post-election year variable is statistically significant at the 1% level in Columns II and IV and indicates that the Tax Reform Index for the VAT rate was around 0.34 standard deviations larger in post-election years than in other years.

In a similar vein, the Tax Reform Index for the PIT rate was higher in post-election years, delivering a parameter estimate that is statistically significant at the 5% level (Table B.10). The Tax Reform Index for SSC was lower in election years (Table B.14). The parameter estimates of the election year variable have negative signs and are statistically significant at the 5% level in Column I and at the 10% level in Column IV. The estimates suggest that the Tax Reform Index

Figure 2.5 : Heterogeneity across tax types



Notes: The figure shows parameter estimates for post-election years (indicated by blue boxes) and 90% confidence intervals (indicated by blue lines) for the full model specification (Equation 2.5) that includes pre-election years, election years, and post-election years for identification. The numerical values are reported in the full model specifications presented in Columns IV of Tables B.9–B.14 in the Appendix). Detailed results for all specifications including the parsimonious model specifications can be found in Tables B.9–B.14 in the Appendix.

for SSC was by around 0.18 standard deviations lower in election years than in any other year. The results do not change when we perform the robustness checks conducted in Section 2.3.6 for each of the tax types. Regarding CIT (Table B.9) and property taxation (Table B.13) we do not find a strategic manipulation around elections.

The estimates for individual tax types emphasize the advantages of our indices, providing more granular results about the origin of the strong post-election effects. Such effects could not be uncovered with previous tax indices, which do not allow for a similar in-depth examination of tax changes after elections.

2.3.8 Election promises and pre-election tax announcements

The observed change in tax policy may simply reflect the realizations of policies that have been announced prior to the election date. Our tax reform indices are designed to tackle the confounding influence of potential pre-electoral promises and announcements of politicians.

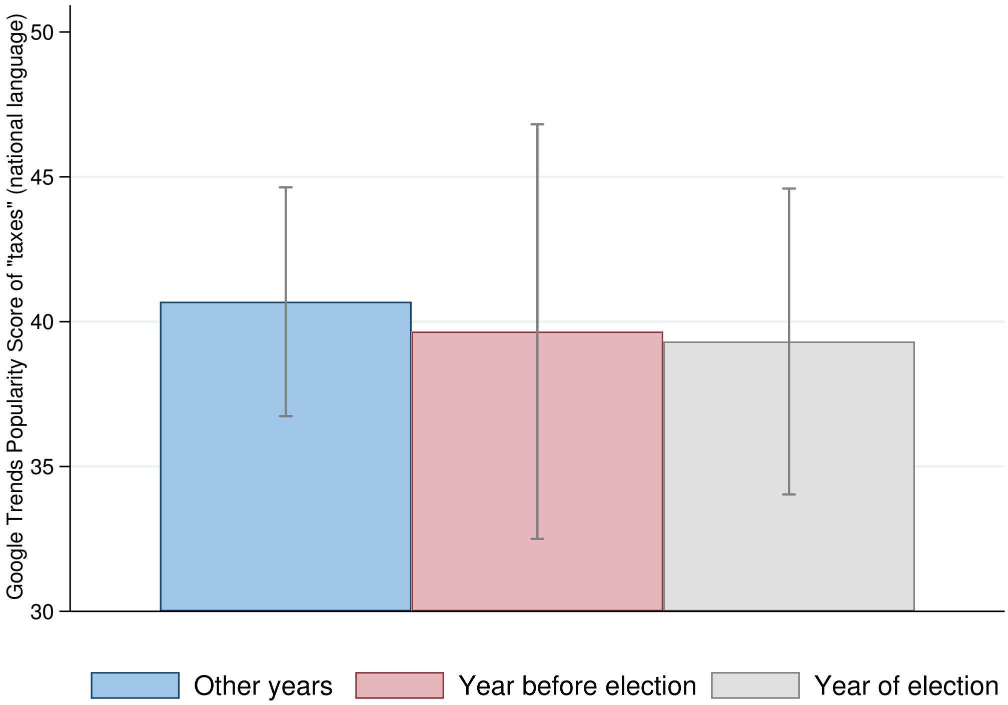
Our tax reform index is coded to reflect a change in policy in the period when the tax reform has been announced, not in the period when it became legally binding. This coding scheme guarantees that the index captures unanticipated tax changes and hence helps disentangle post-election changes from pre-election announcements. A related motivation for this coding scheme is that economic policies usually give rise to adjustments in the labor market and investment decisions well before they become legally binding.

It may also be the case that tax increases after elections reflect election promises made during the election campaign. If election campaigns would have been coined by discussions about tax policies, we would expect greater public attention toward such policies during pre-election years and election years. We use data from Google Trends to examine the popularity of tax policies around elections, collecting Google Trends popularity scores for all countries included in our sample and all sample years that are covered by Google Trends since its launch in 2004. Popularity scores refer to search queries in the official language of our sample countries and are coded between zero (low popularity) and 100 (high popularity).

Figure 2.6 shows popularity scores for the years around elections and shows that tax policies have not been more popular in Google searches in pre-election and election years compared to other years. This result provides suggestive evidence that, on average, tax policies have not been major topics in election campaigns in our sample and speaks against potential biases initiated by anticipation effects.

The minor popularity of tax policies around elections notwithstanding, one may also want to examine in detail whether changes in tax rates after elections have been announced in the party platforms. Clearly, it is unlikely that politicians announce tax increases in a pronounced manner before elections. The models on political business cycles describe that incumbents signal their fiscal competence to voters prior to elections, which speaks against announcements of tax increases prior to elections (see Section 2.3.1). To signal competence, it is more likely that politicians promise tax decreases or promise not to increase taxes after elections. If so, our results would suggest that politicians have not fulfilled their pledges. Compiling data on pledges across countries is quite demanding because doing so requires expertise about the individual political systems, institutions, and language skills because party platforms are typically in the countries' national languages. With a team of researchers from individual industrialized countries, Thomson et al. (2017) have examined the extent to which politicians have fulfilled their pledges. The data by Thomson et al. (2017) disentangle tax policy pledges for six out of twelve countries. Among those six countries, our sample only includes Canada, Ireland, the United Kingdom, and the United States, which are too few to be examined in our panel data model.

Figure 2.6 : Public attention toward taxation policies around elections, Google Trends Popularity Score



Notes: The figure shows how public attention toward tax policies has developed around elections, plotting Google Trends Popularity Scores for election years, pre-elections years, post-election years, and other years. Data is extracted from Google Trends; the popularity scores refer to searches that have been conducted in the official language of the respective countries in the sample. The figure includes all years that are covered by Google Trends since 2004.

2.3.9 Incumbents versus new governments

Incumbent governments have incentives to postpone tax reforms until after elections in order to avoid the adverse effects of unpopular policies on their electoral success. However, new governments can influence tax systems only after they have been elected into office. In Section 2.3.1, we describe our main hypotheses regarding tax reforms around elections, emphasizing that new governments are likely to reinforce electoral cycles in taxation. However, a concern about our results may be that the estimated parameters are fully driven by newly elected governments, in which case the estimated parameters for post-election years would not reflect electoral cycles but rather changes in the ruling incumbent. We address this concern by disentangling the results for re-elected incumbents and opposition parties that newly entered office. Results are reported in Table B.15 in the Appendix. Accounting for government changes in our baseline models does not change the inferences. The inferences also do not change when we estimate a sub-sample for government changes after elections and elections without government changes. These results corroborate that the benchmark regression outcomes are not biased by systematic differences in tax policies between incumbent politicians and newly elected governments.

2.3.10 Tax reforms and other fiscal policy measures

The political business cycle theories describe that incumbents increase spending prior to elections to stimulate the economy and to improve the prospects of getting reelected. These theories are complementary to our hypotheses: When governments increase spending before elections, there may be need to consolidate budgets afterward. One possible alternative for consolidation would be an increase in taxes. When fiscal policies are motivated by electoral considerations, there may be feedback effects between government expenditure and taxes. Examining tax reforms, our benchmark empirical models only reveal a specific part of the strategic manipulation of fiscal policies.

To alleviate concerns about a biasing influence of government expenditure, we re-estimate our benchmark models including data on government expenditure that we collect from the IMF's *"International Financial Statistics"* (IMF, 2022). Results are reported in Table B.16 in the Appendix. The parameter estimates of our tax reform indicator remain unchanged, both qualitatively and quantitatively, when we include government expenditure as a control variable. Inferences do also not change when we account for the acquisition of public debt around elections.

2.4 Conclusion

Motivated by the notorious scarcity of a cross-nationally comparable dataset that includes harmonized measures of tax reforms for tax rates and bases, we introduce a new collection of tax reform indicators: The Tax Reform Index (TRI). We construct indicators that are based on the dataset by Amaglobeli et al. (2018). Our sample includes 23 countries: 16 democratic OECD and/or EU-member countries included in Armingeon et al. (2020) and Brazil, China, the Czech Republic, India, Mexico, South Korea, and Turkey. The data is available over the period 1960-2014. The TRI allows us to uncover distinct trends in taxation over the past six decades. The new dataset may be helpful also for other researchers examining the causes and consequences of tax reforms.

How election-motivated politicians influence economic policies has been examined for a long time. Many previous studies have focused on public expenditures and deficits that politicians often increase before elections. Taxation policies are also an excellent measure to be manipulated around elections: politicians are well advised to postpone tax increases until after elections. Electoral cycles in taxation have not yet been examined across countries, however, because no suitable data was available. Based on our new dataset, we investigate electoral cycles in taxation on the national level where the key tax policy decisions take place.

Researchers have been eager to investigate electoral cycles in taxation. So far, they needed to be satisfied with sub-national or incomplete national data to measure taxation policies. Tax systems are complex: they encompass individual types of taxes and politicians use tax rates, tax bases, and exemptions to design tax systems in manifold ways. What is more, tax systems vary a great deal across countries. Consequently, there was no evidence yet describing how electoral motives influence overall taxation policies.

Our results indicate that election-motivated politicians influenced taxation policies around elections. Tax rate increases seemed to be postponed until the year after the election. This effect is strong: it is numerically important – the overall Tax Reform Index was around 0.24 standard deviations larger in post-election years than in other years – statistically significant at the 1% level and robust to many robustness tests. Election-motivated politicians were especially active in increasing VAT tax rates after elections.

3 Political leaders and macroeconomic expectations: Evidence from a global survey experiment*

3.1 Introduction

> *"American Elections 2020: Joe Biden's victory sparks huge relief in Europe"*
> — Le Monde, November 9, 2020
>
> *"Scientists relieved as Joe Biden wins tight US presidential election"*
> — Nature, November 9, 2020

Subjective expectations about the macroeconomy are the foundation of all forward-looking decisions made by households, firms, and experts. A vast and growing literature studies how these expectations are formed (e.g., Coibion and Gorodnichenko, 2015; Coibion et al., 2018a; Dräger and Nghiem, 2021; Dräger et al., 2022). While the economic mechanisms underlying the formation of economic expectations are increasingly well understood, much less is known about the political origins of economic expectations. Political leaders often have a great impact on national economic outcomes (e.g., Jones and Olken, 2005; Besley et al., 2011), and influence economic expectations in their home country (e.g., Snowberg et al., 2007; Coibion et al., 2021; Mian et al., 2021). But in an interconnected world where the actions of political leaders reach beyond national borders, exceptional leaders may also shape expectations on a global scale.

We provide first evidence on political spillover effects in the formation of macroeconomic expectations. We exploit the change in political leadership induced by the 2020 US presidential election in a large-scale international survey experiment among 837 influential economic experts who provide policy advice in 107 countries. We distributed our survey in two waves, where each wave consists of a randomly selected subset of participants. We gathered the results of the first wave shortly before the election (the control group). The second wave was collected five days later, after Joe Biden had been called president-elect by major US media outlets (the treatment group).

* This chapter is based on joint work with Dorine Boumans, Klaus Gründler, and Niklas Potrafke. A previous version of this study circulated as "The Global Economic Impact of Politicians: Evidence from an International Survey RCT" (CESifo Working Paper No. 8833).

We present three main results. First, the US incumbent change had significant and economically sizable spillover effects on the formation of global macroeconomic expectations. Those experts who were surveyed after Joe Biden won the 2020 US presidential election expected real GDP growth in their host country in 2021 to be 0.98 percentage points (p.p.) higher than experts surveyed before the election date. In the sample of experts outside the United States, the treatment effect increases in size (1.16 p.p.). In contrast, the treated US-based experts have, on average, lower growth expectations than non-treated US-based experts. This result provides strong evidence for a political spillover effect in the formation of macroeconomic expectations. Second, the treatment effect for GDP growth is driven by more positive expectations about foreign trade. For experts outside the United States surveyed in the second wave, the mean value of expectations about changes in trade volumes is 1.95 p.p. larger than for experts in the control group. Examining experts from countries that were in a trade war with the United States during the Trump presidency, the treatment effect of Biden's victory on expected trade volumes increases to 2.25 p.p. Third, the US incumbent change increased experts' uncertainty about the future state of the economy. We asked participants to predict the percentage chance for several possible outcomes of macroeconomic variables and calculate individual-level probabilistic density forecasts. We use these distributions to compute dispersion measures that quantify the level of uncertainty for each expert and find greater post-election uncertainty. We find no treatment effect for the sub-sample of US-based experts, who have been better informed about Biden's intended policies prior to the election than experts outside the United States.

Our results have important implications for understanding the origins of expectation formation. They suggest that macroeconomic expectations are influenced by global factors that are beyond the control of national policymakers. These global factors – political shocks being a prime example – may well have large numerical effects on macroeconomic expectations and, in turn, macroeconomic outcomes. Our findings also highlight the impact of politicians on the formation of expectations about the future state of the economy. Given the dominant role of the US president in world politics, our results are obtained in a setting where experts' expectations might be particularly responsive to political impulses. Against the backdrop of China's rise in world politics and the global reactions in response to Russia's invasion of Ukraine, however, our results may also apply to the impact of other global leaders. Studying this impact is a promising avenue for future research.

Contribution to the literature: The main contribution of our paper is to provide first evidence on political spillover effects in the formation of economic expectations. Our study builds on previous work that examines how politicians and national leaders influence the economy (Jones and Olken, 2005; Besley et al., 2011; Yao and Zhang, 2015; Brown, 2020; Easterly and Pennings, 2020), economic expectations of agents (e.g., Snowberg et al., 2007; Treisman, 2011; Huberman et al., 2018; Coibion et al., 2021; Bachmann et al., 2021; Mian et al., 2021), and electoral surprise on economic forecasts (Shelton, 2012). Our results contribute to this literature by uncovering cross-national spillover effects on expectations. These spillover effects complement the literature on state actors' foreign influence (see Aidt et al. (2021) for a survey of the literature).

Our study explores the process of expectation formation of professional economic experts. By advising and informing policymakers, firms, and the general public, economic experts exert a major influence on other agents' expectations. Against this backdrop, the scarcity of empirical evidence on the determinants of experts' expectations is startling. A burgeoning literature aims to fill this gap, exploring assessments of economic experts (e.g., Gordon and Dahl, 2013; Sapienza and Zingales, 2013; DellaVigna and Pope, 2018; Gründler and Potrafke, 2020; Zingales, 2020) and the factors that shape economic expectations of professional economists (e.g., Malmendier et al., 2021; Andre et al., 2022). We contribute to this literature by showing that experts immediately update their expectations when new political information becomes available. This type of rational expectation updating is in stark contrast with a sluggish reaction to new information typically observed for the general public (e.g., Dräger et al., 2022), providing important insights into the process of expectation formation of professional economists.

We also relate to the literature exploring the determinants of economic expectations. This literature typically analyzes the formation of expectations by households, firm managers, or professional forecasters (Dräger et al., 2016; Coibion et al., 2018b; Coibion et al., 2018a, Dräger and Nghiem, 2021; D'Acunto et al., 2022). Our study contributes to the knowledge about the political origins of macroeconomic expectations. While previous work has shown that political shocks can influence inflation expectations (Dräger et al., 2022), our study offers a global perspective on political spillover effects regarding expectations about key macroeconomic variables.

Our study also adds to the literature investigating how political uncertainty influences economic outcomes (e.g., Pastor and Veronesi, 2012, 2013; Kelly et al., 2016; Baker et al., 2016; Bloom et al., 2018) and how electorally-induced policy uncertainty impacts agents' economic expectation (Gerber and Huber, 2009; Jens, 2017; Falk and Shelton, 2018).[1] Measuring political uncertainty is afflicted with a series of methodological challenges (Kelly et al., 2016). Our innovation to address these challenges is twofold. First, we use exogenous variation in political uncertainty by implementing a survey experiment around the 2020 US presidential elections. Second, we ask participants to predict the percentage chance of certain outcomes of the macroeconomic variables included in our survey. We use these numbers to retrieve individual-level measures of revealed uncertainty based on the resulting probabilistic density function. The positive effect of Biden's victory on the degree of uncertainty of international experts complements the existing literature by indicating that ambiguity about the policies of a newly elected incumbent may increase the uncertainty of economic experts.

We finally connect to the literature on large-scale survey experiments in economics. So far, most studies have conducted national survey experiments (e.g., Kuziemko et al., 2015; Snowberg and Yariv, 2021; Stantcheva, 2021; see Haaland et al. (2021) for a survey of the literature). Our work joins the ranks of an emerging strand of studies that conduct large-scale survey experiments in multiple countries (Alesina et al., 2018; Algan et al., 2021; Fehr et al., 2022; Alesina et al., 2023). We advance on this literature by conducting a global survey experiment that allows us to draw inferences about the external validity of the political spillover effects.

3.2 The 2020 US presidential election

We first describe the events on election night, 3 November 2020, and the subsequent events until major US media outlets called the election for Biden on 7 November 2020. We then discuss unique features of the US presidential election that make it an ideally-suited laboratory to study political spillover effects on global macroeconomic expectations.

3.2.1 Events on election night and subsequent days

The outcome of the US presidential election on 3 November 2020 was announced on 7 November 2020. It took some time to count the votes, particularly, because many citizens voted early and via post. The Republican incumbent, Donald Trump, declared himself the winner of the election on 3 November 2020. At that time, Donald Trump won critical swing states such as Florida and Ohio and was leading in states such as Wisconsin, Michigan, Georgia, North Carolina, and Pennsylvania. He would have won the election if he had won the states

[1] In a similar vein, financial markets responded to flawed poll data on the US presidential election day in 2004 (Snowberg et al., 2007).

where he was leading on 3 November 2020. However, the lead changed in many tight states while the postal ballots were counted. Michigan and Wisconsin were called for Biden on 5 November 2020.

Donald Trump was ahead in the swing state Pennsylvania until 6 November 2020, but it was called for Biden on 7 November 2020. Biden had 273 votes in the Electoral College at this time (CNN) – 270 votes are needed for a majority in the Electoral College that elects the US president. Consequently, major US media outlets called Joe Biden the winner of the election on 7 November 2020. The international press quickly picked up the news and disseminated it worldwide.

Because of Trump's strong political and societal polarization, the 2020 presidential election was perceived to be one of the most important elections in the recent history of the United States. The election had the highest turnout since 1900. With more than 80 million votes, no candidate in the history of the US presidential election has ever received as many votes as Biden did.

3.2.2 The election as a natural experiment

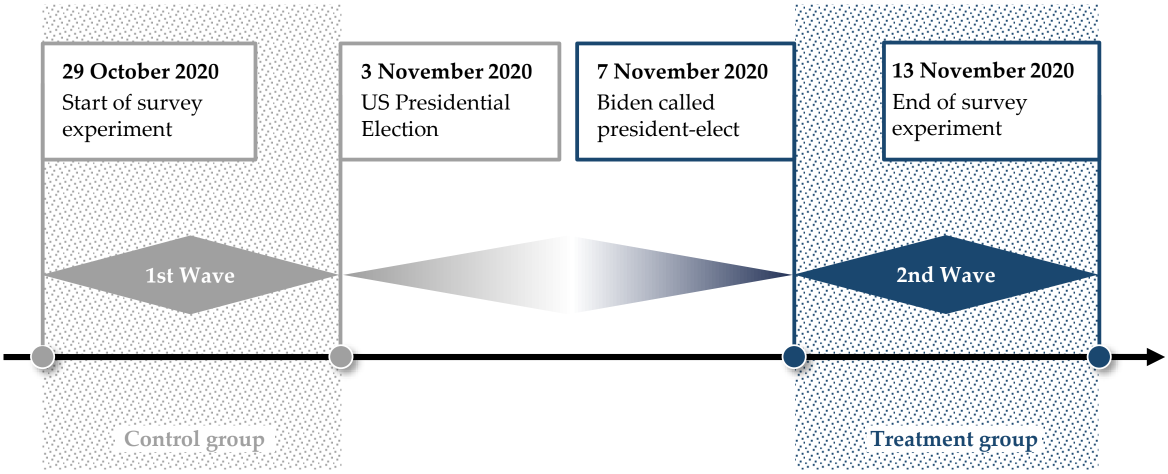
Figure 3.1 visualizes the general setup we designed to exploit the 2020 US presidential election as a natural experiment examining the political origins of expectations formation. We conducted a large-scale global survey among economic experts that consisted of two waves. The first wave was sent out shortly before the election took place. Experts surveyed in this wave are in the control group of the experiment (29 October 2020 – 3 November 2020). The second wave was sent out five days later, directly after Joe Biden was called president-elect (8 November 2020 – 13 November 2020).[2] Experts polled in this wave are in the treatment group of our experiment. Given the two-step nature of the survey, the intuition of our approach is to compute treatment effects by comparing sample means of expectations between experts in the control and treatment group, conditional on fixed effects and control variables.

3.2.3 Anticipation of the electoral outcome

Joe Biden led in many polls conducted prior to the 2020 US presidential election that sought to forecast the popular vote (see, for example, The Economist, 2020). However, four years earlier, Donald Trump's victory in the US presidential election of 2016 caught many by surprise. Almost all polls and experts predicted a victory for Hillary Clinton. Nevertheless, Donald Trump

[2] We sent out the second wave on 8 November 2020 to exactly match the distribution time with the distribution time of the previous wave.

Figure 3.1 : Setup of our survey experiment: Timing of the survey's 1st and 2nd wave



Notes: The figure shows the general setup of our survey experiment and the timing of the two waves of our survey that constitute the control group and the treatment group. The control group was surveyed in the first wave, shortly before the election (29 October 2020 – 3 November 2020). The treatment group was surveyed in the second wave, directly after Joe Biden had been called president-elect by major US media outlets (8 November 2020 – 13 November 2020).

won the overwhelming majority of electoral votes (304 out of 538), whilst losing the popular vote by almost three million.[3] Bookmakers' odds for Trump winning the 2020 election were even higher compared to the 2016 election (see Figure C.5 in the Appendix). Against the backdrop of the 2016 presidential election and the inherent unpredictability of elections, we do not expect that experts in the control group considered a victory of Joe Biden for reporting their expectations. Ex ante uncertainty about the electoral outcome was intensified by the special character of the incumbent Donald Trump, who succeeded in heavily mobilizing his supporters to participate in the 2020 election.

Statistical consequences of anticipation effects: Anticipation effects in the control group would downward bias the results because mean expectations in the control group would be higher when some experts reported more favorable macroeconomic environments in 2021 based on an anticipated Biden victory.

[3] The division between the popular vote and that of the Electoral College has been shown to be driven by regional heterogeneity in political polarization and socioeconomic and demographic factors. Accounting for such factors in standard election models would have given rise to the prediction of Donald Trump's victory in 2016. Using data gathered until just before the 2020 US presidential election, these augmented models also predicted a tight race between both candidates regarding the outcome of the Electoral College votes and suggested no clear favorite (Ahmed and Pesaran, 2022).

3.2.4 Advantages of the empirical setup

Strong economic and political power of the United States: The US economy is by far the largest economy on the globe. At the time of the 2020 US presidential election, the International Monetary Fund estimated nominal GDP in the United States to be 20,807,269 million US-Dollars, amounting to approximately one quarter of global production (IMF, 2020). Consequently, the state of the US economy strongly influences the economic performance of other countries (e.g., Corsetti et al., 2014). The United States is also the most influential international power in world politics, and global leadership is a key element of the political role that the United States envisages for itself (Congressional Research Service, 2020). There is also evidence of US political power being harnessed to influence countries' decisions in favor of US economic interests (Berger et al., 2013). Viewed together, the US president is likely to be the most economically and politically powerful politician in the world.

High levels of public attention: The 2020 US presidential election was closely followed by the international community. For instance, the French "Le Figaro" wrote on 3 November 2020 that *"outside the World Cup (soccer) finals, there is hardly any planetary suspense comparable to the U.S. presidential election"* (Gelie, 2020). In a similar vein, the Italian daily La Repubblica titled *"the world is waiting"* (Castelletti et al., 2020). There is little doubt that the US presidential election attracts interest beyond the United States and that its outcome shapes the international landscape. Given the high level of public attention, we have good reasons to assume that international experts closely observed the US presidential election and the events until 7 November 2020, when Joe Biden was called president-elect by major media outlets. Using search data from Google, Figure C.6 in the Appendix illustrates the global surge in interest in the US presidential election at the end of October 2020.

Little scope for confounding by concurrent elections: On 3 November 2020, the US electorate also voted on governors, house representatives, and senators. Gubernatorial elections were held in eleven states and two territories.[4] The outcomes of these elections offer little scope to impact our results, because the number and size of states and territories with gubernatorial elections on 3 November 2020 were small. Except for Montana, where the governor's party changed from Democratic to Republican, the elections did not give rise to changes in the ruling party. Similarly to the gubernatorial election outcomes, the 2020 election for the House of Representatives initiated little change. Although the Democratic Party lost 13 seats compared to the 2018 election, it retained control of the House with a

[4] Gubernatorial elections were held in Delaware, Indiana, Missouri, Montana, New Hampshire, North Carolina, North Dakota, Utah, Vermont, Washington, West Virginia, American Samoa (territory), and Puerto Rico (territory).

222–213 majority. There is also little room for confounding created by the 2020 US Senate elections. Democrats gained three seats in the November general election, leaving the party with a total of 48 seats (46 registered Democrats and two allied independents) and the vice presidency, while Republicans held 50 seats.[5] Electoral uncertainty was only resolved after Georgia's run-off election on 5 January 2021. Both Democratic candidates narrowly won their races and tied the partisan balance in the Senate. Importantly for our setting, however, experts surveyed in the treatment group did not know about the outcome of the 2020 US Congress elections.

3.3 Survey experiment design and descriptive evidence

3.3.1 General design and randomization

Sample: We utilize the unique infrastructure of the Economic Experts Survey (EES, formerly "World Economic Survey", WES) at the ifo Institute and the CESifo research network to reach out to renowned international experts working in universities, research institutes, central banks, multinational companies, embassies, and international organizations. This survey has been used in related studies that examine how professional economists form expectations (e.g., Andre et al., 2022). The strength of the survey is its global coverage. Our survey includes participants from countries that cover 99.5% of the world GDP, 83.4% of the world population, and 74.7% of the global land area.

We contacted a total of 1,552 international experts and received answers from 837 participants (about 54%). The survey ran from 29 October 2020 to 13 November 2020. We focus on renowned economic experts whose opinions influence the national economic debates in their country. Almost all participants in our sample have a university degree and about half of the participants hold a PhD.

Randomization and waves: We randomly split the universe of experts into two subsamples that we separately surveyed in two waves (see Figure 3.1). Randomization was carried out by a software-based randomization generator. The first wave was surveyed from 29 October 2020 until 3 November 2020, 23:59 Central European Time (CET) (the "control group"). The election took place on 3 November 2020. Our sample for the control group incorporates all answers from experts that participated in our survey until public authorities and major news outlets published the first results on 4 November 2020 at 00:00 CET. The outcome was announced

[5] It was unclear which party would hold the two Georgian Senate seats during the next term following the November general election. Election laws in Georgia require Senate candidates to win at least 50% of the vote in the general election, which none of the candidates did.

on 7 November 2020. The second wave (the "treatment group") was collected after the news of Joe Biden's victory had become public, covering the period from 8 November 2020 to 13 November 2020, 23:59 CET. We distributed our survey via the software *qualtrics*^{XM}. The invitation to participate in the survey and other emails were always sent at 12:00 CET. The procedure was identical for the treatment and control group.

3.3.2 Main study

Questions included in the survey: The full questionnaire used for both waves of the survey is presented in Figures C.1–C.4 in the Appendix, showing the design of the web interface and the wording of our questions. Our survey encompasses 12 questions on economic expectations. Experts are asked to provide their expectations for the country in which their professional work is located (the "host country"). The experts' host country is to 80% identical to their country of origin. Our survey includes expectations regarding four key macroeconomic variables: (i) the growth rate of real GDP (in %), (ii) the rate of inflation (in %), (iii) the unemployment rate (in % of the labor force), and (iv) the change in trade volumes (in %).

We differentiate between expectations regarding the short-term macroeconomic environment in 2021 (Questions one, four, seven, and 10) and the macroeconomic environment over the upcoming presidency until the end of 2023. We do not include the election year 2024 (Questions three, six, nine, and 12). The survey includes two categories of questions to elicit experts' point estimates and probabilistic density forecasts of future macroeconomic variables.

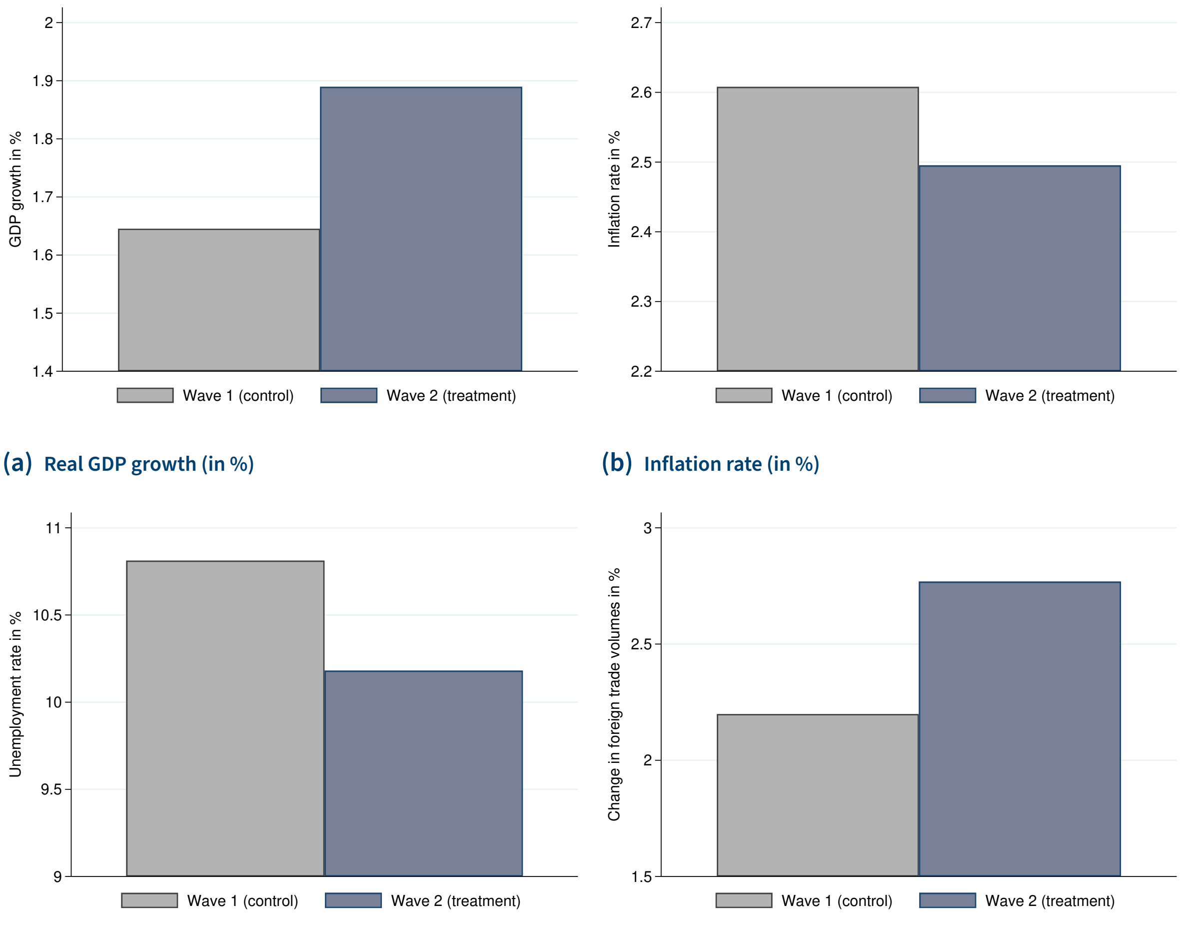
Point estimates of macroeconomic variables: For point estimates, we ask *"What is your estimate of* `[macroeconomic variable]` *in 2021?"*. Participants see a scale encompassing a broad range of possible outcomes (e.g., for real GDP from -15% to +15%) and are asked to put the slider at the position corresponding to their estimate. Participants also have the option to tick a box saying "Don't know".

Probabilistic density forecasts: To measure the degree of uncertainty in experts' expectations, we present a scale showing bins of possible outcomes and ask experts to provide the percentage chance that the macroeconomic variables fall within each bin. Our query asks *"Please indicate which probability you assign to the following* `[macroeconomic variable]` *in 2021"*. Based on the answers to this question we compute summary statistics of the resulting density forecast, which serve as measures for the expert-level degree of uncertainty (see Section 3.6).

3.3.3 Descriptive evidence

Figure 3.2 shows the sample means of our key macroeconomic variables for the randomly chosen experts surveyed in wave one prior to the election ("Pre-Election", the control group) and the experts polled in wave two after the election result had become public ("Post-Election", the treatment group). The descriptive statistics show that experts polled after Biden's victory expect a generally more favorable short-run macroeconomic environment. On average, participants in the treatment group expect higher average levels of real GDP growth, lower rates of inflation and unemployment, and greater increases in international trade volumes.

Figure 3.2 : Means of experts' expectations, pre- versus post-election



(a) Real GDP growth (in %)

(b) Inflation rate (in %)

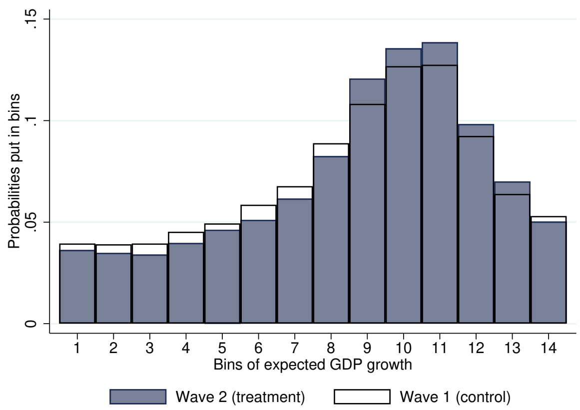
(c) Unemployment rate (in %)

(d) Change in foreign trade volume (in %)

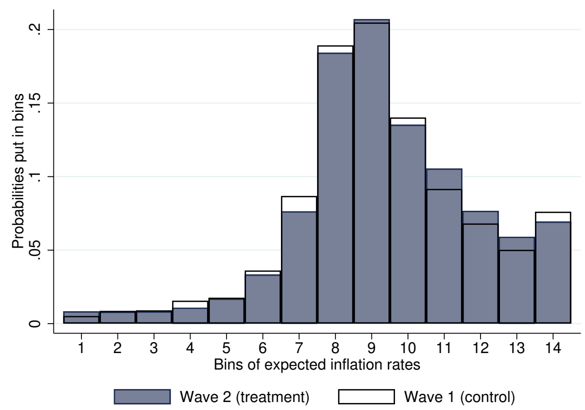
Notes: The figure shows sample means of expectations regarding our four key macroeconomic variables (real GDP growth, inflation, unemployment, and changes in trade volumes). The figure shows mean levels of expectations separately for both waves of our global survey. Grey bars refer to the randomly chosen group of experts surveyed in wave one prior to the election (labeled "Wave 1 (control)", the control group of our survey experiment). Blue bars refer to the group of experts polled after the election (labeled "Wave 2 (treatment)", the treatment group of our survey experiment).

Figure 3.3 plots the probabilistic density forecasts for treated and non-treated experts. Consistent with the descriptive results for the point estimates, experts polled in wave two of our survey experiment assign greater (lower) probabilities to higher (lower) growth rates of GDP than experts surveyed in the first wave. Experts in the treatment group also assign higher (lower) probabilities to lower (higher) unemployment rates. The results show no clear tendency for inflation rates. For trade, we find that experts polled in the second wave put greater probability mass to higher positive changes in trade volumes.

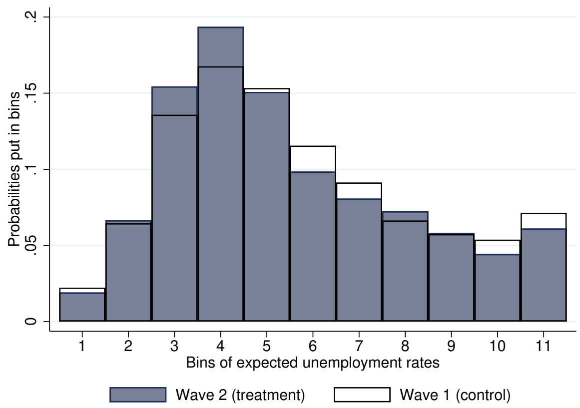
Figure 3.3 : Distribution of experts' expectations over bins, pre- versus post-election



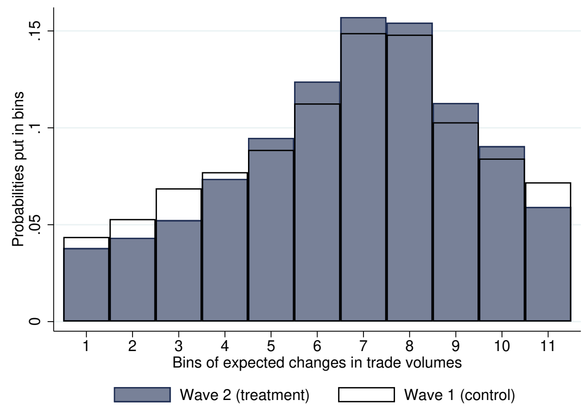
(a) Real GDP growth (in %)



(b) Inflation rate (in %)



(c) Unemployment rate (in %)



(d) Change in foreign trade volume (in %)

Notes: The figure shows the distribution of experts' expectations over bins regarding our four key macroeconomic variables (real GDP growth, inflation, unemployment, and changes in trade volumes). For each bin, experts are asked to quantify the expected chance that the variable will fall into the respective bin category. The figure shows overlapping histograms of expectations separately for both waves of our global survey. Plain bars refer to the randomly chosen group of experts surveyed in wave one prior to the election (labeled "Wave 1 (control)", the control group of our survey experiment). Blue bars refer to the group of experts polled after the election (labeled "Wave 2 (treatment)", the treatment group of our survey experiment).

Consistency of expectations: In Figure C.7 in the Appendix, we compare the point estimates provided by experts with means retrieved from the probabilities experts distributed over bins. For each macroeconomic variable, the figure shows a strong positive correlation, pointing to a high level of internal validity of experts' reported expectations.

3.4 Empirical strategy

3.4.1 Pre-analysis plan and hypothesis

We submitted our pre-analysis plan on 28 October 2020. The pre-analysis plan comprised three building blocks. First, it specified the outcome variables that we are interested in (GDP growth, inflation, unemployment, and trade). Second, it included the setting of our analysis, specifying that we ask economic experts working in 120 countries and randomly split the sample into two balanced sub-samples. It then described our strategy of asking half of the participants during the five days before the election and the other half during the five days following the election; examining the effect of the US presidential election in a survey experiment setting. Third, the pre-analysis plan also included information about the procedure of the online questionnaire. No changes have been made to the intended specification submitted prior to the experiment. Given that the outcome of the US election became clear in the course of 7 November 2020 however, we sent out our survey's second wave on 8 November 2020.

Our pre-analysis plan also included our main hypothesis. The purpose of our study is to examine the global impact of exceptional politicians on expectations about macroeconomic outcomes. We use the 2020 US presidential election as a survey experiment to examine the causal effect of a particularly influential politician, the US president, on global macroeconomic expectations. The motivation of this setup is to study global political spillover effects in the formation of macroeconomic expectations. How do economic experts change their expectations in response to the reelection or recall of the incumbent Donald Trump? Our hypothesis from the pre-analysis plan is:

Hypothesis 1. *If Trump were to win the US presidential election, we would expect economic expectations to decline.*

Vice versa, this hypothesis implies more favorable macroeconomic expectations in case of an electoral success of Joe Biden.

3.4.2 Estimation strategy

Our empirical strategy is designed to examine whether the outcome of the 2020 US presidential election has influenced experts' expectations about their host countries' future macroeconomic performance. While a comparison of group means prior to and after the election date shown in Figure 3.2 is informative, the differences in means may be influenced by country-specific factors and confounding events. We address these concerns in our econometric specification.

Each respondent filled out our questionnaire once, either in the first or the second wave of our survey. Respondents differ, however, in the day (t) they participated in our survey. The baseline empirical specification is given by

$$M_{ei(t)} = \gamma T_{e(t)} + \eta_i + \zeta_{e(t)} + \mu_e + \varepsilon_{ei(t)} \quad , \tag{3.1}$$

where the dependent variable $M_{ei(t)}$ denotes expert e's expectations about the level of macroeconomic variable M for the year 2021 in country i, filling out our survey at day t. We ask experts about four key macroeconomic variables: The growth rate of real GDP, the inflation rate, the unemployment rate, and the percentage change in trade volumes. The treatment variable $T_{e(t)}$ indicates whether experts were polled in the first $(T_{e(t)} = 0)$ or the second $(T_{e(t)} = 1)$ wave of our survey. Against the backdrop of the narrow time span between the two survey waves and the election, the major difference between the groups is that experts surveyed in the second wave knew that Joe Biden would be the 46th president of the United States.

Identification: Given the randomized assignment of experts to waves one and two of our survey, the parameter γ should identify the causal effect of having knowledge about the outcome of the US election. In view of the specific setup of our study, four threats to identification remain. First, the number of included experts is small for some countries. In the absence of a perfect balance for each country in the sample, the past macroeconomic environment of countries may influence expert e's expectation about the future.[6] Heterogeneity in the initial state of the economy may matter for the estimates given the substantial differences in macroeconomic conditions across countries during our survey

[6] The formation of expectations by economic agents is usually modeled via an AR(1) process (e.g., Mankiw and Reis, 2002; Coibion et al., 2018a). This process has also been shown to match the formation of macroeconomic expectations by economic experts (Malmendier et al., 2021).

period. Second, while observable differences between countries can be controlled for, there may be unobserved heterogeneity across countries (e.g., culture, political history, institutions, or political and economic ties to the United States) that influence the estimated treatment effect. Third, there may be confounding events between the treatment and the day experts participated in the survey. Fourth, experts may put varying levels of effort into filling out the survey.

The specification of Equation 3.1 tackles the empirical challenges. We include country dummies to account for unobserved cross-country heterogeneity and the host country's past macroeconomic performance (η_i). Fixed effects for countries also eliminate confounding effects from the relationship of the experts' host country with the United States. The model also includes dummies measuring the distance (in days) between the date t at which expert e participated in the survey and the election day ($\zeta_{e(t)}$). These dummy variables account for confounding treatments and address the fact that the US election was likely more present in experts' minds directly after Joe Biden had been called president-elect by major media outlets. Finally, we include the time (in seconds) experts took to fill out the survey (μ_e). This variable accounts for differences in the endeavor of experts and controls for "box checking". We also expect this variable to be correlated with other unobserved personality traits among experts.

In our benchmark estimates, we only include countries for which we have at least polled three experts to alleviate the concern that results are driven by outliers. We later change this requirement in our robustness tests.

3.4.3 Key identifying assumption and balance tests

The key identifying assumption underlying the model in Equation 3.1 is that in the absence of the treatment, the control and the treatment groups would be statistically identical, i.e.

$$\mathrm{E}[\varepsilon_{ei(t)}|T_{e(t)} = 1] = \mathrm{E}[\varepsilon_{ei(t)}|T_{e(t)} = 0] = 0 \quad . \tag{3.2}$$

This assumption cannot be tested directly because $\varepsilon_{ei(t)}$ is unobserved. When randomization was successful, the identifying assumption should be fulfilled by construction (see, for example, Bruhn and McKenzie, 2009). We can, however, conduct tests to examine whether the assumption in Equation 3.2 is *likely* to hold by comparing the sample means of observable characteristics between experts in the treatment and control group.

Our balanced tests provide no evidence for differences between the treatment and control group regarding gender, age, or education (see Figure C.8). The balance tests also show that treated experts do not differ from non-treated experts in their field of study or their affiliation (see Figure C.9).

Potential differences in the country composition between the treatment and control group do not seem to translate into systematic group differences in macroeconomic observables. Our balance tests show no differences between the control and treatment group for GDP growth, inflation, unemployment, or trade. The sub-samples are balanced regarding the initial conditions in the year prior to our survey experiment (Figure C.10) and the averages during the Trump presidency (Figure C.11). To eliminate the confounding influence of unobserved factors, our baseline model includes country fixed effects.

We are also interested in the effect heterogeneity between US-based experts and experts working outside the United States. Identifying causal effects in these analyses requires that the treated US experts do not differ from the non-treated US experts. Our balance tests for the United States show that this is the case (Figure C.12).

Finally, the consequences of the US presidential election may depend on the size and the global political influence of countries. Using the total population of experts' host countries as a proxy, we show that our sample is also balanced regarding countries' global political influence (Figure C.13).

3.5 Spillover effects on macroeconomic expectations

3.5.1 Benchmark results

Table 3.1 reports our baseline results. In Columns I–IV, we present the treatment effect of Biden being voted US president on macroeconomic expectations of professional economists for the year 2021. Results are shown for the growth rate of real GDP (Column I), the inflation rate (Column II), the unemployment rate (Column III), and the percentage change in trade volumes (Column IV). We present estimates for three samples. The first sample, shown in Panel A, includes experts from all countries in our survey. In Panel B, we investigate experts living outside the United States. In Panel C, we examine the expectations of US-based experts.

Results for the full sample of experts: In the full sample of experts, the treatment effect on the expected growth rate of real GDP in the year 2021 is 0.984. This effect is statistically significant at the 10% level ($t = 1.90$). Numerically, the parameter estimate shows that being surveyed in the second wave increases experts' expectations regarding the growth rate of GDP in 2021 by 0.984 p.p. This result suggests that information about Biden becoming US president has a substantial impact on the expectations of professional economists about the future growth rate of their host country. Consistent with the effect on expectations about GDP growth, we also find that treated experts expect unemployment rates to be 0.56 p.p. lower than non-treated experts ($t = 1.26$). In addition, we find positive effects on expected trade volumes ($t = 1.59$). For inflation, the effect is close to zero ($t = 0.11$).

Spillover effects: In Panels B and C, we distinguish between US-based and non-US-based experts to specifically examine spillover effects of the US leadership change on macroeconomic expectations. We find that the results for both subgroups differ considerably. In the sample of non-US-based experts (Panel B), the treatment effect increases in size (1.159 p.p.) and is statistically significant at the 5% level ($t = 2.03$). This result provides strong evidence for a political spillover effect in the formation of macroeconomic expectations. In contrast, the results in Panel C show that treated experts located in the United States have, on average, lower growth expectations than non-treated experts, although standard errors of this estimate are large ($t = 1.15$). The results for unemployment rates are comparable between the sample of US-based experts and experts living outside the United States. For inflation, we find remarkable differences between host countries. While experts living outside the United States expect slightly lower inflation rates in the second wave compared to the first wave ($t = 0.66$), we find positive treatment effects for the United States ($t = 1.55$).

3.5.2 Mechanisms

What are the mechanisms underlying our treatment effects? Column IV of Table 3.1 reports effects for the expected change in trade volumes, differentiating between the sample of non-US-based experts (Panel B) and the sample of US-based experts (Panel C). The sample split for trade volumes reveals major differences between experts working in the United States and those working in other countries. In the sample of experts outside the United States, the treatment effect for trade expectations is positive and statistically significant at the 5% level ($t = 2.30$). The effect size suggests that experts who were informed that Donald Trump, known for his protectionist policies, had been elected out of office expected trade volumes to be 1.949 p.p. higher in 2021 than experts polled before the election. We find no treatment effects for US-based experts. The results for trade volumes are consistent with Joe Biden's announcements prior to the 2020 election, signaling his beliefs in "fair trade" and his plans to *take down trade barriers that penalize Americans and resist a dangerous global slide toward protectionism* (Biden, 2020). In Table C.2 in the Appendix, we specifically look at the expectations of experts

Table 3.1 : The 2020 US presidential election and economic expectations of experts, baseline results

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.984*	-0.0289	-0.566	1.375
	(0.518)	(0.253)	(0.451)	(0.863)
Number of Experts	662	665	677	569
Number of Countries	68	68	68	68
R-Squared	0.207	0.760	0.794	0.176
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.159**	-0.183	-0.547	1.949**
	(0.572)	(0.276)	(0.518)	(0.847)
Number of Experts	620	620	632	541
Number of Countries	67	67	67	67
R-Squared	0.211	0.772	0.792	0.184
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of Experts	42	45	45	28
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

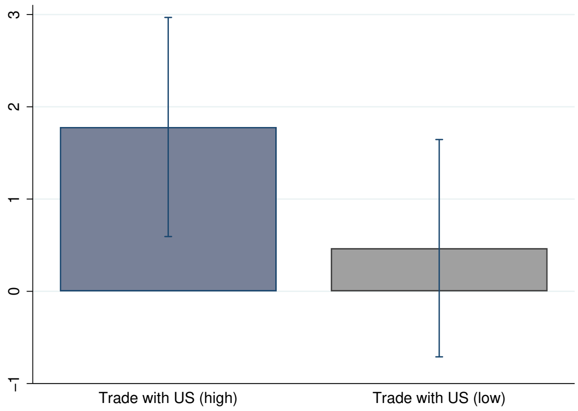
Notes: The table shows the estimated effect of the 2020 US presidential elections on the expectations of international experts. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. "Country-FE" are fixed effects on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denotes the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

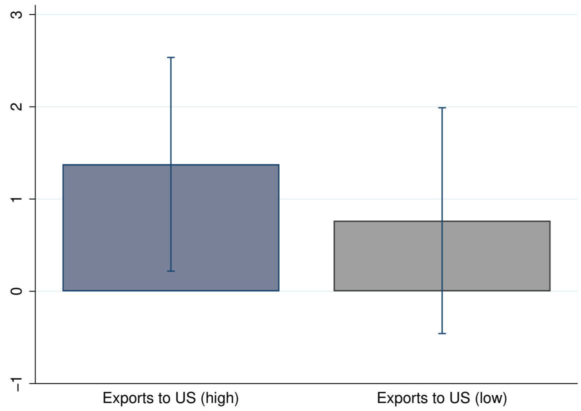
from countries that were at “trade war” with the United States during the Trump presidency. We find that treatment effects on trade volumes for those experts increase to 2.28 p.p.

If the political spillover effects on expectations about GDP growth would materialize via more positive trade expectations, we would expect to see stronger treatment effects for experts from countries that have close trade relations with the United States. Figure 3.4 shows the treatment effects on GDP growth separately for experts from countries with strong and weak

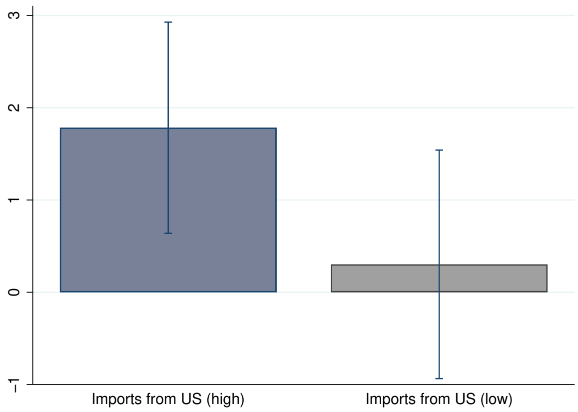
Figure 3.4 : Treatment effects on growth expectations conditional on trade relations with the United States



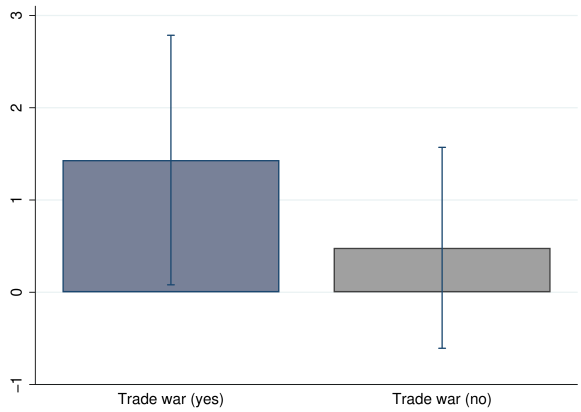
(a) Trade with the United States



(b) Exports to the United States



(c) Import from the United States



(d) Trade war with the United States

Notes: The figures show treatment effects conditional on four measures that quantify the degree of trade relations with the United States. “Trade with the US” shows re-estimates of the benchmark model separately for experts from countries with a ratio of trade flows (exports plus imports) relative to GDP greater (labeled “high”) or lower (labeled “low”) than the median level in the sample (5.18% of GDP). “Exports to the United States” and “Imports from the United States” show the results separately for export and import flows. “Trade war” visualizes treatment effects on GDP for countries that have been at trade war with the United States under the Trump administration (“yes”) and the set of countries that have not been at trade war with the United States (“no”). All estimations replicate the benchmark specifications shown in Table 3.1.

trade ties to the United States. We measure the degree of trade links to the United States via four variables. The first variable considers the share of imports and exports relative to GDP and re-estimates the benchmark model separately for experts from countries below and above the median value in the sample (5.18% of GDP). We then investigate exports and imports separately using the sample median as a cut-off. Finally, we report results separately for countries whose host countries have been at trade war with the United States under the Trump presidency, and for those that have not been exposed to trade sanctions. For all measures of trade links, we find economically and statistically significant estimates for experts from countries with strong trade relationships with the United States. For countries with weaker links to the United States, the estimated parameters are considerably smaller and lack statistical significance at conventional levels. These results provide evidence in support of the trade mechanism.

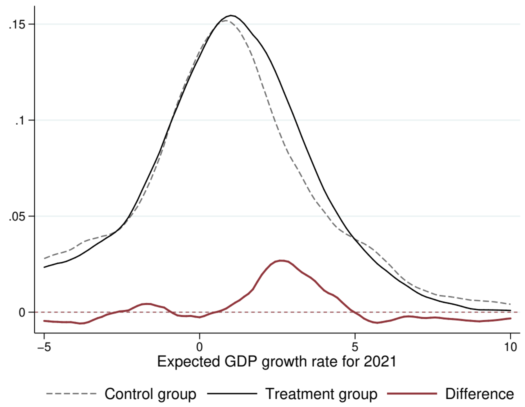
3.5.3 Distribution of expectations

A major question is whether the treatment effects are driven by outliers. Figure 3.5 visualizes the distribution of macroeconomic expectations across experts in the control group and experts in the treatment group conditional on the full set of variables included in our baseline model (Equation 3.1). To display spillover effects, the figure is computed using the sample of experts living outside the United States (Panel B of Table 3.1).

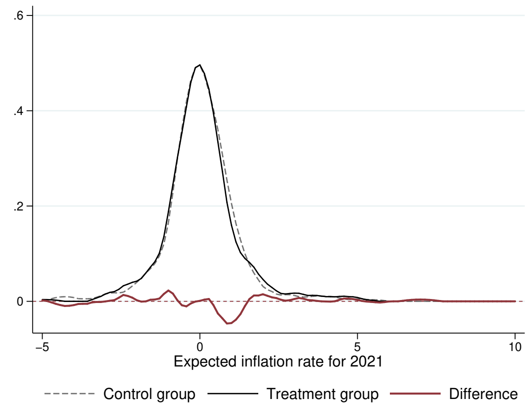
Regarding expected GDP growth for 2021, Figure 3.5 shows that the positive treatment effects stem from a larger portion of experts that report expected GDP growth rates of between 1% and 5%, whereas the tails of the distribution are quite similar between experts in the control group and the treatment group. We also observe lower expected inflation rates and unemployment rates, although here the differences are smaller. For trade, the distribution of expectations of experts in the treatment group has a negative skew, whereas the distribution of experts in the control group is more right-skewed, with a lower fraction of participants expecting negative changes in trade volumes, and a greater fraction of experts being more optimistic about future trade volumes.

Most importantly, the distributions shown in Figure 3.5 demonstrate that the differences in expectations between experts in the treatment and the control group are not driven by individual outliers.

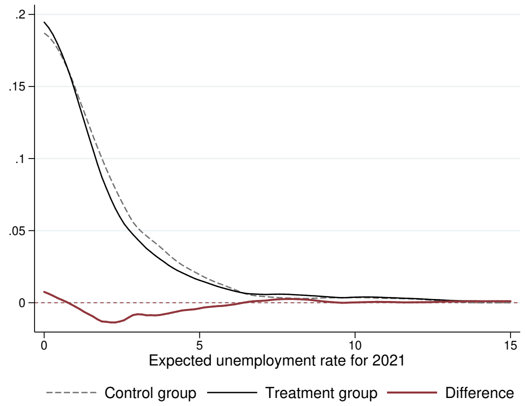
Figure 3.5 : Distribution of macroeconomic expectations, control group, treatment group, and difference



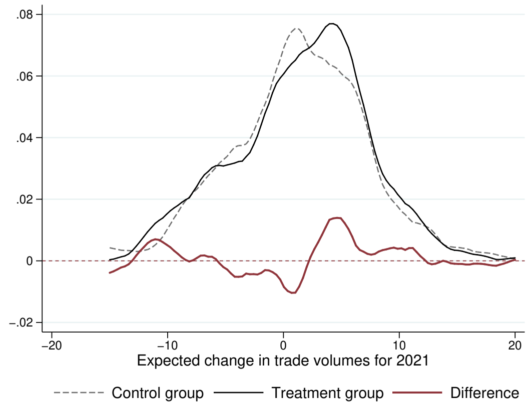
(a) **GDP growth**



(b) **Inflation rate**



(c) **Unemployment rate**



(d) **∆ Trade volumes**

Notes: The figure shows the distribution of macroeconomic expectations for experts in the control group (dashed grey line) and experts in the treatment group (solid black line). The solid red line plots the difference between both distributions. The figures visualize densities conditional on the variables included in the empirical model (Equation 3.1) employed to obtain the baseline estimates reported in Table 3.1. Densities refer to the model of non-US-based experts.

3.5.4 Treatment heterogeneity

The benchmark results deliver average effects over the full sample of global experts. We next examine heterogeneity in the treatment effects across the time horizon of expectations and across geographic units and political ties to the United States.

Temporal heterogeneity: How persistent is the effect of the US incumbent change on global macroeconomic expectations? We explore the temporal structure of the treatment effects on long-term expectations regarding the future state of the macroeconomy. To do so, our survey asks participants about their expectations of the macroeconomic environment up to the year 2023 (we exclude the highly contested election year 2024).

In Table C.14 in the Appendix, we present re-estimates of our benchmark model when we replace expectations for 2021 with expectations up until 2023. This analysis reveals a substantial degree of temporal heterogeneity in the treatment effects. Regarding long-run expectations, the treatment effects are close to zero.[7] These results suggest that the US incumbent change impacted macroeconomic expectations of international experts primarily in the short run.

The results are in line with many standard macroeconomic models in which macroeconomic variables return to a steady state after an exogenous shock so that these shocks have only transitory effects on the future development of the macroeconomy (e.g., Clarida et al., 2000; Rudebusch, 2002). An implication of such models would be that exogenous shocks impact agents' expectations only in the short run, particularly regarding the expectations of well-informed experts. This implication is precisely what our estimates on long-run expectations suggest.

Geographic and political heterogeneity: The experts included in our sample provide expertise for countries that differ in their political and economic ties to the United States. We next examine treatment heterogeneity across geographic units and across the degree of political ties to the United States.

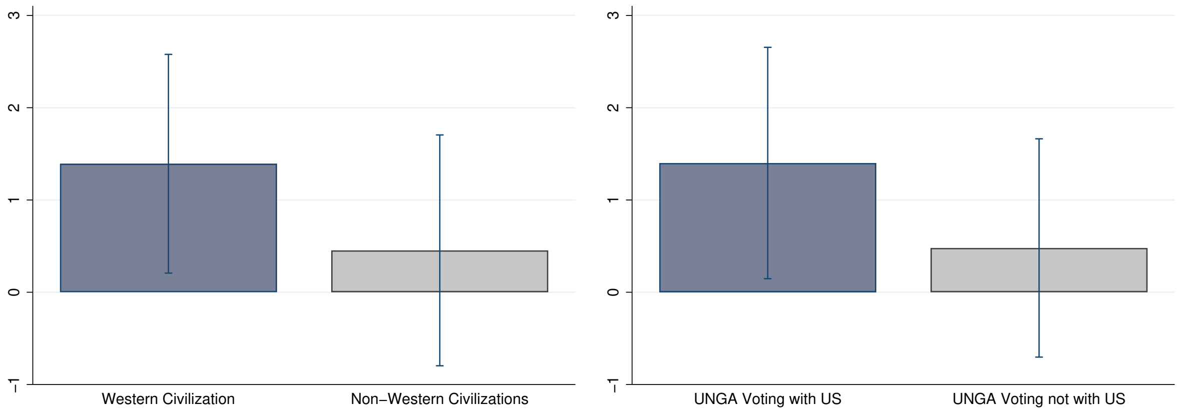
In Panel A of Figure 3.6, we report treatment effects separately for countries that are part of the Western civilization and compare the results to the sample of non-Western countries. For the classification of Western civilization, we follow Huntington (2000), who distinguishes nine major civilizations.[8] Countries belonging to particular civilizations share a common heritage of social norms, ethical values, traditional customs, beliefs, political systems, artifacts, and technologies. We hence expect spillover effects from the US president on expectations to be stronger for experts working in countries that belong to the traditional Western world. Consistent with this hypothesis, Panel A shows that the treatment effects are particularly strong for experts working in Western countries.[9]

[7] Inferences are also similar when we restrict the sample to experts included in the benchmark estimates (Table C.15).

[8] According to Huntington, the Western civilization consists of the United States and Canada, Western and Central Europe, Australia, Oceania, and most of the Philippines.

[9] The results are qualitatively similar when we exclude or include the United States in the sample of the Americas.

Figure 3.6 : Treatment heterogeneity, geographic and political division



(a) Geographic heterogeneity **(b) Political heterogeneity**

Notes: The figure shows heterogeneity in the treatment effects for GDP across geographic units and political clusters. Panel A shows treatment effects separately for Western and Non-Western Civilizations. The classification of countries refers to Samuel P. Huntingtons *"Clash of Civilizations"* (Huntington, 2000). Vertical lines represent 90% confidence intervals. Panel B shows heterogeneity in the treatment effects across voting behavior in the United Nations General Assembly (UNGA). The analysis considers the share of US-aligned votes which a country casts in the UNGA. A country is coded to vote in alignment with the United States when at least 40% of all votes are identical with the vote of the United States between the millennium and the time when Donald Trump entered office (2000–2016).

In Panel B of Figure 3.6 we more specifically examine political ties to the United States, distinguishing between countries based on their voting record in the United Nations General Assembly (UNGA). The analysis considers the share of US-aligned votes which a country casts in the UNGA. Countries are coded as voting in line with the United States if at least 40% of all roll-call votes are identical to the vote of the United States. The analysis encompasses the period between the millennium and the time when Donald Trump entered office (2000–2016).[10] The results in Panel B reveal treatment heterogeneity across political ties to the United States, with the effects being larger for experts from countries that are politically closer to the United States.

The treatment heterogeneity shown in Figure 3.6 is consistent with the trade mechanism. Trade tensions during the Trump presidency intensified between the United States and traditional Western allies, particularly from Europe.[11] The administration of Donald Trump made use of Section 232 of the Trade Expansion Act of 1962 which allowed him to impose tariffs on trading partners on national security grounds. Trump argued that the European

[10] We use the data collection by Voeten et al. (2009), which is updated regularly and comprises all roll-call votes in the UN General Assembly between 1946 and 2021.

[11] We also find positive parameter estimates for growth expectations of 1.74 p.p. when we specifically examine the sample of Chinese experts. The sample size of Chinese experts is however limited.

Union's *"unfair"* trade policies were jeopardizing the existence of critical US industries and that the European competition was *"endangering national security"*. The European Union retaliated the US tariffs on European steel and aluminum by imposing tariffs on jeans, bourbon whiskey, peanut butter, orange juice, and motorcycles. The worsening trade relations between the United States and the European Union were accompanied by deteriorating political ties, also reflected in voting behavior in the UNGA (Mosler and Potrafke, 2020).

3.5.5 Confounding events

A concern of our estimation strategy is that confounding events occurring during our survey period may influence our results. The statistical power of our analysis derives from the large sample of countries included and the randomization process. Any event specific to an individual country or geographic region should be eliminated by randomization and our global perspective. However, to the extent that confounding events influence all countries in our sample to a similar degree, our estimates may be biased. We next examine potential biases of our results caused by such confounding events.

Spread of Covid-19: The most relevant international phenomenon in 2020 was the global Covid-19 pandemic. It has been shown that during its initial spread, the number of daily cases of SARS-CoV-2 influenced policy recommendations of economic experts (Gründler and Potrafke, 2020). An increase in confirmed SARS-CoV-2 cases during our survey period would produce a downward bias of the estimates (via more negative expert expectations in our treatment group), in which case our estimates would reflect lower bounds. We would expect that controlling for differences in the number of SARS-CoV-2 cases increases the parameter estimates. In Table C.3, we show that this is indeed the case. When we account for the number of confirmed Covid-19 cases on the day the experts filled out our survey, the coefficient on real GDP increases from 0.98 to 1.25 and becomes statistically significant at the 5% level.

Announcement of effectivity of the vaccine by Pfizer and BioNTec: A related confounding event may be the news about the imminent availability of the second vaccine against Covid-19.[12] On 9 November 2020, the second day of our treatment period, the companies Pfizer and BioNTech announced that their vaccine developed against SARS-CoV-2 had proven to be 90% effective at preventing the spread of the virus. We conduct two analyses to examine whether the announcement of the vaccine's effectiveness confounds our results. In Table C.4,

[12] The Russian vaccine against Covid-19, *Sputnik V*, was registered on 11 August 2020 by the Russian Ministry of Health as *Gam-COVID-Vac* (Callaway, 2020; Cohen, 2020). The initial approval for distribution in Russia was based on the preliminary results of Phase I-II studies published on 4 September 2020 (Logunov et al., 2020).

we exclude all observations from experts who participated in our survey on 9 November 2020 or later. In Table C.5, we examine the narrowest possible band of days around the treatment to eliminate any other potentially confounding event. The sample is limited to observations from the day prior to the election (control group) and 8 November 2020 (treatment group). This specification delivers treatment effects that are very similar to our benchmark estimates.

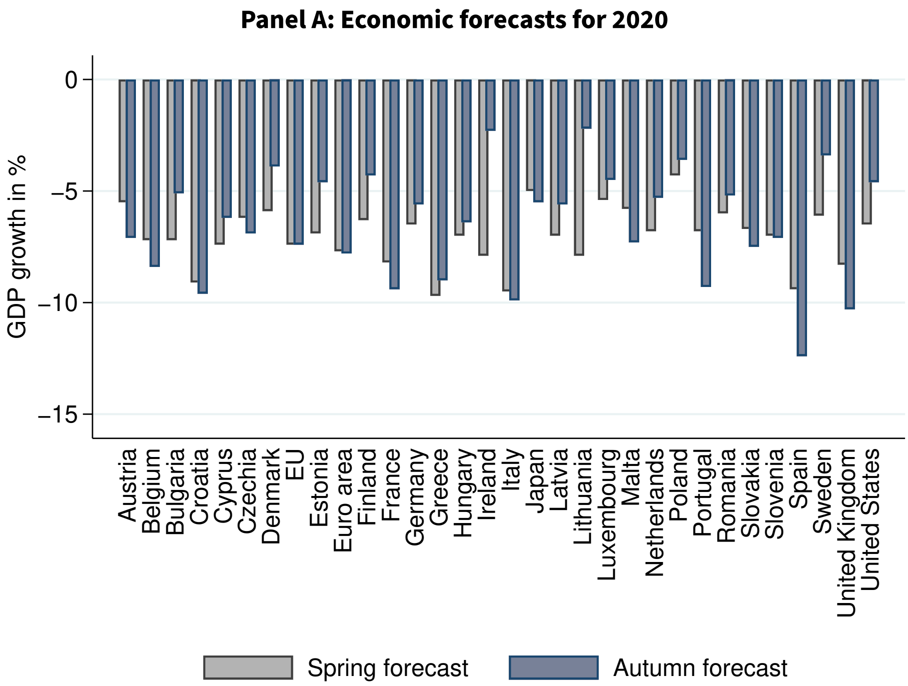
Economic forecasts: A remaining threat to identification is that the publication of major economic forecasts may bias our results. Economic experts usually factor in economic forecasts when forming their expectations about future economic growth. If economic forecasts affected participants in the treatment and control group differently, our estimated treatment effect would be biased. We exclude that economic forecasts drive our results by analyzing all economic forecasts published by government bodies, international organizations, and research institutes between 29 October 2020 and 13 November 2020.[13]

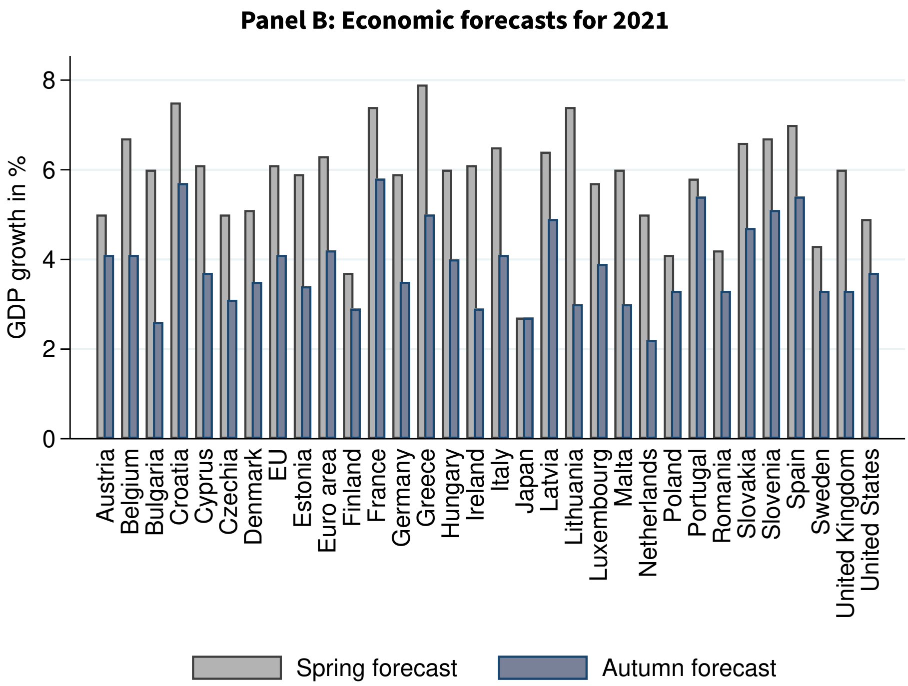
Screening publication dates and reported forecasts of a large number of economic outlooks published for the countries in our sample, there have been no forecasts that could have influenced our results except for the Autumn 2020 European Economic Forecast (European Commission, 2020a). Examining the potential impact of forecasts for Europe is particularly important given the strong effects we found for experts working in European countries. The European Economic Forecast was published by the European Commission on 5 November 2020 and generated considerable media response. Figure 3.7 shows that growth expectations for 2020 did not differ substantially from the Spring 2020 forecast. However, growth expectations for 2021 decreased considerably for most countries compared to the Spring 2020 forecast. Given the time of its publication, the forecast could not have influenced the growth expectations of participants surveyed between 29 October 2020 and 3 November 2020. However, the forecast might have influenced experts' expectations surveyed in our treatment group. Given the negative prospects for 2021 reported by the European Economic Forecast, respondents polled in the second wave are likely to decrease their economic growth expectations for 2021 in response to the forecast, and hence report lower GDP growth expectations in our survey. If anything, the publication of growth forecasts should downward bias our results. We are thus confident that we estimate a lower bound of the treatment effect, at least for countries included in the Autumn 2020 European Economic Forecast.

National confounding events: A final source of confounding may arise from national events. Even though the time window between the first and the second wave was only five days,

[13] We also investigated publications of monetary policy summaries and minutes of central banks, outlook reports by the World Bank, and country reports by the International Monetary Fund.

Figure 3.7 : Economic forecasts for 2020 and 2021, spring and autumn forecasts



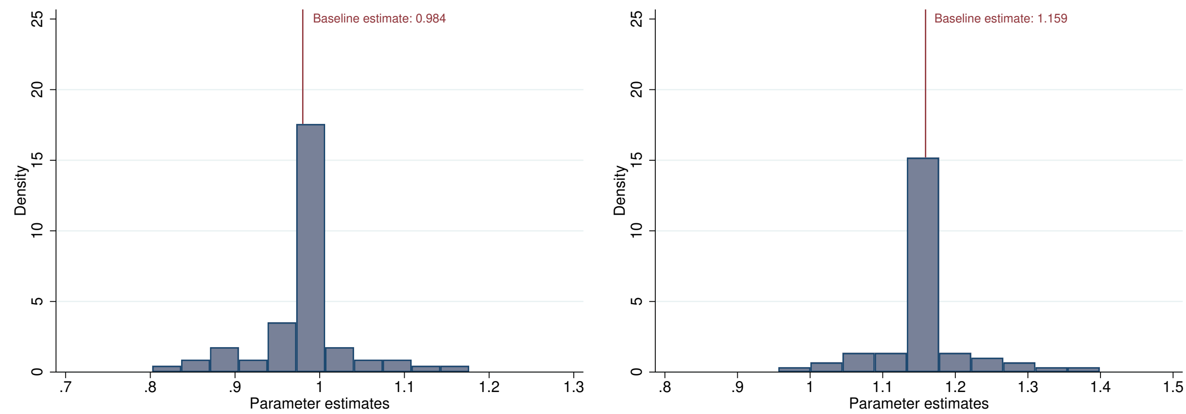


Notes: The figure shows expected GDP growth for 2020 (Panel A) and 2021 (Panel B) from the Spring 2020 and Autumn 2020 European Economic Forecast of the European Commission (European Commission, 2020a). GDP growth is forecast for the EU countries, the United Kingdom, Japan, and the United States.

there may have been multiple country-specific events around the globe that might influence the expectations of national experts. It is unlikely that such local events would impact macroeconomic expectations globally. However, national events may impact the expectations of experts who provide expertise for the countries in which such events occur.

To explore the consequences of national confounding events and their potential to confound the global results, Figure 3.8 presents the distribution of estimates from a leave-one-out analysis. Each replicate is computed by excluding experts from one specific country i. Panel A reports results for the full sample of countries. In Panel B, we replicate the analysis for all experts that provide expertise for host countries outside the United States.

Figure 3.8 : Accounting for regional confounding events: Distribution of point estimates when excluding participants from individual countries (leave-one-out)



(a) Full sample of experts **(b) Sample without US-based experts**

Notes: The figure shows the distribution of point estimates from our baseline model when we re-estimate the model I times, with each jackknife replicate being estimated excluding participants from country $i = 1, \ldots, I$. Distributions are obtained based on the full sample of participants (Panel A) and the sample of participants whose host countries are outside the United States (Panel B). The red vertical line displays the benchmark parameter obtained in the baseline estimations reported in Table 3.1.

The results of the leave-one-out-analysis deliver estimates that are close to the baseline parameter estimate, both for the full sample of experts and the sub-sample of experts based outside the United States. Most importantly, the results of Wald tests show that none of the estimates depicted in Panel A and B are statistically distinguishable from the baseline parameter estimate. The results of the leave-one-out analysis also allow us to compute a jackknife estimator $\hat{\gamma}_{\text{Jack}}$, which can in turn be used to calculate an estimate of the jackknife bias

$$\widehat{\text{bias}}(\hat{\gamma})_{\text{Jack}} = (I-1)(\hat{\gamma}_{\text{Jack}} - \hat{\gamma}_{\text{OLS}})$$

where $\hat{\gamma}_{\text{Jack}}$ is the average of all jackknife replicates computed for countries $i = 1, \ldots, I$. For the full sample of experts, the data suggests a jackknife bias of $\widehat{\text{bias}}(\hat{\gamma})_{\text{Jack}} = 0.0104$, for the sample of experts outside the United States, we get an estimate of $\widehat{\text{bias}}(\hat{\gamma})_{\text{Jack}} = 0.0026$. These results suggest that the jackknife bias is (very) small, amounting to between 0.002 and 0.01 p.p. of GDP growth. The estimates also suggest that the scope for confounding national events is very limited.

3.5.6 Additional results

We conduct many additional analyses to assess the robustness of our baseline results; for brevity, we report these results in the supplementary Appendix B. The set of additional analyses includes (i) several estimates where we alter the sample composition of experts and require a minimum of experts in waves one and two from the same country, (ii) estimates that condition the results on the past macroeconomic environment in experts' host country, (iii) several analyses that evaluate the robustness of the estimates to changes in the econometric specification and the construction of standard errors, and (iv) additional estimations that account for socio-demographic and biographic characteristics of the experts included in our sample (e.g., age, education, affiliation, the field of study, etc.). Our key findings re-appear in all of these additional analyses.

3.5.7 Experimenter demand effects

Our outcome variables are self-reported expectations of experts, giving rise to the possibility of experimenter demand effects (i.e. experts giving answers in line with what they think we want them to say. See, for example, De Quidt et al., 2018). A bias of our results caused by experimenter demand effects requires participants to (i) know that they are part of a survey experiment, (ii) want to help us, and (iii) know which answers would be helpful. There are four arguments that alleviate such concerns. First, we did not promote the intent of our survey experiment. In the invitation to participate in our survey, we wrote *"Dear* `[Ms./Mr.XY]`*, As a leading economic expert, we are pleased to invite you to participate in the Economic Expert Survey of the ifo Institute. Your opinion matters! Please access the online survey via your personal link:* `[Link to survey]`*. Your data will be stored and analyzed in full compliance with the highest standards of the data protection laws of the European Union. The survey will take you less than 5 minutes. We look forward to hearing from you!"*. The ifo Institute in Munich has been conducting the EES (and its predecessor, the World Economic Survey, WES) since 1981. Most of the experts have been participating in the survey for years or decades. Previous surveys were primarily conducted to measure experts' expectations for the next year. The experts know about the general intention of the EES to extract forecasts for the upcoming year. Previous versions of the survey were typically sent at a similar time in the year. Most importantly, previous waves of the WVS for decades asked about the identical macroeconomic variables. There was no reason for experts to assume that the survey is designed to identify the effects of the US presidential election. Second, we delayed the AEA registry until shortly before the survey started to minimize the chance of participants reading about our study design. Third, the invitation letter was signed by an assistant working in ifo's survey department who sent similar invitations for prior waves of the EES and the WES. Fourth, it was impossible to predict who would become president prior to the election. Hence, guessing responses that would produce interesting results would have been extremely difficult, particularly for participants in the control group surveyed before the outcome of the election became clear.

3.5.8 Alternative explanations

Our interpretation of the baseline results is that the treatment effects reflect the global impact initiated by the change in political leadership from Donald Trump to Joe Biden. The protectionist policies and break with long-standing international relationships engendered by the political ideas of President Trump may have led experts to be more pessimistic about their host country's growth perspectives in the face of a possible second Trump term. These results are consistent with global political spillover effects in the formation of macroeconomic expectations.

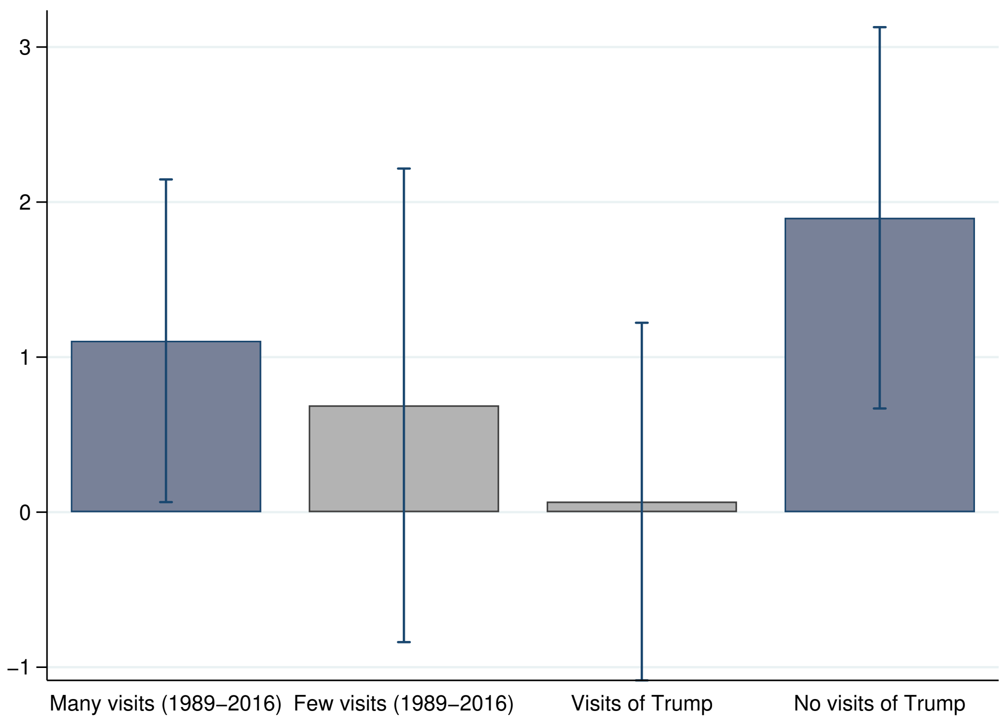
An alternative explanation of the treatment effects may be that the spillover effects are initiated by a change in the political party in power rather than by a change in the president. In Section 3.2, we discuss the specific features of the 2020 US presidential election that create a unique opportunity to disentangle partisan effects from the effects of politicians.

Additional suggestive evidence is also consistent with a political leader effect. If the more positive macroeconomic expectations after the election were driven by a party effect, we would expect to see a treatment effect for the entire reign of the Democratic Party. The temporal heterogeneity reported in Section 3.5.4 counters the explanation that experts' more positive views after the election are caused by their positive assessment of a Democrat in office during the legislative period 2021-2023 (excluding the highly contested election year 2024).

Complementary evidence on a limited probability of the results being driven by a party effect comes from the analysis of US presidential visits abroad. A large literature examines diplomatic visits of the president as a presidential tool commonly used for the advancement of foreign policy agenda (e.g., Canes-Wrone et al., 2008; Ostrander and Rider, 2019).[14] Given competing demands over their time, the decision of presidents on when, where, and how long to visit foreign countries directly reflects the presidents' political priorities.

Figure 3.9 considers the role of diplomatic visits of the US president for the treatment effects on GDP growth expectations. The intuition of this approach is that how presidents allocate their time and attention abroad reflects their strategic decision on diplomatic priorities. As these decisions are made by the president rather than the party, the results should tell us more about the degree to which the treatment effects reflect partisan or presidential origins.

Figure 3.9 : Treatment effects on growth expectations depending on presidential visits



Notes: The figure shows treatment effects separately for countries with many or few presidential visits between the fall of the Iron Curtain and the start of the Trump presidency (1989–2016), using the mean level of visits as a sample split. The figure also reports estimates for the sample of countries that Trump has visited between 2016–2020, and the sample of countries Trump has not visited but that have been visited before by US presidents since 1989. Vertical lines represent 90% confidence intervals. All estimations replicate the benchmark specifications shown in Table 3.1.

[14] The presidents' structural advantage in foreign policy vis-á-vis Congress places the president as the *"primary agenda setter in American politics with respect to foreign policy matters"* (Peake, 2001).

The results conditional on presidential visits support the argument of a political leadership effect. Re-estimating the empirical model for the subset of experts from host countries that have been visited frequently between the fall of the Iron Curtain and the start of the Trump presidency (1989–2016), we find large and statistically significant treatment effects on experts' macroeconomic expectations. The effects are weaker for countries with few visits of the US president. This heterogeneity in the treatment effects shows that the US incumbent change particularly impacted the expectations of experts from countries that are generally prioritized by US presidents. When we specifically examine the countries that have and have not been visited by Donald Trump between 2016 and 2020 conditional on prior visits since 1989, we find that, on average, the treatment effects are large and highly statistically significant for the countries Donald Trump has put at the bottom of his diplomatic priority list. The treatment effects for the sample of deprioritized countries suggest that experts from these countries expect political ties to regain strength under the new incumbent Joe Biden. Taken together, the effects conditional on presidential visits provide supplementary evidence in support of a political leadership effect.

3.6 Uncertainty in expectations

The results so far provide evidence for global political spillover effects regarding experts' point estimates of the future development of macroeconomic variables. But does the US incumbent change also influence experts' confidence in their own guesses? A recurring argument in political economy posits that if the uncertainty about electoral outcomes is resolved, agents feel more confident about their economic forecasts. To examine this mechanism in our setting, we design an approach to elicit "revealed" uncertainty levels of participants and examine whether the degree of uncertainty changed after the victory of Joe Biden. Modeling respondents' revealed uncertainty is motivated by a classical challenge in social surveys: when directly asking participants about how certain they are, it is unclear whether answers reflect true uncertainty or only reporting behavior. A further advantage of using probabilistic expectations over traditionally asked questions aiming at eliciting respondents' uncertainty (e.g., *"Do you think it is 'very likely', 'likely', 'unlikely', or 'very unlikely' that a specific event occurs?"*) is that they facilitate inter-personal comparability.

To measure revealed uncertainty, we supplement our questions regarding point estimates with a series of questions asking for the perceived distribution of possible future outcomes. Specifically, for each of our macroeconomic variables, we ask: *"Please indicate which probability you assign to the following* `[change of macroeconomic variable]` *in 2021"*. The presentation of these questions is shown in Figures C.1–C.4. The range of possible outcomes depends on the macroeconomic variable. For growth, we ask respondents to report their expected probability for an increase in real GDP for 14 possible outcomes: (<-6.0%); (-6.0% to -5.0%); (-5.0% to -4.0%); (-4.0% to -3.0%); (-3.0% to -2.0%); (-2.0% to -1.0%); (-1.0% to 0.0%); (0.0% to +1.0%); (+1.0% to +2.0%); (+2.0% to +3.0%); (+3.0% to +4.0%); (+4.0% to +5.0%); (+5.0% to +6.0%); and (>+6.0%). These bins encompass the whole range of outcomes that our macroeconomic variables may take, and we ask experts for their assessment regarding the percentage chance that a certain outcome may occur.

We use the resulting density forecast to calculate measures of dispersion that reflect revealed uncertainty levels. The main idea behind our strategy is that a higher variation of respondents' answers across the bins of our scale reflects greater uncertainty. In contrast, uncertainty is lower when experts assign large values to specific outcomes and fill out fewer bins. In the most extreme case, experts who assign 100% to a single bin are very certain about a specific outcome. Based on the probability density function for each expert, we calculate the coefficient of variation as a measure of uncertainty. Compared to other dispersion measures (e.g., the range of the variance), the coefficient of variation is less susceptible to small variations in the extreme values of experts' density forecasts.

Figure 3.3 plots histograms that show the unconditional distribution of probability masses across bins for each macroeconomic variable across treatment status. The unconditional distributions are likely to be affected by systematic differences in uncertainty across countries. Given that the distribution over bins requires some effort from respondents, considering the time participants put into answering the questions is important. To address these issues, we closely replicate our baseline empirical specification for point estimates via

$$U_{ie(t)} = \gamma T_{e(t)} + \eta_i + \zeta_{e(t)} + \mu_e + \varepsilon_{ie(t)} \quad , \tag{3.3}$$

where $U_{ie(t)}$ denotes our proxy for uncertainty. In Table 3.2, we present results for the coefficient of variation, our preferred uncertainty measure. Table C.16 in the Appendix provides complementary evidence using alternative dispersion measures (standard deviation, variance, and mean absolute deviation between the second and the fourth quintile). The main result is that the 2020 US presidential election did not reduce uncertainty. If anything, the election of Joe Biden has increased experts' uncertainty about their host country's economic condition in 2021.

Table 3.2 : The 2020 US presidential election and economic expectations of experts, effects on experts' uncertainty

Dependent variables: Uncertainty about key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.218* (0.126)	0.102 (0.124)	0.0970 (0.107)	0.0466 (0.156)
Number of Experts	740	708	690	574
Number of Countries	68	68	68	68
R-Squared	0.190	0.224	0.195	0.197
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	0.237* (0.142)	0.103 (0.138)	0.0984 (0.121)	-0.0284 (0.162)
Number of Experts	702	672	647	549
Number of Countries	67	67	67	67
R-Squared	0.196	0.230	0.197	0.195
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	0.154 (0.192)	0.307 (0.266)	0.116 (0.190)	0.742 (0.594)
Number of Experts	51	43	44	25
R-Squared	0.431	0.488	0.298	0.599
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). The table presents results on the effect of the US presidential election on experts' degree of uncertainty about these variables. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. "Country-FE" are fixed effect on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denoted the duration (in seconds) experts took to fill out their survey.

* Significant at the 10% level

Theories describing how the resolution of political uncertainty influences agents' expectations implicitly assume that agents are informed about the future policies of newly elected leaders. From a viewpoint of many international experts, however, the election strategy of Joe Biden was mainly built on voting out the incumbent Donald Trump. For many experts outside the United States, there was large uncertainty about the policies Joe Biden would pursue and how they may affect their host country. This explanation is consistent with the results in Table 3.2 showing that the election has increased uncertainty for experts outside the United States, but not for US-based experts.

3.7 Conclusion

Political leadership matters for the state of the economy (e.g., Jones and Olken, 2005; Besley et al., 2011; Brown, 2020; Easterly and Pennings, 2020) and the formation of national economic expectations (e.g., Treisman, 2011; Huberman et al., 2018; Coibion et al., 2021; Bachmann et al., 2021; Mian et al., 2021). In this study, we showed that exceptional politicians influence expected economic outcomes on a global scale. We used the US presidential election as a natural experiment to quantify the effect of the US president on global economic expectations. The impact of the US president on global macroeconomic expectations is large. Experts who have been informed that Joe Biden won the 2020 US presidential election expected real GDP growth in their country in 2021 to be 0.98 p.p. higher than experts polled prior to the election date.

Our finding adds a novel piece to the growing literature on the origins of expectation formation. While the economic mechanisms underlying the formation of economic expectations are increasingly well understood (e.g., Coibion and Gorodnichenko, 2015; Coibion et al., 2018a; Coibion et al., 2018b; Coibion et al., 2022), little was known about the political origins of economic expectations so far (Dräger et al., 2022). We show that even a single politician can influence macroeconomic expectations on a global scale. Politicians and businesses need to consider how foreign politicians and policies influence expectations in their country when making policy and investment decisions.

A promising avenue for future research is investigating channels through which the US president influences global macroeconomic expectations. Our evidence suggests that one mechanism is an expected increase in trade volumes under the Biden administration. Increased trade volumes may increase real GDP growth in trading partner countries. Another task for future research is examining the external validity of our findings. While our results based on the US president may reflect upper-bound estimates in light of US dominance in world politics, heads of other global powers such as China and Russia might also influence global macroeconomic expectations.

4 Democratic tornadoes? County-level evidence on the influence of local disasters on US gubernatorial elections

4.1 Introduction

Natural disasters influence electoral outcomes. The literature provides three reasons: First, voters might be irrational and blame the incumbent for the negative shock (e.g., Wolfers et al., 2002; Achen and Bartels, 2002, 2016; Leigh, 2009; Healy et al., 2010; Huber et al., 2012). Second, voters might reward or punish the incumbent depending on the incumbent's disaster relief effort (e.g., Healy and Malhotra, 2010; Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Cole et al., 2012; Neugart and Rode, 2021; Cooperman, 2022). Third, voters might learn new information from the incumbent's crisis response and update their beliefs about the incumbent (e.g., Ashworth et al., 2018; Cerqua et al., 2021; Masiero and Santarossa, 2021). I provide evidence for a fourth reason how natural disasters influence electoral outcomes: The shock itself changes voters' preferences.

I study how deadly tornadoes influenced gubernatorial elections in the United States from 1960 to 2020. I combine county-level election data with geospatial information on the incidence of tornadoes. Similar to Deryugina and Marx (2021), I only use lethal tornadoes, allowing me to identify events that affected populated areas and caused, on average, tens of millions of USD worth of damage. Tornadoes make useful natural experiments because (i) they vary over space and time, (ii) are limited in their geographic scope, and (iii) are short-lived and unpredictable. The National Oceanic and Atmospheric Administration (NOAA) maintains a database capturing tornado-related fatalities, injuries, and property damage of each reported tornado since 1950. I employ an event study design that compares election outcomes of counties with a deadly tornado experience since the last election to counties without in the same state. My identifying assumptions are that (i), conditional on a host of fixed effects and controls, tornado fatalities are unrelated to any unobservable determinants of voting behavior and (ii) voters do not anticipate tornadoes and change their voting behavior before the event. I find no pre-trends in voting behavior, supporting the parallel trends assumption.

My main result is that Democrats benefit from deadly tornadoes. The Democratic vote share increased by 0.43 percentage points (p.p.) after a deadly tornado. The effect increases with treatment intensity. Deadly tornadoes within 12 months before the election and tornadoes causing more fatalities trigger a stronger response by the electorate. In my baseline event study specification, I only include counties experiencing their first deadly tornado since 1950 in the treatment group. Inference does not change when I allow for multiple treatments. An advantage of the event study design is the possibility of studying treatment effect persistence. In the baseline specification of my event study, voters react immediately and strongly to a deadly tornado. The effect partly carries over to subsequent elections. This finding is similar to Bechtel and Hainmueller (2011), who find a 25% effect size persistence in the second election after the 2002 Elbe flooding in Germany.

My study setup allows me to examine the underlying mechanism of why Democrats benefit from deadly tornadoes in gubernatorial elections. I control for governor incumbency effects by investigating open seat elections. The baseline results are confirmed when sampling challenger races only, indicated by an increase of the two-party vote share for the Democratic candidate of more than half a p.p. in treated counties. To account for potential party incumbency effects, I present separate results for treated counties under a Democratic and Republican governor. Deadly tornadoes do not seem to influence voting behavior when a Republican governor is in power. Under a Democratic governor, the effect size is about twice as large as in the baseline specification. These results suggest that two mechanisms work either in conjunction or against each other. First, and in line with the literature, I observe a party incumbency mechanism: The candidate of the incumbent party benefits from the governor's disaster response in the subsequent election. Second, I still observe a shift in voters' preferences toward the policy platform offered by Democratic candidates. My evidence suggests that the strength of the two mechanisms is about equal in magnitude.

Federal intervention is a threat to identification when investigating the effect of local disasters on electoral outcomes. To rule out that the increase in the Democratic vote share is driven by a comparative advantage of Democratic governors in attracting federal disaster relief money, I only consider deadly tornadoes without a presidential disaster declaration. Treatment intensity as measured by a tornado's devastating impact decreases, but the inference does not change for the subsample analysis.

My study contributes to the literature on voters' responses to negative exogenous shocks, such as natural disasters and economic shocks.[1] Empirically, results are mixed. So far, most

[1] Natural disasters: Abney and Hill (1966), Achen and Bartels (2002), Healy et al. (2010), Healy and Malhotra (2010), Bechtel and Hainmueller (2011), Gasper and Reeves (2011), Cole et al. (2012), Chen (2013), Heersink et al.

studies focus on incumbent vs. challenger races and not on the party affiliation of candidates. Several studies find that the incumbent's electoral fortune is unaffected by an exogenous shock (e.g., Abney and Hill, 1966; Ebeid and Rodden, 2006; Kayser and Peress, 2012), many find negative effects (e.g., Wolfers et al., 2002; Achen and Bartels, 2002, 2016; Leigh, 2009; Healy et al., 2010; Gasper and Reeves, 2011; Cole et al., 2012; Heersink et al., 2017), and some find a positive effect of exogenous shocks on the incumbent's vote share (e.g., Bechtel and Hainmueller, 2011; Chen, 2013; Masiero and Santarossa, 2021; Neugart and Rode, 2021). Little is known about how the candidates' party affiliation and policy platform influence voters' response to natural disasters.[2] I depart from the existing literature by providing evidence for a new mechanism linking negative exogenous shocks to a change in voting behavior via altered policy preferences.

My study also contributes to the literature on experience-based preference formation. Older evidence on how natural disasters affect risk aversion is mixed (see Chuang and Schechter (2015) for a literature review). However, more recent studies consistently suggest that people become more risk averse after experiencing floods (Cameron and Shah, 2015), earthquakes (Cameron and Shah, 2015; Hanaoka et al., 2018), tsunamis (Cassar et al., 2017), cyclones (Brown et al., 2018), rainfall-shocks (Di Falco and Vieider, 2022), or any weather-related disaster (Bourdeau-Brien and Kryzanowski, 2020; Johar et al., 2022).

Methodologically, my study advances research on the granular spatial analysis of voter behavior. Few studies have analyzed nationwide electoral behavior at the county level over a long time period.[3] This is largely due to limitations in data availability, especially for non-federal elections (Darmofal and Strickler, 2019). Most researchers rely on congressional districts to examine the distribution of voters within a state.[4] Amlani and Algara (2021) compiled a comprehensive dataset of county-level US gubernatorial election outcomes, which I use in this study.

(2017), Heersink et al. (2020), Masiero and Santarossa (2021), Cerqua et al. (2021), Neugart and Rode (2021), and Cooperman (2022). Economic shocks: Wolfers et al. (2002), Ebeid and Rodden (2006), Leigh (2009), and Kayser and Peress (2012).

[2] An exception is Heersink et al. (2020), who examine the effects of Hurricane Sandy on the 2012 presidential election and show that those counties previously supporting Obama reacted far more positively to disaster damage than those that had earlier opposed him ("partisan retrospection").

[3] If studies employ county-level electoral data, it is mostly from US presidential elections (see, for example, Frendreis et al., 1990; Knack and Kropf, 2003; Gomez et al., 2007; Ambrosius, 2016; Darmofal and Strickler, 2019).

[4] One notable exception is Hopkins (2018), who analyzes, among other things, county-level election returns from US gubernatorial elections starting as early as 1928.

The remainder of this paper is organized as follows. In the next section, I introduce a spatial model of voting with valence and give some empirical context. Section 4.3 provides background information on US governors, US gubernatorial elections, and tornadoes. In Section 4.4, I present the data and show descriptive evidence. I formalize my empirical strategy in Section 4.5 and present my results in Section 4.6. Section 4.7 discusses my findings. Section 4.8 concludes.

4.2 Theory and empirical context

Crisis experiences change preferences. The change in preferences might change voters' preferred policy. Equation 4.1 gives a spatial model of voting with valence, where voter i's utility from supporting candidate j is some combination of policy preferences on issue k and the candidate's valence attributes V_j.[5]

$$u_{i,j,k} = -(1-w)\underbrace{(|\theta_{j,k} - \theta^*_{i,k}|)^\alpha}_{\text{Policy}} + w\underbrace{(|V_j - V^*_i|)^\beta}_{\text{Valence}} \quad , \tag{4.1}$$

$\theta_{j,k} - \theta^*_{i,k}$ predicts that voter i's utility is declining in the distance between the voter's preferred policy ($\theta^*_{i,k}$) and the policy platform of candidate j on issue k ($\theta_{j,k}$) (spatial component). α parameterizes the shape of the decline. $V_j - V^*_i$ predicts that voter i's utility is declining in the distance between the voter's preferred valence attributes of the next governor (V^*_i) and the candidate's valence attributes (V_j) (valence component). Valence attributes are non-policy candidate qualities, such as charisma, competence, and trustworthiness.[6] β parameterizes the shape of the decline. w is a parameter assigning relative weights to the policy and the valence component of the model.

Studies in political science have focused on different parts of the model. One branch of the literature suggests that crisis-induced anxiety shifts the voter's preferred policy toward "protective policies", benefiting conservative candidates whose policy platforms are more likely to emphasize these dimensions (Stenner, 2005; Getmansky and Zeitzoff, 2014; Albertson and Gadarian, 2015; Campante et al., 2020). However, disentangling the policy and the valence

[5] Spatial and valence models of voting are central theories explaining voting behavior (Downs, 1957; Stokes, 1963; Manin, 1995; Krehbiel, 1998; Ansolabehere and Snyder, 2000; Sanders et al., 2011). This model is based on Bisbee and Honig (2022).

[6] Valence theory identifies three principal heuristics that voters employ: Evaluation of party leaders' competence, attitudes about which party can handle the most important problems facing the country best, and party identification (Sanders et al., 2011).

component is nontrivial as conservative candidates may also benefit because voters prioritize “leadership” and “strength”, valence attributes more associated with conservatism, in times of crises (Holman et al., 2016, 2019).

My empirical investigation builds on the model in Equation 4.1 and allows me to exclude a valence mechanism because I compare treated counties to non-treated counties within the same state. The candidates running for the gubernatorial office are the same across intrastate treatment and control groups. It is unlikely that the candidates’ valence characteristics (V_j) change after a deadly tornado because tornadoes are local disasters limited in their geographic scope. However, even if the candidates’ valence characteristics change after a deadly tornado, the change equally affects treated and control counties.

Voters in treated counties might change their preferred valence attributes of the next governor (V_i^*) after a deadly tornado. Intuitively, they would place relatively more weight on leadership qualities in their electoral decision compared to voters in control counties. My study setup mitigates concerns that the results are driven by the model’s valence component because my observation period spans 60 years with almost 1,000 US gubernatorial elections. It is plausible that individual valence characteristics are close to randomly distributed among Democratic and Republican candidates in my sample. If anything, Republicans possess more valence qualities that are favored in times of crises, which would downward-bias the magnitude of the effect (see Holman et al., 2016, 2019). I further subject my results to several robustness checks and show that my findings hold in open seat elections. Individual valence characteristics of the incumbent cannot influence the electoral results of open seat elections. I also find that results hold when splitting the sample to account for a party incumbency mechanism by investigating elections with Democratic and Republican incumbency separately.

Holding the model’s valence component constant across voters in treated and control counties leaves the policy component to explain differences in voting behavior between treated and non-treated voters. The policy component again consists of two parts: The voter’s preferred policy ($\theta_{i,k}^*$) and the policy platform of candidate j ($\theta_{j,k}$). It is unlikely that a deadly tornado in a county affects the state-wide policy platform of the gubernatorial candidates. However, even a change in the policy platform of candidate j ($\theta_{j,k}$) after a deadly tornado would affect voters’ utility in treated and control counties equally, given that voters in treated and control counties are not systematically different from each other before the deadly tornado.[7]

[7] The pre-treatment trends of my event study design show that voters in treated and control counties do not differ in their voting behavior before the treatment (see Section 4.6).

What is left is the voter's preferred policy ($\theta^*_{i,k}$) to explain differences in voting behavior between treated and non-treated voters. Recent studies have shown that risk aversion increases after natural disasters (Cameron and Shah, 2015; Cassar et al., 2017; Hanaoka et al., 2018; Brown et al., 2018; Bourdeau-Brien and Kryzanowski, 2020; Di Falco and Vieider, 2022; Johar et al., 2022). Increased risk aversion makes voters demand more social insurance and increased protection from a negative economic shock. The model of political cycles by Pastor and Veronesi (2020) predicts that agents elect Democrats – the party promising more social security and redistribution – when risk aversion is high.[8] Therefore, my hypothesis to be tested empirically is:

Hypothesis 1. *Democratic candidates benefit from deadly tornadoes in US gubernatorial elections.*

I expect that the voter's preferred policy changes after experiencing a deadly tornado. Voters demand more social insurance and better protection from a negative economic shock. The voter's preferred policy moves closer toward the policy platform offered by the Democratic candidate because Democrats promise more social insurance. Vice versa, the voter's preferred policy moves further away from the policy platform of the Republican candidate because the Republican party advocates for a smaller government and less social security. On average, the treated voter increases her utility by voting for the Democratic candidate.

4.3 Background information

4.3.1 State government and the governor

State governments shape economic policy-making. They set tax rates, decide on state spending and running deficits, establish minimum wages, cap the maximum weekly unemployment benefits, and so on. In line with the partisan theory (Hibbs, 1977; Alesina, 1987), Democratic state governments spend relatively more, run larger deficits, adopt more encompassing Medicare coverage, were more active in promoting universal care, and redistributed income more than Republican governments (see Potrafke (2018) for a survey).

Governors head the state government and have far-reaching powers (Beyle et al., 2008), including budgetary and veto powers, executive orders, and informal powers (see Bernick

[8] Pastor and Veronesi (2020) also show that Democratic voters are more risk averse. I propose that the increase in the Democratic two-party vote share after a deadly tornado is driven by an increase in risk aversion and a higher demand for social insurance.

(2016) for a survey). They are also responsible for appointing officials and judges, and make legislative proposals. Governors thus have significant influence over the direction of the state budget and policy environment, and powers may go as far as circumventing the state legislature (Cahan, 2019). Term limits of US governors play a crucial role in identifying the importance of the governor for state policy-making. Besley and Case (1995) find that governors facing a binding term limit behave differently than those who are able to run again.

The partisan affiliation of governors is highly relevant for policy-making. For instance, only Republican governors have rejected the expansion of Medicaid as part of the Affordable Care Act (Haeder and Weimer, 2013; Barrilleaux and Rainey, 2014). If a Democrat is in office, taxes, spending, and other policy instruments respond to a binding term limit. Similarly, Sieg and Yoon (2017) show that the governor's ideology has a large impact on taxes. Fiscal conservatives prefer lower expenditures and lower taxes than fiscal liberals.

The governor is, by the state constitution, the most prominent actor in response to a disaster. He decides on mobilizing state resources to mitigate the adverse effects of a disaster. If the required disaster response efforts surpass the state's available resources, the governor may opt to request additional resources from the federal government. The decision of whether to request a disaster declaration directly from the president lies with the governor. The *Stafford Act* (1988) determines the process: After a disaster, the state's governor initiates a preliminary damage assessment. If a disaster appears to overwhelm local and state capacities, he can send an official disaster declaration request to the president (United States Congress, 1988).[9]

4.3.2 US gubernatorial elections

US gubernatorial elections take place in November.[10] Every year, the governorship is up for election in a subset of the states: 36 states hold their gubernatorial election in midterm years, nine states hold them in presidential election years, and five hold them in odd-numbered years.[11] These election schedules have changed little over the past 60 years. Changes in election timing were Florida 1964 and Illinois 1976, switching from presidential election years

[9] The Disaster Relief Act Amendments of 1974 (Public Law 93-288) codified the procedures of the presidential disaster declaration process. According to Lindsay and McCarthy (2015, p. 20), the fundamentals of the process have been in place since 1950 and "changed very little over time".

[10] In the rare case of a special or recall election, the election might not take place in November (see following footnote). In general, incumbents cannot decide when elections take place.

[11] Exception: Special elections and recall elections. In these cases, the winner finishes the current term – a new election will still be held according to the state's fixed schedule (Cahan, 2019). Recent examples of special elections are Utah 2010 (incumbent resigned), West Virginia 2011 (incumbent died), and Oregon (incumbent resigned). Recent examples of recall elections are California 2003 (electricity crisis), Wisconsin 2012 (incumbent's restrictions on unions), and California 2021 (incumbent's personal behavior and leadership).

to midterm years, and Louisiana 1975 moving back one year relative to presidential election years.

A Governor's term lasts four years, except for New Hampshire and Vermont, where terms are two years.[12] Most governors were subject to term limits between 1960 and 2020. Gubernatorial term limits are prescribed by the state constitution.[13] If states have a gubernatorial term limit, it is mostly two terms, except for Virginia, where incumbents cannot stand for re-election. In a few states, governors can also serve a maximum of eight out of 12 or 16 years. Some states offer the possibility of re-eligibility after four years out of office. Term limits increase the number of challenger races.[14]

4.3.3 Tornadoes

A tornado is a "mobile, destructive vortex of violently rotating winds having the appearance of a funnel-shaped cloud and advancing beneath a large storm system" (Oxford English Dictionary, 2022). According to the National Weather Service, tornadoes are "nature's most violent storms" (National Oceanic and Atmospheric Administration, 2015). They can cause fatalities and devastate a neighborhood in seconds. Tornadoes can be more than three kilometers in diameter and stay on the ground for more than 100 kilometers. Wind speeds can attain more than 480 kilometers per hour. However, most tornadoes are much weaker and do not harm people or structures.

Tornadoes are very common in the United States. While tornadoes disproportionally affect regions of the South and Midwest known as "Tornado Alley", they have been observed in every US state. Tornadoes make useful natural experiments because they (i) vary over space and time, (ii) are limited in their geographic scope, and (iii) are short-lived and unpredictable.

4.4 Data

4.4.1 US gubernatorial elections

My data on US gubernatorial elections come from Amlani and Algara (2021). They provide county-level data on presidential, senatorial, and gubernatorial elections from 1872 to 2020. The main data sources are raw county-level election returns from *Congressional Quarterly*

[12] Switched from two-year to four-year terms: Massachusetts, Michigan, Minnesota, Nebraska, and North Dakota in the 1960s; Iowa, Kansas, Louisiana, New Mexico, South Dakota, Texas, and Wisconsin in the 1970s; Arkansas in 1986; Rhode Island in 1994.

[13] An exception is Wyoming, where term limits are given by its statutes.

[14] As the name suggests, challenger races or open seat elections are elections without an incumbent candidate.

and the *United States Historical Election Returns* provided by the Inter-university Consortium for Political and Social Research (ICPSR Study 1). The data captures 98.5% (2461/2499) of all gubernatorial elections.[15]

I only employ data from 1960 onward to ensure that counties in the control group did not experience a deadly tornado for at least 10 years.[16] Excluding the years before 1960 also mitigates concerns due to (i) major changes in the timing of gubernatorial elections and term limits in the years before 1960 (Besley and Case, 1995), (ii) changes to the Democrat's and Republican's party ideology in the immediate years following World War II, and (iii) data quality and completeness.[17] I do not consider first-round gubernatorial elections if there is a general election run-off. This is relatively rare and only affects the gubernatorial elections in Arizona (1991) and Louisiana (1979, 1991, 2003, 2015, 2019). The final dataset includes 812 gubernatorial elections in 48 states.[18]

The data includes information on the date of the election, state, the name of the county, candidates' names, the total number of votes, the number of votes for the Democratic candidate, the number of votes for the Republican candidate, the Democratic two-party vote share, and incumbency status.[19] Panel A of Table 4.1 shows county-level summary statistics for total votes, Democratic votes, Republican votes, and the Democratic two-party vote share. On average, the number of total votes per county is 23,880, while much fewer voters are cast in the majority of counties. The mean of Democratic votes is higher than the mean of Republican votes. The opposite is true for the median. This suggests that voters in rural areas with fewer votes per county vote relatively more Republican, whereas voters in counties with many voters (mostly urban counties) vote relatively more for Democratic candidates.

[15] Of the 38 gubernatorial elections missing, only 10 would be of interest to this study as they are conventional two-party contested elections. These include Nebraska 1866, Texas 1866, New Jersey 1877, New Hampshire 1878, Virginia 1889, Vermont 1892, California 1898, Kentucky 1900 (special), Delaware 1928, and Kentucky 1954 (Amlani and Algara, 2021).
[16] See Section 4.5 for the estimation strategy and Section 4.6 for the pre-treatment trend results.
[17] No two-party contested election is missing, and data is available for most counties.
[18] I exclude US gubernatorial elections in Alaska and Hawaii because county-level electoral data is incomplete. However, no deadly tornado struck a county in Alaska or Hawaii between 1950 and 2020. Some non-two-party contested elections are not available in the data, such as those with major independent candidates (e.g., Maine 1974, Utah 1992, and Texas 2006), major third-party candidates (e.g., New York 1990, Minnesota 1998, and Colorado 2010), missing Democratic or Republican candidates (e.g., Alabama 1962, Tennessee 1966, and Illinois 1986), and multiple candidates from a major party (Alabama 1970 and Louisiana 1987). I include all available non-two-party contested elections in my analysis, as third-party or independent candidates affect election outcomes in treated and comparison group counties equally and do not introduce bias into my results.
[19] The Democratic two-party vote share is the number of votes for the Democratic candidate relative to the total number of votes for the Democratic and the Republican candidate.

Table 4.1 : County-level summary statistics

Variable	Mean	Median	Std. Dev.	Max.	Obs.
Panel A: Gubernatorial election outcomes					
Total votes	23,880.45	6,833	75,537.07	2,941,101	49,549
Dem. votes	11,713.55	3157	41,324.83	2,114,699	49,549
Rep. votes	11,537.54	3366	34,572.56	1,389,995	49,549
Dem. two-party vote share (in %)	48.86	48.10	16.32	100.00	49,549
Panel B: County characteristics					
Total population	74,444.58	21,006	254,252.3	9,818,605	49,549
Size (sqkm)	2,523.27	1,658.42	3,387.02	52,072.34	49,549

Notes: Panel A reports the following county-level summary statistics for US gubernatorial election outcomes: Total votes, Democratic votes, Republican votes, and the Democratic two-party vote share. Panel B reports summary statistics for the counties' population and the counties' size. Source: Amlani and Algara (2021).

Amlani and Algara (2021) also report population totals and the size of each county.[20] Examining population levels and density is useful because it describes how rural or urban a county is. Panel B of Table 4.1 gives summary statistics for the total population and the size of a county. 74,444 people lived in an average county between 1950 and 2020. A county's size is, on average, 2,523 square miles, about eight times the City of New York.

4.4.2 Tornadoes

My data on tornadoes come from the NOAA Tornado Database, which tracks the path of every known tornado since 1950.[21] The database reports the number of fatalities and injuries for each tornado, along with the counties affected and the date and time when it occurred. The majority of tornadoes reported between 1950 and 2020 caused no known property damage, fatalities, or injuries (see Panel A of Table 4.2). It is unlikely that these tornadoes have triggered a response by the electorate. In line with Deryugina and Marx (2021), I restrict my sample to tornadoes that are most likely to have prompted a response by voters: Those that caused at least one fatality.[22]

[20] The population data is taken from the US census and updated every 10 years.
[21] Available from `https://www.spc.noaa.gov/wcm/#data`.
[22] The database also provides damage estimates. However, these measures are very imprecise and thus much noisier measures of severity than fatalities. Uncertain damage is recorded as zero damage in the data, making

Table 4.2 : Tornado-level summary statistics

Variable	Mean	Median	Std. Dev.	Max.	Obs.
Panel A: All tornadoes					
Fatalities	0.08	0	1.39	158	66,244
Injuries	1.34	0	17.47	1,740	66,244
Property damage (millions USD)	3.11	0	52.43	2,800	8,438
Panel B: Deadly tornadoes					
Fatalities	4.20	1	9.36	158	1,681
Injuries	46.32	11	119.04	1,740	1,681
Property damage (millions USD)	50.74	2	251.34	2,800	327

Notes: The table reports fatalities, injuries, and property damage for all (deadly) tornadoes in Panel A (B). Data on property damage is taken from Deryugina and Marx (2021) and covers the years 2002 to 2017. Source: National Oceanic and Atmospheric Administration (2022).

Panel B of Table 4.2 shows summary statistics for the 1,681 deadly tornadoes in my sample: On average, a deadly tornado kills more than four people, insures 46, and causes more than 50 million USD in property damage. The mean (median) amount of federal disaster aid per tornado fatality was 13.3 (2.98) million USD in 2008 dollars (Deryugina and Marx, 2021).[23]

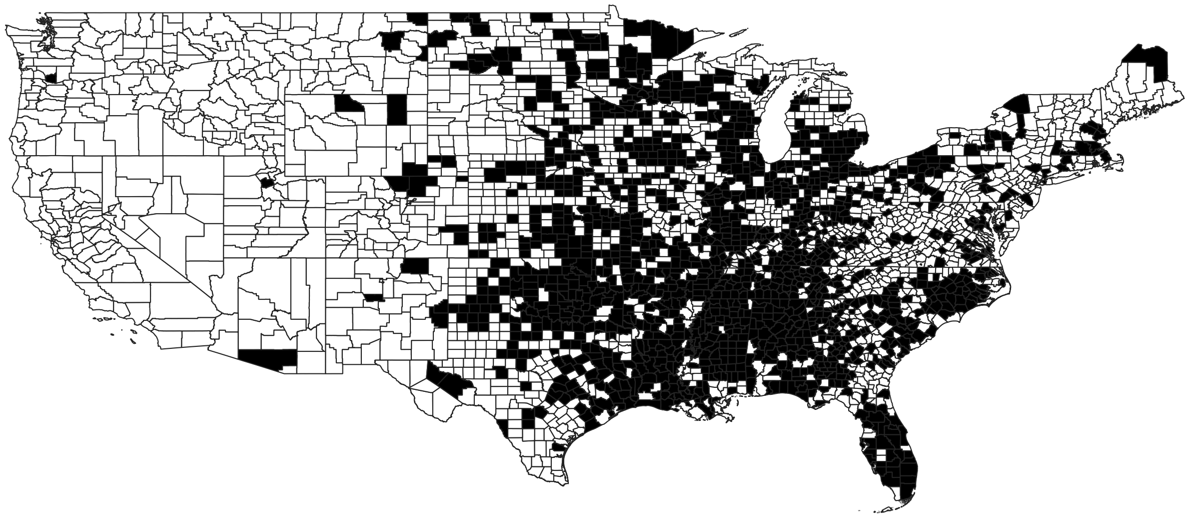
Figure 4.1 displays the counties affected by at least one deadly tornado between 1950 and 2020. 1,389 counties in my dataset experienced at least one deadly tornado.[24] This is equivalent to more than 40% of all US counties. Almost half of the counties hit by a deadly tornado were struck multiple times. In total, my dataset includes 2,499 county observations with a deadly tornado. Most deadly tornadoes only affect one county and even the most devastating ones rarely pass through more than a couple of counties. While deadly tornadoes strike most often in the central United States, an area known as “Tornado Alley”, they occurred in 42 states during the observation period.

it impossible to distinguish uncertain damage from true zeroes. While injuries may also indicate severity, no distinction is made between minor and major injuries, making them less comparable than fatalities.

[23] Deryugina and Marx (2021) use FEMA data (`https://www.fema.gov/disasters`). They divide the total state-year disaster aid for tornado-related disasters by the corresponding number of tornado fatalities.

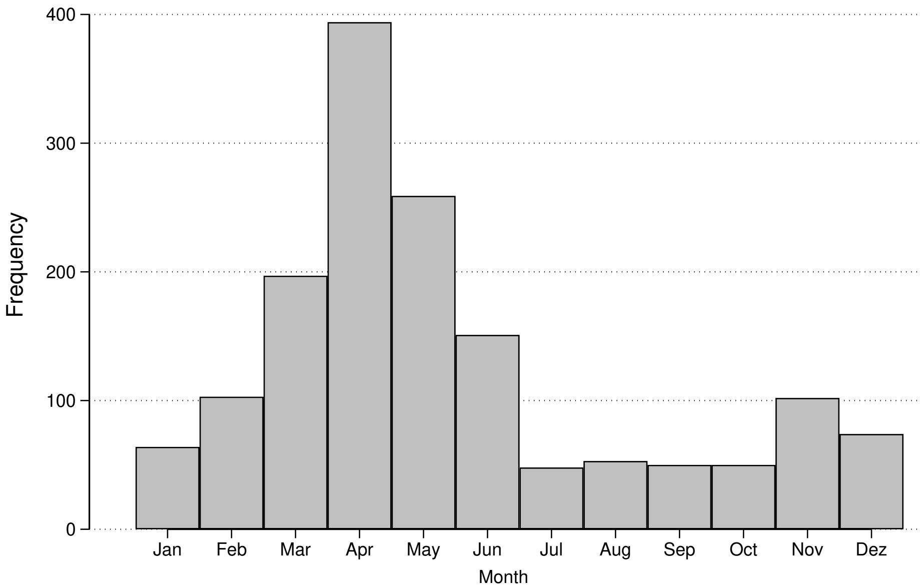
[24] See Table D.2 in the Appendix for the frequency of deadly tornadoes by county. The total number of counties in my dataset is 3,108).

Figure 4.1 : Deadly tornadoes in the United States, 1950–2020



Notes: The figure shows all reported deadly tornadoes in the United States from 1950 to 2020 by county. Counties which experienced a deadly tornado between 1950 and 2020 are colored black. Counties with no deadly tornado between 1950 and 2020 are colored white. Data for Alaska and Hawaii are not reported because no deadly tornado struck a county in Alaska or Hawaii between 1950 and 2020. Source: National Oceanic and Atmospheric Administration (2022).

Figure 4.2 : Deadly tornadoes by month

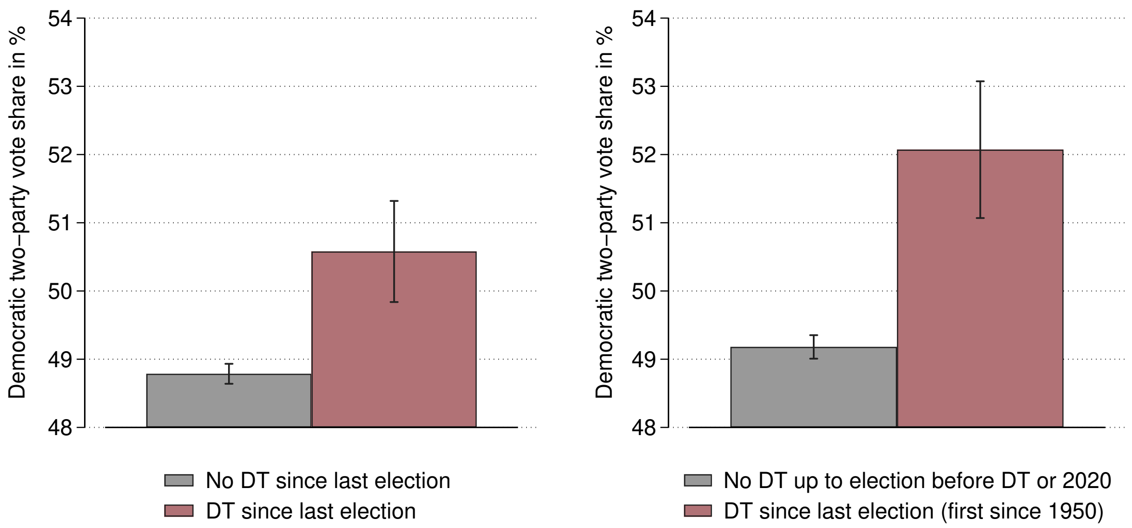


Notes: The figure shows all reported deadly tornadoes in the United States from 1950 to 2020 by month. Source: National Oceanic and Atmospheric Administration (2022).

Figure 4.2 shows the frequency of deadly tornadoes by month. Deadly tornadoes can strike at any time of the year. However, they occur most frequently around April and May, with NOAA defining the so-called "tornado season", depending on the region, as the months from March to July. Comparably few deadly tornadoes happen in fall and therefore in very close proximity to state or federal elections. This setup allows me to capture the true effect of a deadly tornado on voter behavior. If a deadly tornado strikes in an election year, it most likely occurs four to eight months before the election. Voters have time to digest the effects of the tornado, allowing their preferences to change, which then translates into a shift in their preferred policy. Concurrently, the timeline leaves little to no room for idiosyncratic confounding events that affect the treated county differently than other state counties. My event study design further mitigates concerns that effects are driven by confounding events.

4.4.3 Descriptive statistics

Figure 4.3 : Voting behavior in counties with and without a deadly tornado since the last gubernatorial election



Notes: DT = deadly tornado. The left panel allows for multiple treatments and shows the Democratic two-party vote share in gubernatorial elections for counties with a DT since the last gubernatorial election (red) and counties without a DT since the last gubernatorial election (grey). The right panel shows the Democratic two-party vote share in gubernatorial elections only for counties which experienced a DT since the last gubernatorial election with that tornado being the first lethal one since 1950 (red) and counties which did not experience a DT from 1950 to either the election before the DT or the end of the sample period in 2020 (grey).

Figure 4.3 illustrates a comparison in gubernatorial electoral outcomes between counties which experienced a deadly tornado since the last gubernatorial election and counties which did not experience a deadly tornado since the last gubernatorial election. The left panel shows the Democratic two-party vote share in gubernatorial elections for counties with a deadly tornado since the last gubernatorial election (red) and counties without a deadly tornado since the last gubernatorial election (grey). The difference in the means between the two groups is 1.793 p.p., implying that voters in counties hit by a deadly tornado before the gubernatorial election are more likely to vote for the Democratic candidate than voters in counties with no deadly tornado before the election. The difference between the two groups is statistically significant (see Table D.1, Panel A in the Appendix for t-test results).

The right panel of Figure 4.3 shows the Democratic two-party vote share in gubernatorial elections only for counties which experienced a deadly tornado since the last gubernatorial election, with that tornado being the first lethal one since 1950 (red) and counties which did not experience a deadly tornado from 1950 to either the election before the deadly tornado or the end of the sample period in 2020 (grey). The difference in the means between the two groups is even larger than before, suggesting that counties which experience a deadly tornado for the first time respond even stronger by voting relatively more Democratic (the difference in means between the two groups is 2.891 p.p. and statistically significant, see Panel B of Table D.1 in the Appendix for t-test results).

4.5 Estimation strategy

My empirical strategy is designed to examine whether deadly tornadoes have influenced US gubernatorial election outcomes. This is a situation with staggered treatment timing and the possibility of multiple events per unit. For identification, I follow von Bismarck-Osten et al. (2022) and propose a difference-in-difference design, assuming that the true causal model for the Democratic two-party vote share (Y_{ct}) in county c in election t is

$$Y_{ct} = \alpha_c + \beta_t + \tau_{ct} D_{ct} + \varepsilon_{ct} \quad . \tag{4.2}$$

In Equation 4.2, α_c and β_t capture the county and election fixed effects (absorbing time-invariant county characteristics and election-specific characteristics, such as candidate quality). $D_{ct} = 1[t \geq E_c]$ is the indicator that the county is "treated" (i.e., a deadly tornado hit the county), where in the baseline E_c is the election when county c is treated for the first time since 1950. Further, τ_{ct} captures the "treatment effect" that is, the impact of the deadly tornado on the Democratic two-party vote share, while ε_{ct} is the residual such that $E[\varepsilon_{ct}|\alpha_c, \beta_t, D_{ct}] = 0$. The parallel trends assumption in the model predicts that, absent the treatment, the expected outcome is $\alpha_c + \beta_t$. Equation 4.2 also allows for heterogeneous treatment effects by county and election.

To derive the treatment effect, I apply the "imputation" estimator by Borusyak et al. (2022). Estimating Equation 4.2 as a conventional event study, meaning by ordinary least squares (OLS) with two-way fixed effects and some lags and leads of treatment, produces unreliable estimates in presence of effect heterogeneity.[25] To overcome biased estimators in event study designs, recent studies propose several robust estimators (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). The "imputation" estimator has some attractive properties, such as efficiency, transparency, and conservative standard errors. I compute the estimator using the *did_imputation* Stata command provided by Borusyak et al. (2022).

I follow von Bismarck-Osten et al. (2022) in constructing the imputation estimator in three steps. First, the county and election fixed effects α_c and β_t are estimated by OLS on the subsample of untreated observations only ($D_{ct} = 0$) using data until the election before the deadly tornado in each county. Second, I obtain an unbiased estimate $\hat{\tau}_{ct} = Y_{ct} - \hat{\alpha}_c - \hat{\beta}_t$ for each treated observation. While treatment effects for each election and county cannot be estimated consistently, Borusyak et al. (2022) show that averages of $\hat{\tau}_{ct}$ across many observations can, given appropriate regularity conditions. Any such average of interest can therefore be reported in the third step. Here, I deviate from von Bismarck-Osten et al. (2022) in that I am not interested in the average effect on a number n of elections after the treatment but only in the average effect on the first election after the treatment:[26]

$$\hat{\tau} = \frac{1}{|C|} \sum_{c \in C} \hat{\tau}_{c,E_c} \quad , \tag{4.3}$$

where C is the set of counties c observed in period E_c. The imputation estimator leverages all difference-in-differences contrasts between some county c in period E_c relative to periods before treatment, $t < E_c$ (reference periods) and relative to other counties which have not been treated yet by E_c.

In my baseline specification, I start out by estimating the effect of the first deadly tornado in a given county on the Democratic vote share. To provide empirical support for the assumption of parallel trends, I follow Borusyak et al. (2022) and estimate the regression on the set of untreated observations only:

[25] See Borusyak and Jaravel (2017); Strezhnev (2018); De Chaisemartin and d'Haultfoeuille (2020); Goodman-Bacon (2021).

[26] I also investigate the average effect on the second, third, and fourth election after the treatment but not as an average across multiple subsequent elections.

$$Y_{ct} = \alpha_c + \beta_t + \sum_{k=-K}^{-1} \mu^k \mathbb{1}(t - E_c = k) + \varepsilon_{ct} \quad , \tag{4.4}$$

The event-time coefficient μ^k traces the differential time path of the Democratic two-party vote share in treated relative to untreated counties before and after the first deadly tornado. Specifically, estimates $\mu^k; \tau \geq 0$ represent the average effect of the first deadly tornado on the Democratic two-party vote share in election $k \equiv t - E_c$ after the event. I estimate all event-time indicators and report the coefficients for the five elections before and the four elections after the treatment. Throughout the paper, I report standard errors clustered at the county level.

Parallel trends: One may worry about the plausibility of the parallel trends assumption in my setting. That is, counties belonging to different tornado-prone regions within a state might be on different voting behavior paths.[27] The proposed approach for pre-trend testing by Borusyak et al. (2022) employed here deviates from the conventional method, in which pre-trend or placebo coefficients are estimated simultaneously with the treatment effect $\hat{\tau}$. The advantages are (i) separating the validation of the design (i.e., of the ex-ante assumption of parallel trends) from the estimation given the design, (ii) improved efficiency of treatment effect estimation as all untreated observations are utilized in the imputation process, and (iii) the removal of the correlation between the treatment effect and the pre-trend estimators. The latter prevents potential bias from following the conventional practice of trusting the results only conditionally on the pre-trend test passing (Roth, 2022). This strategy should assuage concerns about violations of the parallel trends assumption in my setting.

Multiple treatments per unit: Counties may be hit more than once by a deadly tornado over time. Indeed, 613 counties in my dataset experienced a deadly tornado more than once between 1950 and 2020.[28] This poses a potential threat to the causal identification of deadly tornado effects on voting behavior. If each deadly tornado is viewed as a separate event with its own persistent effects it would be impossible to distinguish between the effects of different past deadly tornadoes.

I propose three solutions to mitigate concerns that the estimated effect is biased because of past treatments. First, in my baseline sample, I exclude county-election observations

[27] While there is substantial heterogeneity in the likelihood of deadly tornado experience across counties in the United States, the difference decreases substantially when only comparing counties within the same state (see Figure 4.1).

[28] See Adda (2016) for a similar setup with multiple treatments per unit.

after a second deadly tornado. Hence, I only consider counties which have been treated *for the first time since 1950* (see Section 4.6.1). As I employ election data from 1960 onward, I leave sufficient time for treatment effects to "cool off" if counties were experiencing a deadly tornado before 1950. Second, and related to the "cooling off" period of treatment effects, I investigate the treatment effect's persistence. I believe it is reasonable to assume that the effects of a negative shock caused by a deadly tornado do not last more than a few years for most people. Third, I also exploit the randomness of the relative timing a deadly tornado has with respect to US gubernatorial elections (see Section 4.6.2). Effect transitoriness suggests that tornadoes close to elections would have a larger effect on voting behavior.

4.6 Results

In Section 4.6.1, I report the baseline results for estimating Equation 4.4 with the sample of counties which have been struck by a deadly tornado *for the first time since 1950*. Never-treated and later-treated counties constitute the comparison group. In Section 4.6.2, I investigate the influence of varying treatment intensities across deadly tornadoes on voting behavior. Treatment intensity increases with the number of fatalities and with proximity to the election. In Section 4.6.3, I analyze how voting behavior reacts to deadly tornadoes when allowing for multiple treatments.

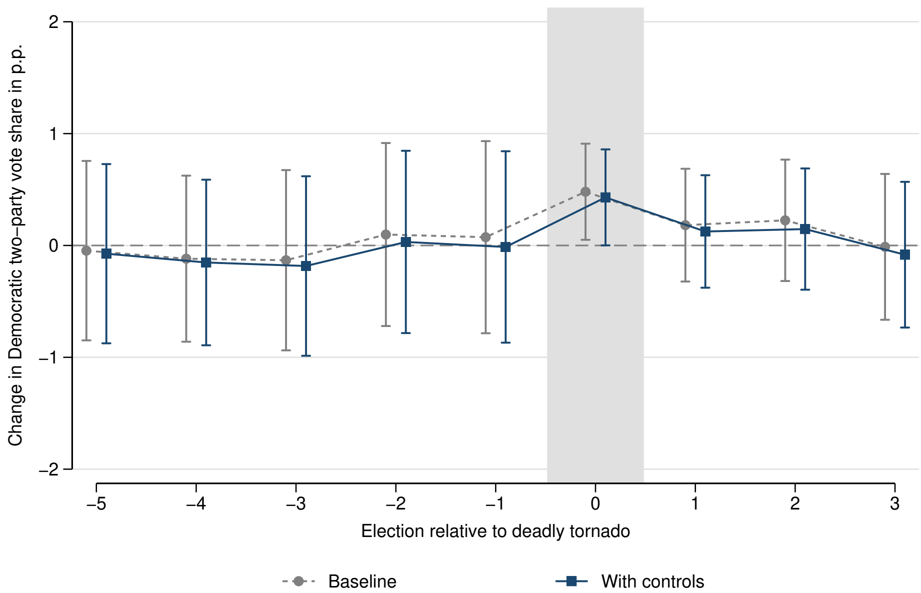
4.6.1 Baseline

My baseline specifications examine the impact of the first deadly tornado since 1950 in a given county on the Democratic vote share in gubernatorial elections. I only include county-election observations up to the point of a second event, that is a second deadly tornado in my observation period. Figure 4.4 plots the pre-trend estimates obtained from the regression outlined in Equation 4.4 and "treatment effects" for up to four gubernatorial elections following the deadly tornado, estimated by the imputation method according to Equation 4.3. Estimates are charted with 95% confidence intervals (CI). The grey dashed line illustrates the development of pre-trend and treatment coefficients with county and election fixed effects. The blue solid line connects coefficients with additional population controls. The figure validates the empirical design: I observe no signs of different pre-trends with the empirical strategy explained in Section 4.5, as pre-trend coefficients are small and statistically indistinguishable from zero. Hence, the Democratic two-party vote share does not differ systematically between treated counties and the comparison group before the treatment.

By contrast, the treatment coefficient in period zero is 0.48 (0.43) p.p. and statistically distinguishable from zero at the 95% CI in the baseline (with population controls). Period zero is the first election after the deadly tornado. Hence, voters in counties struck by a deadly tornado are relatively more likely to vote for the Democratic gubernatorial candidate: The vote share for the Democratic candidate is almost half a percentage point higher in treated counties than in the comparison group. Although the effect refers to a single county, a deadly tornado might very well affect state-wide electoral outcomes considering multiple examples of tight races in the US gubernatorial election history (e.g., 1950 Michigan (margin: 0.06%), 1962 Minnesota (0.01%), 1981 New Jersey (0.08%), and 2004 Washington (0.005%)). Further, the increased vote share for the Democratic candidate after a deadly tornado is preliminary evidence for a change in voters' preferred policy, as described in Section 4.2. I investigate the proposed mechanism in more detail by isolating the change in preferred policy from other potentially confounding channels, such as gubernatorial or party incumbency, in Section 4.7.

The treatment coefficients suggest that the treatment effect is somewhat persistent. I only observe statistically significant treatment coefficients in the first election following a deadly tornado. The magnitude of the treatment coefficients for the second and third election after the event is around one-third of the treatment effect in period t but statistically

Figure 4.4 : Baseline model specifications



Notes: The figure shows the results for my baseline event study design. The grey dashed line illustrates the development of pre-trend and treatment coefficients with county and election fixed effects. The blue solid line connects coefficients with additional population controls. I observe the difference in the Democratic two-party vote share between treated and control counties for five gubernatorial elections before and four gubernatorial elections after a deadly tornado in treated counties. The test for parallel trends includes five pre-treatment periods. Pre-trend and treatment coefficients are shown in Column I and II, Table D.3.

indistinguishable from zero at the 95% CI. This suggests that part of the effect may carry over to the second and the third election before vanishing in the fourth election with the coefficient being back to around zero.[29] I report all pre-trend and treatment coefficients along with the corresponding 95% CI in Column I, Table D.3, for my baseline specification. I add a county's total population as a control variable in Column II, Table D.3, because more people in a given county may increase the likelihood of a tornado-prone death while simultaneously being correlated with a higher vote share for Democrats. While county fixed effects are suitable to capture time-invariant characteristics (i.e., a county's size), they fail to pick up dynamic effects. As a county's population changes over time, I control for population developments in the following specifications.

4.6.2 Treatment intensity

So far, my sample includes all deadly tornadoes regardless of the number of fatalities and the exact timing of the tornado. It is reasonable to assume that deadly tornadoes closer to an election are more likely to impact election results. The change in voters' preferences is stronger because it is less confounded by other experiences. The number of fatalities is a good proxy for the magnitude of a tornado. Casualties are strongly correlated with economic damage (Deryugina and Marx, 2021). The line of argument goes as follows: The more tornado-related casualties, the greater the negative shock and the more likely a change in preferences and, hence, a change in voting behavior.

Deadly tornado less than 12 months before the election: My study design is ideally suited to investigate variable treatment intensity due to the exogenous timing of a deadly tornado. I conduct a subsample analysis including only deadly tornadoes striking within one year before the gubernatorial election (320 treated county-election observations). The treatment coefficient for period t increases by around 75% compared to the baseline specification with controls and is inside the statistical significance of the 95% CI.

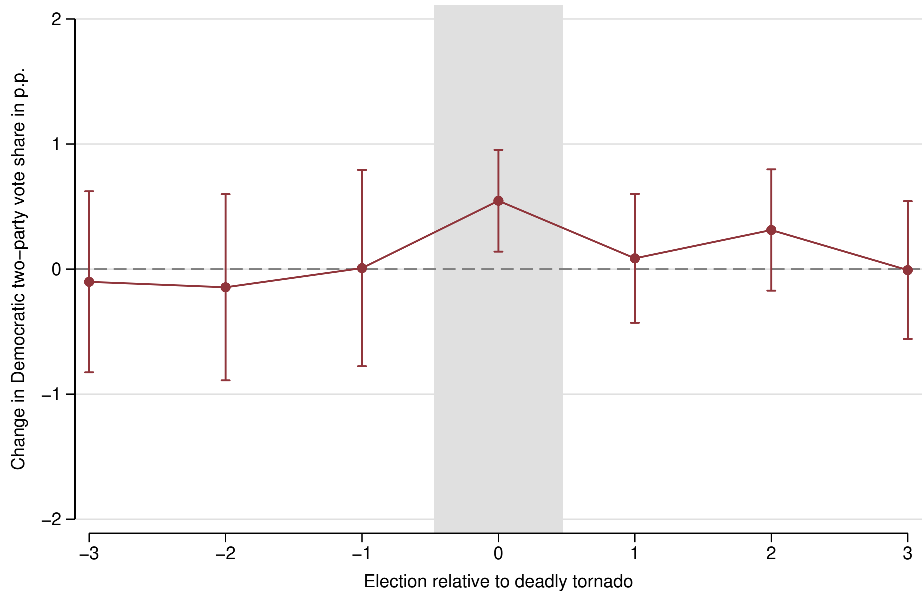
Deadly tornado with five or more fatalities: In this subsample analysis, I define the treatment as experiencing five or more tornado-induced fatalities since the last US gubernatorial election (235 treated county-election observations). The treatment coefficient for period t increases to 0.568 p.p, however remains outside any statistical significance at conventional levels.

[29] Bechtel and Hainmueller (2011) investigate short- and long-term electoral effects from the 2002 Elbe flooding in Germany and find a similar pattern.

Deadly tornado with five or more fatalities and less than 12 months before the election: Combining the two treatment intensity measures (timing and fatalities), I estimate a model with 80 treated county-election observations. The treatment coefficient for period t is large (1.673 p.p.) and significant at the 90% CI. The standard error increases further due to the relatively small number of treated county-election observations.

4.6.3 Multiple treatments

Figure 4.5 : Multiple treatments allowed



Notes: The figure shows the results for my event study design allowing for multiple treatments per county. I observe the difference in the Democratic two-party vote share between treated and control counties for three gubernatorial elections before and four gubernatorial elections after a deadly tornado in treated counties. The test for parallel trends includes five pre-treatment periods. However, coefficients for $t-4$ and $t-5$ are not reported because the sample is highly unbalanced in these periods. Pre-trend and treatment coefficients are shown in Column III, Table D.3.

In the previous model specifications, I only included counties experiencing their first deadly tornado since 1950 in the treatment group. This implies that county-election observations after a second treatment were excluded from the sample. To reinforce my argument that deadly tornado experiences shift voters' preferences toward the policy platform offered by Democratic candidates, it would be ideal to demonstrate that multiple treatments in a given county have similar effects. A second deadly tornado should, with some temporal distance from the first, increase the Democratic vote share again. To investigate this empirically, I relax the assumption of spill-over effects from a deadly tornado in the sense that I limit the "cooling off" period between events to four election periods. Assuming that the election at period

t is treated – a deadly tornado struck since the last gubernatorial election – then election periods $t+1$, $t+2$, and $t+3$ would be part of the "cooling off" period, in line with treatment coefficients considered so far in the event study. I, hence, incorporate in my treatment group all deadly tornadoes between 1950 and 2020, which hit a given county without a fatal tornado experience for at least three electoral cycles.

Figure 4.5 shows the same pattern as previous model specifications: (i) treated counties do not differ in their Democratic two-party vote share from counties in the comparison group before the event, (ii) a deadly tornado causes an increase in the Democratic two-party vote share in the following gubernatorial election, and (iii) evidence suggests that the treatment effect is somewhat persistent but decreases sharply in the next elections. The treatment coefficient in period zero is slightly higher than in the baseline specification with controls and statistically distinguishable from zero at the 99% CI.

4.7 Discussion

This section investigates mechanisms to explain the increase in the Democratic vote share in gubernatorial elections after deadly tornadoes. I examine governor and party incumbency effects and discuss the federal disaster response to tornadoes. In Appendix A, I show that the impact of deadly tornadoes on electoral outcomes manifests on the state level rather than on the federal level and provide reasons.

4.7.1 Incumbency effects

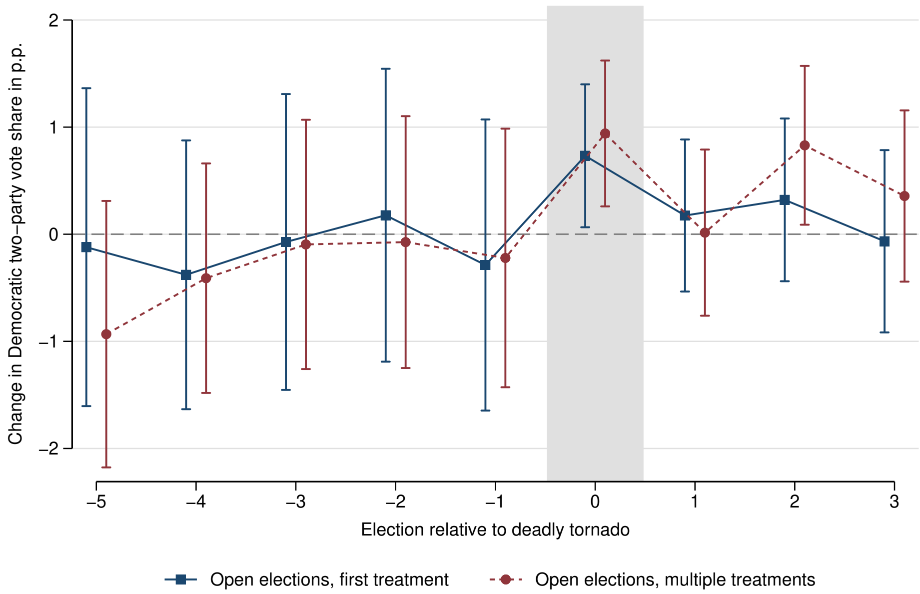
The incumbent's status, performance, and characteristics influence voter behavior after disasters.[30] Depending on this mix and its reception by voters, disasters may have positive, negative, or no effect on the incumbent's vote share.[31] Identifying the exact mechanism through which a disaster influences the incumbent's vote share poses a challenge to researchers. My study setup allows me to analyze the mechanism of how disasters influence voting behavior while ruling out incumbency effects. My proposed channel – a change in voter's preferences – should ideally work independently of incumbency effects.

[30] Irrational retrospective voting: Wolfers et al. (2002); Achen and Bartels (2002, 2016); Leigh (2009); Healy and Malhotra (2010); disaster relief effort: Healy et al. (2010); Bechtel and Hainmueller (2011); Gasper and Reeves (2011); Cole et al. (2012); Neugart and Rode (2021); Cooperman (2022); new information: Ashworth et al. (2018); Cerqua et al. (2021); Masiero and Santarossa (2021).

[31] (+): Bechtel and Hainmueller (2011); Chen (2013); Masiero and Santarossa (2021); Neugart and Rode (2021); (−): Wolfers et al. (2002); Achen and Bartels (2002, 2016); Leigh (2009); Healy et al. (2010); Gasper and Reeves (2011); Cole et al. (2012); Heersink et al. (2017); (∼): Abney and Hill (1966); Ebeid and Rodden (2006); Kayser and Peress (2012).

Open seat elections: To rule out that the increase in the Democratic vote share after a deadly tornado is driven by a systematic difference in the incumbent's response along party lines, e.g., the Democratic governor attracts more financial disaster relief, she is more present in the media, she is more empathetic, etc., I only consider open seat elections. Term limits are the most common reason for open seat elections. Others are (personal) decisions by standing governors not to run for reelection despite the judicial ability. Figure 4.6 confirms my baseline results. Excluding governor incumbency effects do not change inferences. I still observe no signs of different pre-trends in voting behavior for treated counties and the comparison group. The treatment coefficient in period zero is 0.733 p.p. and inside the statistical significance of the 95% CI. In the model specification allowing for multiple treatments, the coefficient increases to almost one p.p. and is statistically significant at the 1% level. Standard errors of pre-trend and treatment coefficients are larger because of the smaller sample size.

Figure 4.6 : Open seat elections



Notes: The figure shows the results for my baseline event study design including controls (blue squares) and allowing for multiple treatments (red dots) for elections with Democratic party incumbency in period t. I observe the difference in the Democratic two-party vote share between treated and control counties for five gubernatorial elections before and four gubernatorial elections after a deadly tornado in treated counties. The test for parallel trends includes five pre-treatment periods. Pre-trend and treatment coefficients for the baseline specification with controls are reported in Column I, Table D.4.

Controlling for party incumbency: While open seat elections rule out governor incumbency effects, they do not account for potential party incumbency effects. It is possible that the Democratic candidate in the next election gets credit (or punished) for the Democratic governor's disaster response. The Democratic governor herself does not stand for reelection but her party does. The succeeding candidate may have previously served in the governor's cabinet, actively contributing to the state's executive disaster response. Splitting the sample in elections with Democratic and Republican governor incumbency yields dissimilar results.

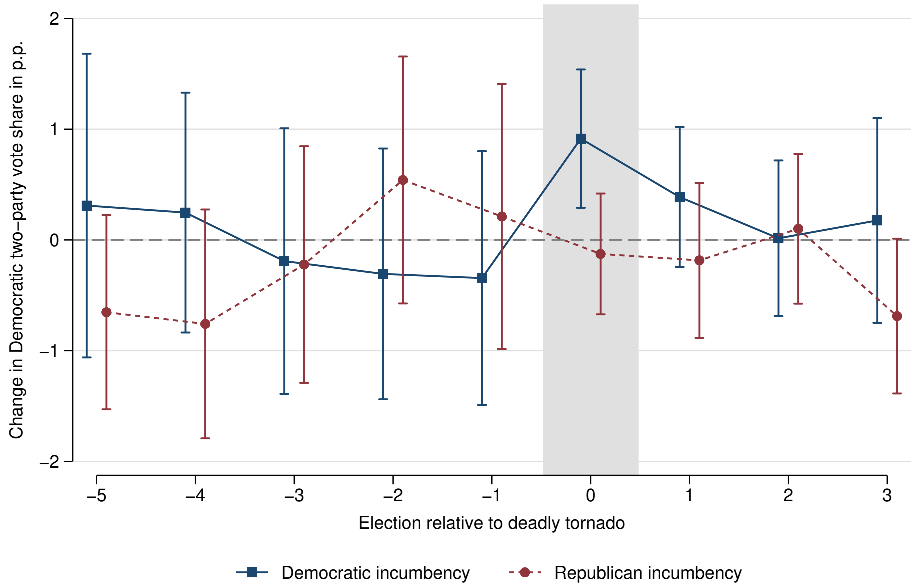
Figure 4.7 illustrates the results for counties being treated when a Democratic governor was in power (blue) and Republican governor incumbency (red). In Panel A, I only include counties experiencing their first deadly tornado since 1950 in the treatment group. Panel B shows results for the model specification with multiple treatments. Under Democratic incumbency, the treatment effect in t – the election following the deadly tornado – is about twice as large compared to the baseline and statistically significant at the 1% level (1,066 treated county-election observations). Vice versa, the treatment coefficient for the sample with Republican incumbency only (897 treated county-election observations) is slightly negative and statistically indistinguishable from zero at conventional levels. This implies that voters in counties experiencing a deadly tornado under a Democratic governor are more likely to vote for the Democratic candidate in the next gubernatorial election whereas voters in treated counties controlled by a Republican governor do not vote differently compared to the comparison group.

The evidence suggests two mechanisms that either work in conjunction with or against each other. The party incumbency mechanism indicates that, on average, the candidate of the incumbent party benefits from the governor's disaster response in the subsequent election. The mechanism's strength is about the same magnitude as the identified effect of a shift in voters' preferences toward the policy platform offered by Democratic candidates and the resulting increase in the Democratic vote share. No aggregate treatment effect can be observed when examining treated counties in Republican-controlled states. Treated counties in states governed by a Democrat vote about one p.p. more for the Democratic candidate in the following gubernatorial election compared to counties in the comparison group. About half of the increase can be attributed to either mechanism: party incumbency and a shift in the voter's preferred policy.

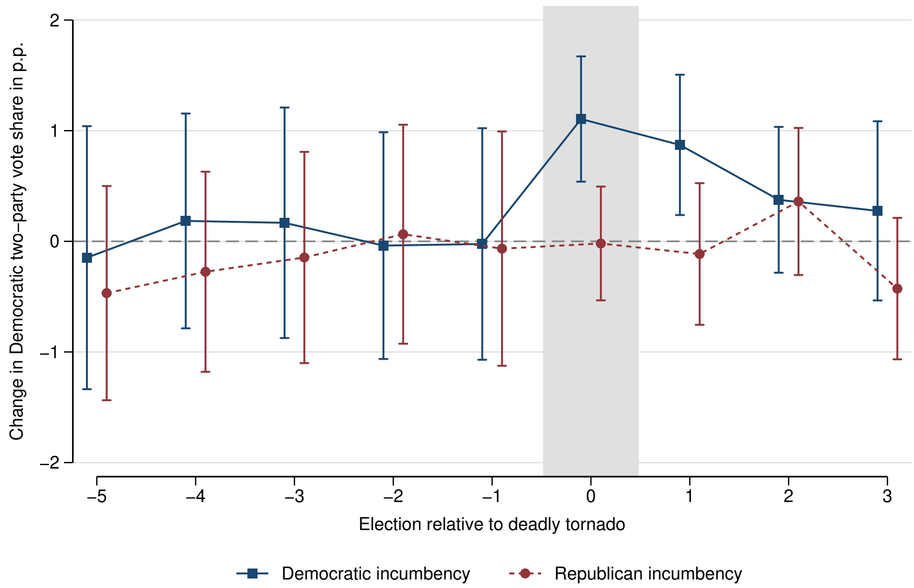
4.7.2 Federal disaster response

A remaining threat to interpreting the results as a shift in voters' preferences toward the policy platform offered by Democratic candidates is the potentially confounding impact of the federal disaster response. Democratic governors may simply be better at attracting federal disaster relief money and therefore get rewarded at the polls in affected counties. A growing

Figure 4.7 : Party incumbency



(a) First treatment

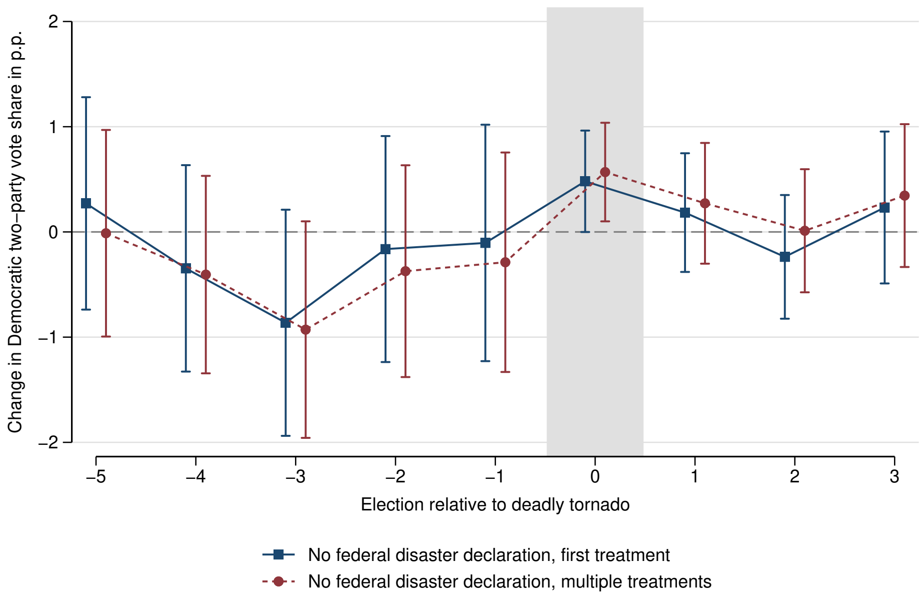


(b) Multiple treatments

Notes: The figure shows the results for Democratic governor incumbency (blue squares) and Republican governor incumbency (red dots) for first treatments (Panel A) and multiple treatments (Panel B). I observe the difference in the Democratic two-party vote share between treated and control counties for five gubernatorial elections before and four gubernatorial elections after a deadly tornado in treated counties. The test for parallel trends includes five pre-treatment periods. Pre-trend and treatment coefficients from Panel A are shown in Columns II and III, Table D.4.

economic literature investigates discretionary mechanisms in the allocation of funds (e.g., Brollo and Nannicini, 2012; Hodler and Raschky, 2014; Burgess et al., 2015; Chu et al., 2021). Schneider and Kunze (2023) document a heterogeneous political bias in presidential disaster declarations in the United States: Counties governed by president's co-partisans receive more relief money after medium-intensity hurricane winds. Disaster declaration decisions are a unilateral power of the US president. She decides which counties are covered by a declaration (Gasper and Reeves, 2011). I exclude the possibility of federal funds flowing into tornado-affected counties by only considering deadly tornadoes with no presidential disaster declaration in the aftermath.

Figure 4.8 : Excluding disaster declarations



Notes: The figure shows the results for my baseline event study design including controls (blue squares) and allowing for multiple treatments (red dots) for deadly tornadoes without a federal disaster declaration. I observe the difference in the Democratic two-party vote share between treated and control counties for five gubernatorial elections before and four gubernatorial elections after a deadly tornado in treated counties. The test for parallel trends includes five pre-treatment periods. Pre-trend and treatment coefficients are reported in Columns I & II, Table D.5.

FEMA (2023) provides data on all official FEMA Disaster Declarations, beginning with the first declaration in 1953. The data includes all federally declared disasters and recovery programs on the state and county-level.[32] I use the incident type and the declaration title to identify declarations due to tornadoes. Furthermore, I match the data with the tornado data using the incident begin and end dates along with the counties covered by the declaration.

[32] Data on the county-level is available from 1964 onward.

Figure 4.8 shows the event study estimates excluding counties which were eligible for federal disaster relief after a deadly tornado. The treatment coefficient in period t is similar in size to the baseline specification with first treated county-election observations. The coefficient just misses statistical significance at the 5% level, but becomes larger and significant when considering all treated county-election observations. Although pre-trend coefficients are more volatile due to the limited sample size, treated counties and the comparison group do not vote systematically differently before the event. Therefore, my inferences hold when excluding the possibility of disaster relief efforts from the federal level.

4.8 Conclusion

In this paper, I build on a spatial model of voting with valence and a rich empirical literature on natural disasters, risk aversion, and electoral outcomes to understand how deadly tornadoes influence US gubernatorial elections. My framework predicts that the voter's preferred policy moves closer toward the policy platform offered by the Democratic candidate after a deadly tornado. Voters demand more social insurance, something Democrats promise and Republicans oppose.

My main analysis describes a causal effect of deadly tornadoes on the rise of the Democratic two-party vote share in US gubernatorial elections from 1960 to 2020. The Democratic candidate's vote share increases by almost half a percentage point in affected counties. I confirm this result by investigating open seat elections to rule out a governor incumbency effect. To further supplement my analysis, I examine evidence for the underlying mechanism explaining the rising fortune of Democratic candidates after a deadly tornado. Deadly tornadoes do not seem to influence voting behavior when a Republican governor is in power. Under a Democratic governor, the effect size is about twice as large as in the baseline specification. The results suggest that two mechanisms work either in conjunction or against each other. In line with the literature identifying a positive incumbency effect, I find that the candidate of the incumbent party benefits from the governor's disaster response in the subsequent election (e.g., Bechtel and Hainmueller, 2011; Chen, 2013; Neugart and Rode, 2021). As proposed by my spatial model of voting with valence, I also observe a shift in voters' preferences toward the policy platform offered by Democratic candidates and the resulting increase in the Democratic vote share. My evidence suggests that the strength of the two mechanisms is about equal in magnitude.

A challenge in empirical studies examining the channel through which a negative shock influences voter behavior is eliminating alternative explanations. My setup allows me to connect a deadly tornado to an increased Democratic vote share via a change in voters' preferred policies. The finding that Democrats benefit from deadly tornadoes partly contradicts existing research documenting a positive relationship between crisis experience and voter preference for leadership and conservatism (e.g., Stenner, 2005; Merolla and Zechmeister, 2009; Holman et al., 2016, 2019, however in the context of terrorism). It accommodates the finding of Pastor and Veronesi (2020), who show that Democratic voters are more risk averse. Their model of political cycles predicts that more risk averse agents elect Democrats because they promise more social security and redistribution. Future research might explore the extent to which risk aversion increases after a deadly tornado. Ideally, future research would also investigate county heterogeneity in voter motivation in US gubernatorial elections. Finally, studies exploring different contexts could discuss my finding that a crisis experience alters a voter's preferred policy.

Appendices

A Appendix to Chapter 1

A 1887 fall election results for the electoral district of Stockholm

Table A.1 : Number of votes for free-trade, protectionist, and independent candidates

Free-Trader	Votes	Protectionist	Votes	Independent	Votes
Key	6,707	de Laval	2,954	Telander	1,856
Nordenskiöld	6,641	Widström	2,946	Crusebjörn	1,777
Taube	6,640	Billing	2,984	Morssing	1,699
Fock	6,640	Palmstierna	2,982		
von Friesen	6,639	Werner	2,876		
Wallden	6,637	Styffe	2,787		
Loven	6,636	Carlsson, E. W.	2,776		
Stackelberg	6,627	Lindmark	2,756		
Abergsson	6,626	Svanberg	2,731		
Grafström	6,620	Bergman	2,717		
Beckmann	6,617	Bexelius	2,717		
Siljeström	6,614	Berndes	2,716		
Hedin	6,591	Nyström, C.	2,715		
Larsson, A. P.	6,497	Cederschiöld	2,710		
Johansson	6,475	Beskow	2,708		
Fredholm	6,466	Höglund, F.	2,691		
Höglund, O. M.	6,420	Björek	2,688		
Erikson, P. J. M.	6,389	Carlsson, A. V.	2,688		
Larsson, Olof	6,197	Lund	2,687		
Hammarlund	4,916	Berg, C. O.	2,649		
Berg, F.	4,911	Wittrock	2,628		
Gustafsson	4,866	Lyth	2,598		

Source: Aftonbladet (1887).

B The Swedish tariff debate

Economists have been investigating and discussing the effect of the Swedish tariff increases on the economy since 1888.

The first scientific contribution to the matter came shortly after World War I. Eli Heckscher and Arthur Montgomery examined the effects of the Swedish 19th-century tariff policy in a public investigation. The final report was published in 1924 and concluded that the increased tariff protection was probably negative for the Swedish economy because it supported mostly domestic market industries and not export industries (Tull- och traktatkommittén, 1924). They later diverted from their previous assessment and argued that the tariffs probably had only small effects on the economy (Heckscher, 1941) and that Sweden would have developed similarly without the tariff increases in 1888 and 1892 (Montgomery, 1966).

In a similar vein, Jörberg (1961) describes that the tariffs may have contributed to import substitution but that the overall effect is difficult to assess. Jörberg (1966) argued that the domestic market may have benefited from the tariffs but that this influenced the Swedish industrialization process only to a small extent. He concluded that the tariffs probably did not have a significant effect on Swedish industrial growth.

Hammarström (1970) argued that the tariffs triggered an import substitution process, particularly in the customer goods industries. Imports of finished products decreased, and raw material imports increased.

Contrary to previous work, Schön (1989) concluded that Swedish tariffs increased economic growth. Tariffs primarily protected industries with long-term growth potential and contributed to Sweden's industrial development.

Bohlin (2005) constructed tariff indices for a large part of the Swedish economy using a sample of commodities between 1885 and 1914, and, similar to Hammarström, emphasized that the tariffs caused import substitution. Import penetration decreased significantly for goods subject to the tariffs of the late 1880s.

> *"Even if one measures the tariff rate in a more appropriate way one may, of course, argue that the rate of protection was not 'high', however, it was apparently high enough in the majority of cases to achieve its aim of deterring imports. It seems obvious that the protectionist system had effects, good or bad, on individual industries and thus also on Swedish economic development in general."*
> — (Bohlin, 2005, p. 25)

More recently, Häggqvist (2018) contributed to the tariff debate by investigating the link between customs revenue and government activity. The Swedish trade liberalization initially forced a switch in the fiscal structure of tariffs toward consumption goods with low demand elasticity. After 1888, tariffs on agricultural and capital goods became more fiscally relevant.

> *"This development took place during a critical time when customs revenue as share of total government revenue really took off and came to be the single most important tax receipt. Trade policy hence came to be a key driver of nineteenth century fiscal development in Sweden."*
> — (Häggqvist, 2018, p. 16)

The most comprehensive analysis of the topic so far was conducted by Persarvet (2019). In his encompassing work, he concludes:

> *"Foreign trade and growth increased rapidly, the later more so after a protectionist trade policy was put in place in the late 1880s and 1890s."*
> — (Persarvet, 2019, p. 180)

> *"In the end, the tariff protection thus probably had a limited impact on the overall development of the aggregate productivity growth of the Swedish economy. Although it might have increased in the short term due to labor shifts, this effect was most likely small."*
> — (Persarvet, 2019, p. 184)

We contribute to this longstanding debate by providing causal evidence on how the tariff increases influenced economic outcomes.

C Swedish fiscal policies in the first era of globalization

C.1 Government revenue

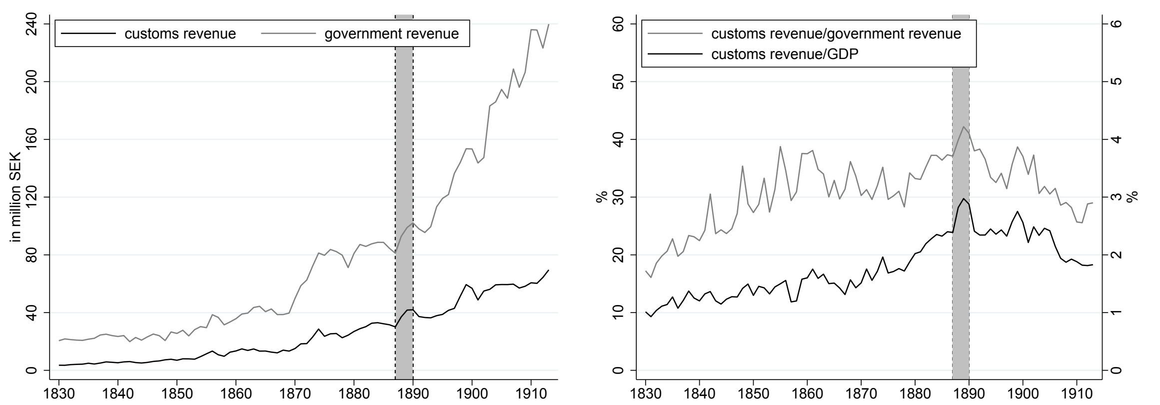
Since 1870 Sweden's government revenue as a share of GDP remained relatively stable at around 6%. In 1888, government revenue as a share of GDP increased substantially to well above 7% and remained at this level throughout 1889 and 1890.

Customs revenue

Customs revenue was the single most important source of government revenue (Häggqvist, 2018). Customs revenue as a share of total government revenue was 39.87% in 1888, 42.20% in 1889, and 41.06% in 1890 — the highest shares over the period from 1830 to 1913 (Figure A.1). Customs revenue increased by 6.94 million SEK or 23.06% in 1888 compared to 1887. Tariffs on grains accounted for more than half of the increase (4.16 million SEK), which changed the composition of customs revenue. While in 1887 customs duties on agricultural products accounted for only 0.1% of customs revenue, this share increased to 14.7% in 1888, 19.4% in 1889, and peaked in 1890 (20.0%). Customs revenue coming from industrial products was low; its share of total customs revenue was 2.9% in 1887 and increased just slightly to 3.6% in 1888, 3.8% in 1889, and 3.7% in 1890.[1]

[1] Customs duties on industrial products increased substantially in the 1890s. At the end of the decade, industrial customs revenue as a share of total customs revenue was above 10%. Major increases occurred after the Cobden-Chevalier treaty expired in 1892: The share of total customs revenue coming from industrial products was 4.5%

Figure A.1 : Customs revenue



Notes: The left panel shows the development of customs revenue and government revenue between 1830 and 1913. The right panel shows the development of customs revenue as a share of government revenue (left y-axis) and as a share of GDP (right y-axis) between 1830 and 1913. The relevant post-treatment period (1888–1890) is shaded in gray. The data are from Häggqvist (2018).

Taxation

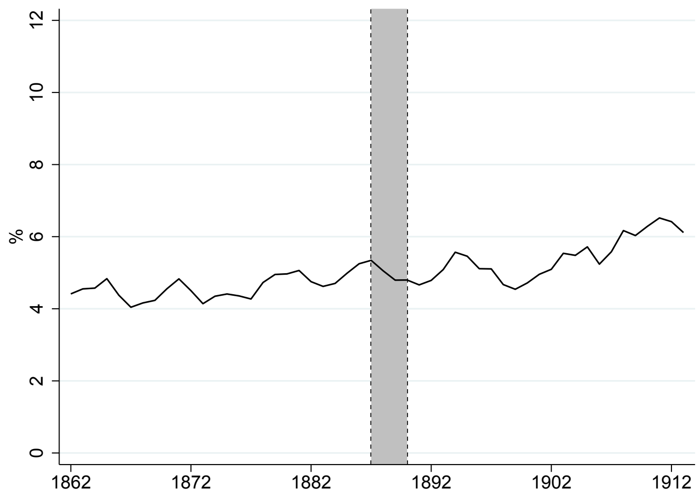
We follow the editorial work of Henrekson and Stenkula (2015) and examine the development of taxation in Sweden for six key aspects of the Swedish tax system: the taxation of labor income, capital income, consumption, inheritance and gifts, wealth, and real estate. The overall tax-to-GDP ratio, excluding customs revenue, changed little between 1862 and 1913 (Figure A.2). Tax revenue usually fluctuated between 4%–6% of GDP.

Before World War I, major national income tax reforms were implemented in 1862, 1903, and 1911, none of which affected our pre-treatment period differently than our post-treatment period. The national tax level on labor income was normally set at 1% but could be increased to 2% if the ordinary appropriation taxes yielded insufficient revenue (Du Rietz et al., 2015c). However, in the years prior to and including 1887 and in our post-treatment period, the national marginal labor income tax rate remained constant. A local labor income tax, excise duties, and a national appropriation tax were also introduced in 1862 and 1863. The marginal local labor tax rate gradually increased from 2% to 5% at the end of the 19th century. Still, overall the marginal labor income tax rates remained low until World War I (Figure A.3, upper left panel).

Changes in capital income taxation affect the incentive to invest and thereby might influence GDP even in the short run. Again, the analysis of Swedish capital income taxation begins in 1862

in 1892, 6.5% in 1893, 7.6% in 1894, 8.2% in 1895, 9.8% in 1896, and 10.4% in 1897 (Häggqvist, 2018).

Figure A.2 : Tax revenue development



Notes: The figure shows the development of tax revenue as a share of GDP between 1862 and 1913. The relevant post-treatment period (1888–1890) is shaded in gray. The data are from Henrekson and Stenkula (2015).

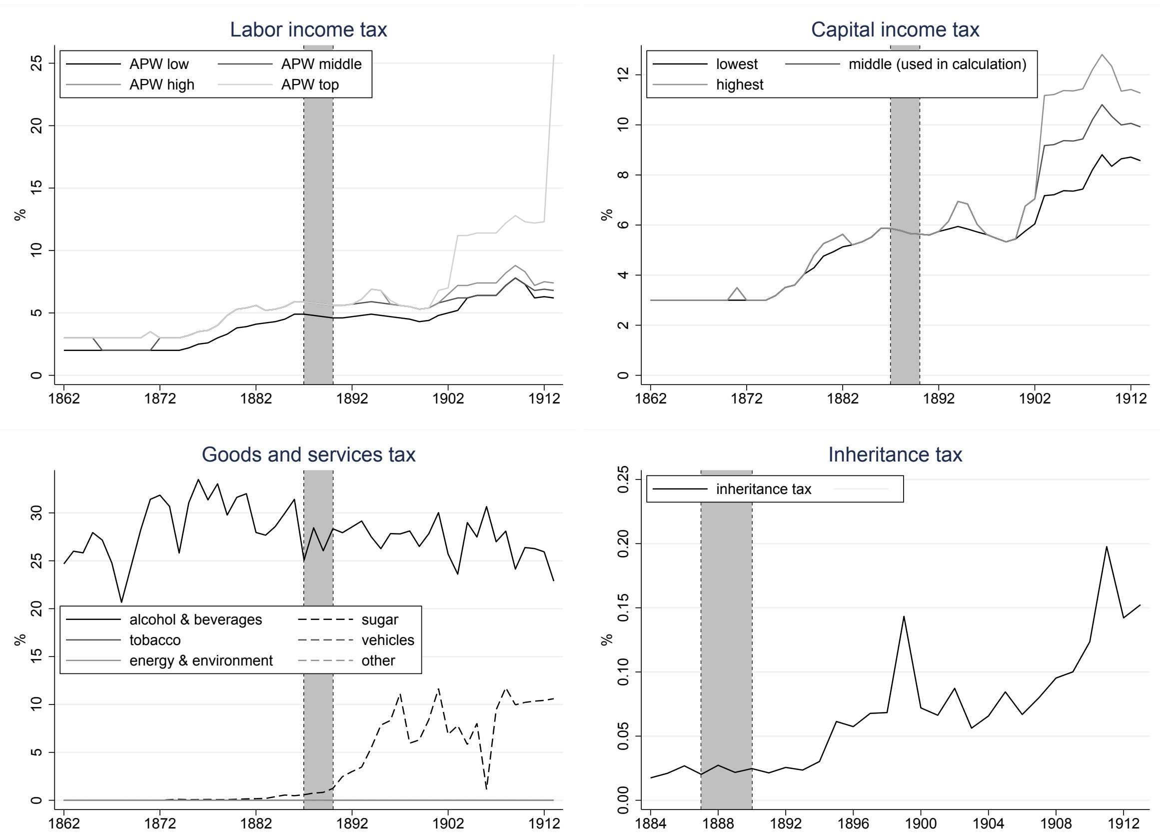
with the introduction of a major central government tax system. The new state appropriation tax law taxed corporate profits in the same way and at the same rates as individual taxpayers' income (approximately 1%, see previous paragraph[2]) (Du Rietz et al., 2015b). Based on King and Fullerton (1984), Du Rietz et al. (2015b) calculate the marginal effective tax rate (METR) on capital income for an investment financed with new share issues, retained earnings, and debt. The METR was low between 1862 and 1913 and changed little in the 1880s (Figure A.3, upper right panel).

Consumption taxes were the second most important state revenue stream after customs duties between 1862 and 1913 (Stenkula, 2015). Consumption taxes accounted for 15% to 20% of total tax revenue and up to 40% of state tax revenue until World War I. Alcohol-related taxes were the most important specific consumption tax, with revenue fluctuating between 20.7% and 33.5% of state tax revenue. Consumption taxes on sugar increased substantially during the 1890s from 1.24% of state tax revenue in 1890 to around 10% at the end of the century. Overall, consumption tax revenue fluctuated considerably in the 60s and 70s, but the 80s were a decade of comparatively stable consumption tax revenue (Figure A.3, lower left panel).

Finally, various duties and fees on estates, inheritances, and wills existed for small and parts of the tax base and population strata throughout the 18th century. In 1885, the modern Swedish

[2] Initially, approximately 1% of taxable profit was paid to the state, and approximately 2% were paid to local governments. The state income tax was stable, but the local tax rate increased to approximately 5% until 1900.

Figure A.3 : Tax rate and revenue development by tax type



Notes: The upper left panel shows the development of the marginal labor income tax rate as the sum of the national and local marginal labor income tax rates and social security contributions paid by employees. The upper right panel shows the development of the marginal effective tax rate on capital income for the highest and lowest statutory marginal corporate tax rate and the middle statutory marginal corporate tax rate used in the calculations by Du Rietz et al. (2015b). The lower left panel shows the development of consumption taxes as a share of state tax revenue. The lower right panel shows the development of inheritance, estate, and gift tax revenue as a share of GDP (data pre-1884 is not available). The relevant post-treatment period (1888–1890) is shaded in gray. The data are from Henrekson and Stenkula (2015).

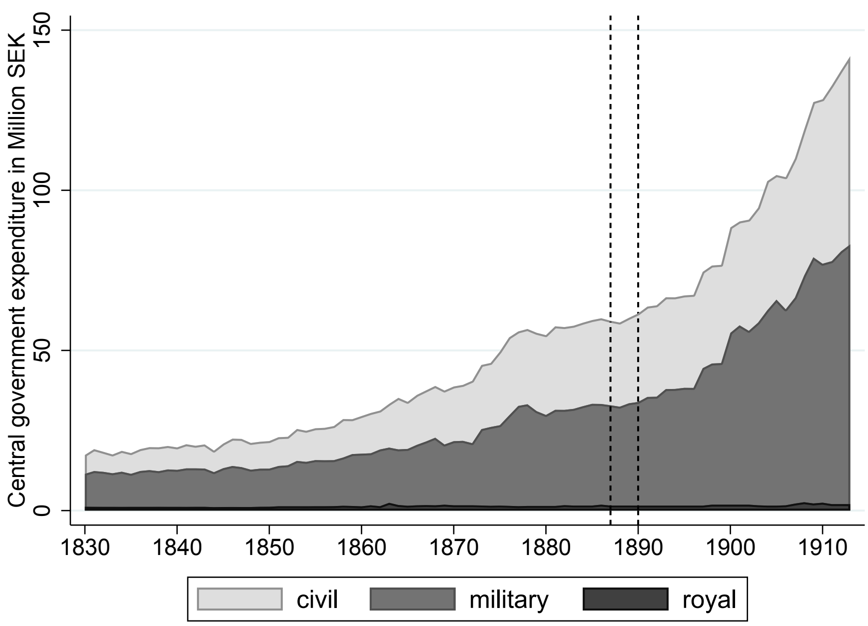
inheritance taxation was introduced as a single tax (the 1884 Stamp Ordinance) (Du Rietz et al., 2015a). The actual tax was imposed on the lots received by the heirs. The income tax reform of 1861/62 also reduced the inheritance tax from around 3% to a flat rate of 1%. The 1884 Stamp Ordinance merged all previous variants of estate taxes into a single tax in the form of a stamp on the total estate value. Direct heirs were taxed at a rate of 0.5%, and other heirs were taxed at a rate of 0.6%. Du Rietz et al. (2015a) find that revenue from the gift, inheritance, and estate taxes were never fiscally important when compared to personal income or wealth taxes, even though tax rates increased substantially in the 20th century. They suggest that the inheritance tax was primarily supposed to reduce large intergenerational transfers at the top of the distribution. Accordingly, inheritance, estate, and gift tax revenue in Sweden was very low and accounted for only 0.017% of GDP in 1884 and 0.030% of GDP in 1894 (Figure A.3, lower right panel). Swedish wealth taxation was introduced in 1911 and is therefore not suitable for consideration in our study (Du Rietz and Henrekson, 2015).

C.2 Government expenditure

Central government expenditure

Swedish central government expenditure increased at an accelerating pace from 1830–1913 (Figure A.4). However, the 1880s and early 1890s were marked by rather steady and moderate increases. In 1888, the increased central government revenue due to the increased customs revenue gave rise to financial desires across the parliamentary benches and the royal court (Beck et al., 1911). On October 12, 1888, Oscar II[3] declared at the Council of State that he wishes to spend the surplus from the increased customs revenue on insurance and pensions, the abolition of the land taxes, and lowering of the municipal taxes. However, the *Riksdag* devoted the increased central government revenue to balance the budget (Beck et al., 1911). Overall, the budget composition changed little after the majority in parliament changed (Schön and Krantz, 2015). Civil, military, and royal expenditure remained unchanged or increased just slightly, thereby following its slow but steady growth path.

Figure A.4 : Central government expenditure, 1830–1913



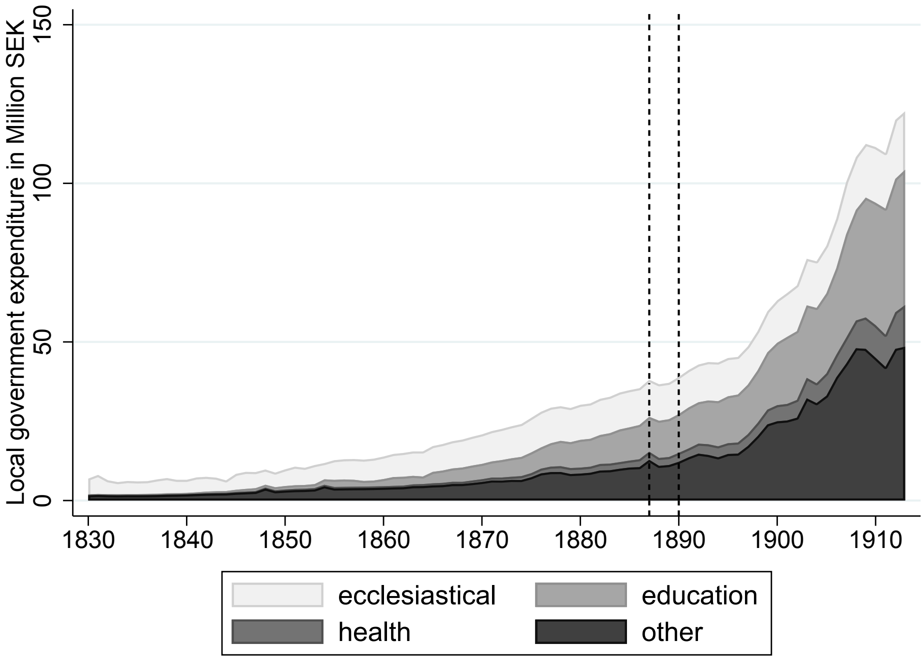
Notes: The relevant post-treatment period (1888–1890) is indicated by the dashed vertical lines. The data are from Schön and Krantz (2015).

[3] Oscar II was King of Sweden from 1872–1907.

Local government expenditure

Swedish local government expenditure developed similarly to Swedish central government expenditure, though being less volatile pre-1900 (Figure A.5). Local government expenditure ought to be unaffected by the majority change in the *Riksdag* and the change in national government. If we observe an increase or decrease in local government expenditure after 1887, our identification strategy would likely be biased because local government expenditure potentially influences short-run macroeconomic outcomes. We do not observe that local government expenditure changed systematically after 1887. Indeed, Schön and Krantz (2015) show that ecclesiastical expenditure hardly changes throughout the 1870s and 1880s whereas educational expenditure follows a steady growth path that accelerates at the turn of the century. Health expenditure remained a minor item on the local government expenditure list until the early 1900s.

Figure A.5 : Local government expenditure, 1830–1913

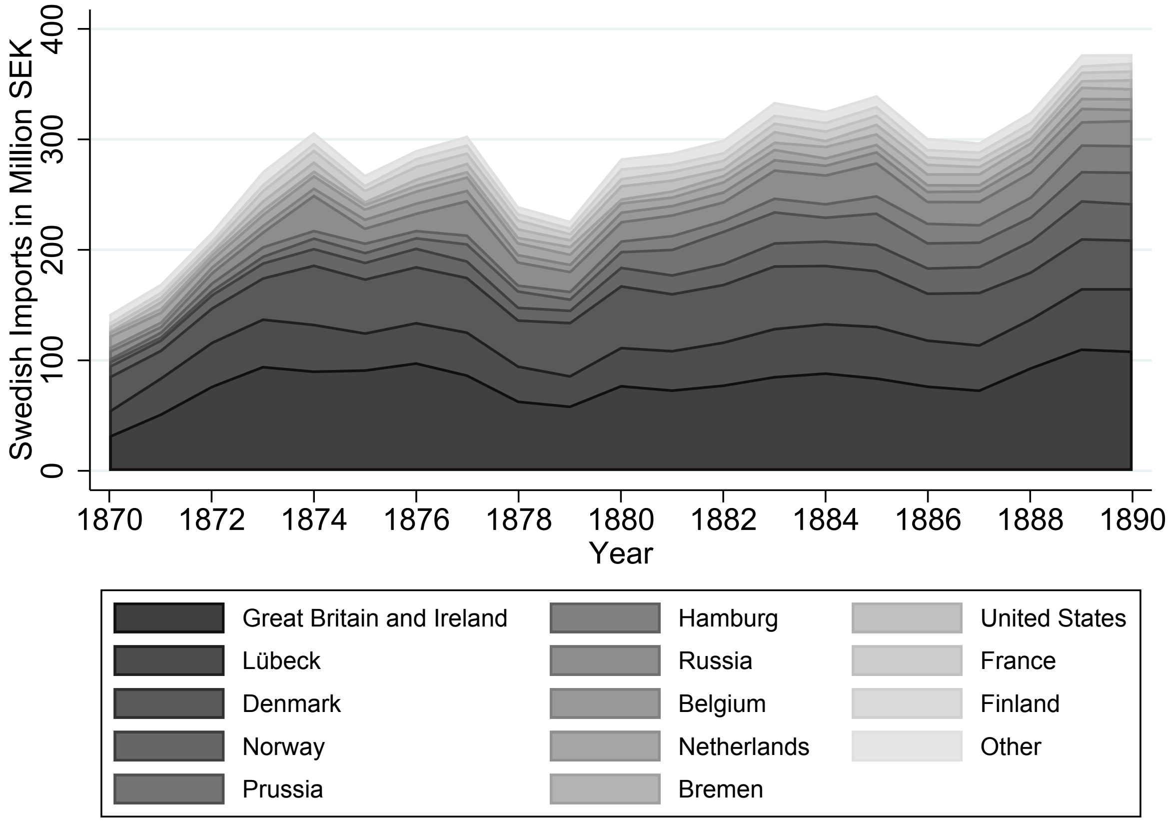


Notes: The relevant post-treatment period (1888–1890) is indicated by the dashed vertical lines. The data are from Schön and Krantz (2015).

D Composition of Swedish imports

D.1 Swedish imports by trading partner

Figure A.6 : Swedish imports by trading partner, 1870–1890



Notes: The data are from the Swedish Board of Trade: Annual Statistics 1870 to 1890.

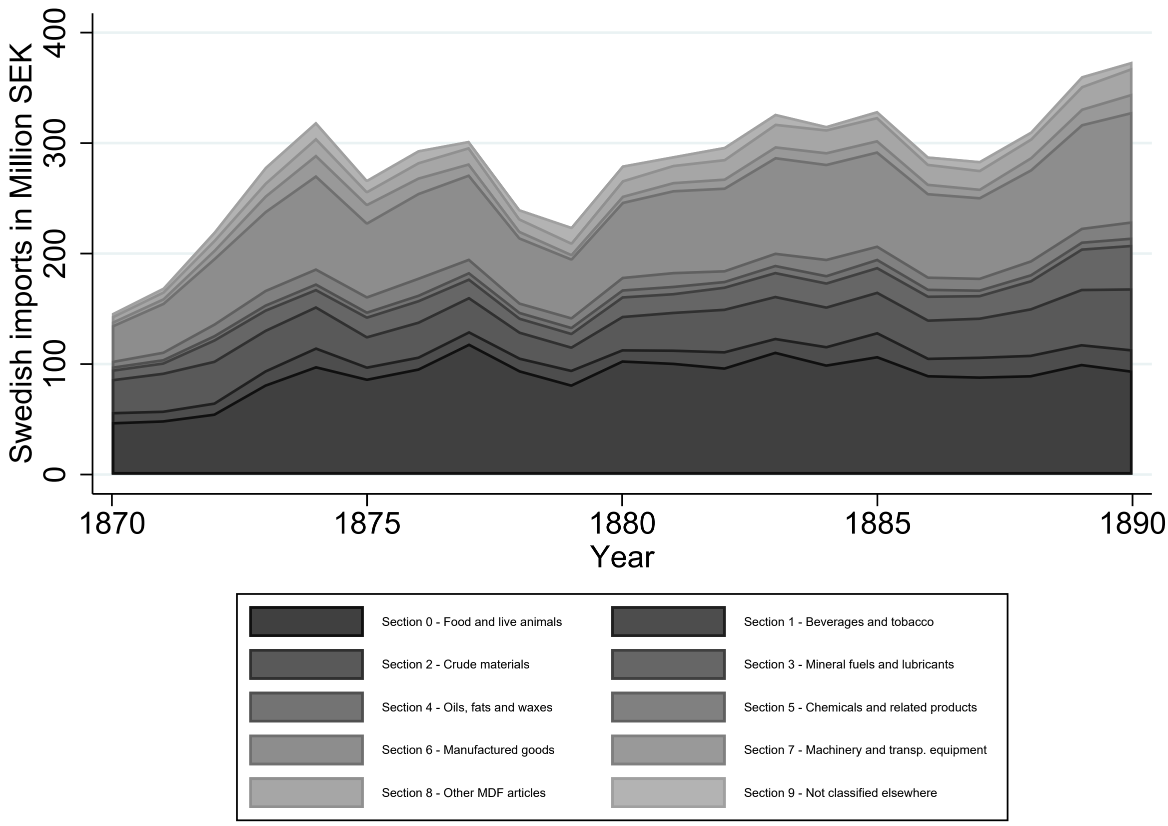
Table A.2 : Swedish imports by trading partner: Growth rates

Countries	1871	1872	1873	1874	1875	1876	1877	1878	1879	1880	1881	1882	1883	1884	1885	1886	1887	1888	1889	1890
Norway	-4%	29%	14%	11%	0%	11%	-11%	-23%	-4%	51%	1%	11%	11%	6%	8%	-4%	3%	20%	22%	-4%
Finland	23%	-11%	40%	10%	-24%	52%	3%	-24%	-8%	73%	-25%	3%	6%	9%	0%	-16%	4%	2%	-17%	20%
Russia	-29%	87%	44%	138%	-57%	15%	100%	-33%	-13%	-3%	7%	-10%	51%	2%	15%	-34%	6%	6%	-6%	9%
Denmark	-19%	24%	20%	44%	-9%	4%	-2%	-16%	15%	16%	-8%	1%	9%	-7%	-4%	-16%	12%	-11%	7%	-3%
Prussia	-27%	34%	60%	57%	-7%	7%	64%	-5%	-31%	40%	63%	27%	-4%	-23%	31%	-20%	-3%	-2%	22%	8%
Luebeck	43%	22%	8%	-2%	-21%	9%	7%	-18%	-13%	25%	3%	9%	12%	3%	4%	-11%	-2%	8%	23%	4%
Hamburg	70%	58%	24%	-20%	25%	-23%	20%	-33%	31%	38%	31%	-21%	27%	-2%	29%	13%	-11%	15%	32%	0%
Bremen	0%	18%	9%	-9%	6%	22%	-5%	1%	41%	-51%	60%	4%	14%	59%	-8%	2%	2%	-3%	5%	-11%
Netherlands	-13%	-23%	45%	14%	-19%	19%	11%	-12%	-14%	-8%	-9%	19%	2%	-29%	2%	-10%	-9%	26%	29%	8%
Belgium	23%	28%	14%	10%	25%	12%	3%	-28%	-4%	35%	-2%	6%	4%	-6%	13%	-9%	5%	7%	19%	-17%
UK and Ireland	62%	49%	23%	-4%	1%	7%	-11%	-27%	-7%	31%	-5%	6%	10%	4%	-5%	-9%	-5%	27%	18%	-2%
France	1%	43%	58%	15%	-6%	15%	-20%	-14%	-32%	18%	20%	-2%	0%	12%	-5%	-17%	-8%	9%	13%	4%
USA	450%	-53%	179%	4%	-70%	126%	34%	2%	-20%	95%	-18%	-37%	56%	-44%	61%	0%	-24%	-37%	41%	39%
Other	-2%	7%	50%	-16%	-8%	-21%	10%	-18%	-6%	48%	11%	15%	0%	-14%	-3%	4%	-17%	12%	5%	-22%
Total	19%	28%	25%	13%	-13%	8%	4%	-21%	-5%	25%	2%	4%	11%	-2%	4%	-11%	-1%	9%	16%	0%

Notes: The data are from the Swedish Board of Trade: Annual Statistics 1870 to 1890.

D.2 Swedish imports by sector

Figure A.7 : Swedish imports by sector, 1870–1890



Notes: The data are from Persarvet (2019).

E Synthetic control weights

Table A.3 : Synthetic control weights by outcome

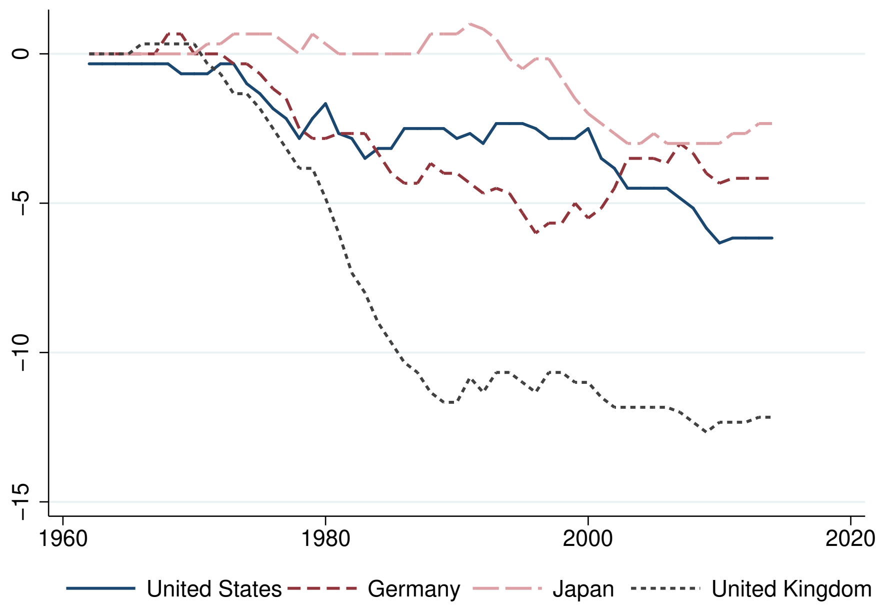
	GDP	Imports	Government Revenue	Government Expenditure
Belgium	0	0	0.128	0
Canada	0	0	0.410	0
Denmark	0.217	0.365	0.061	0.222
Finland	0.436	0	.	.
Netherlands	0	0.102	0.126	0.229
Norway	0.173	0	0	0.052
Switzerland	0	.	0	0
UK	0.003	0	0.231	0.314
USA	0.170	0.532	0.045	0.182

Notes: Own calculations.

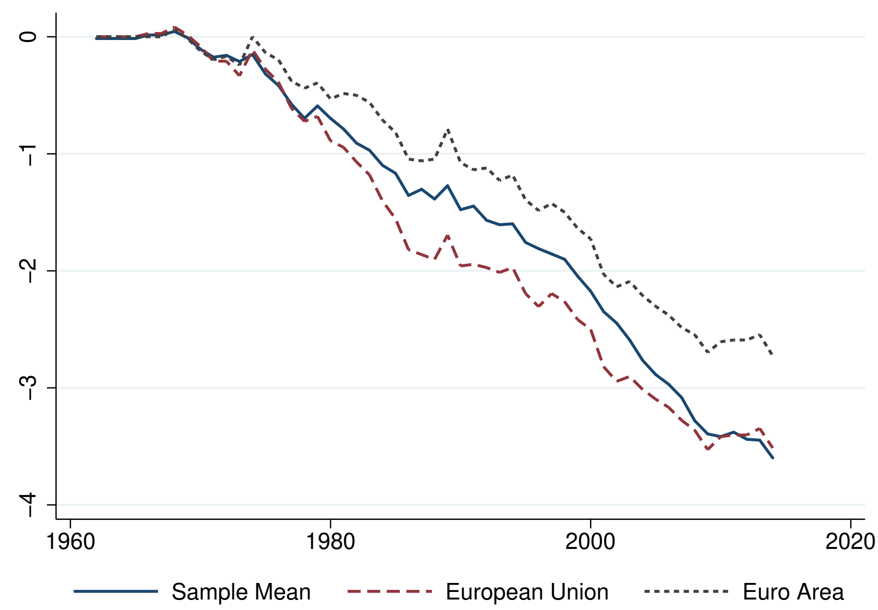
B Appendix to Chapter 2

A Supplementary figures

Figure B.1 : Trends in tax bases, selected countries and samples, 1960–2014



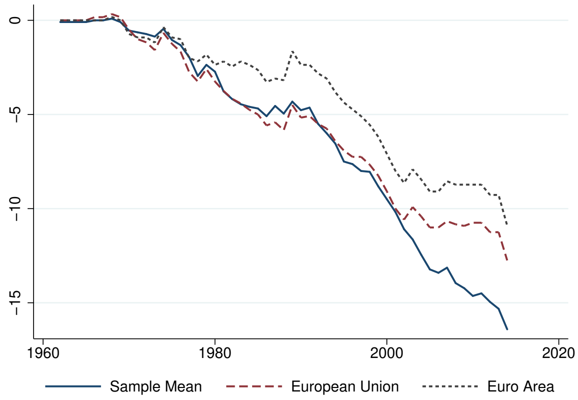
(a) Trends in taxation: The United States, Germany, Japan, the United Kingdom



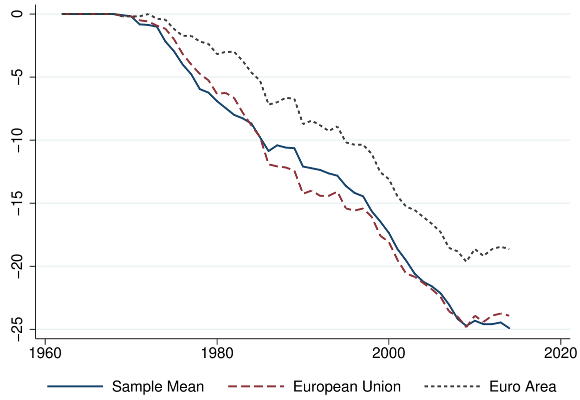
(b) Trends in taxation: Sample mean, the European Union, and the Euro Area

Notes: The figure illustrates the accumulated version of the aggregate Tax Reform Index ($\mathfrak{S}^{A}_{iT'}$) for tax bases to compare trends in taxation between (1) the United States, Germany, Japan, and the United Kingdom and (2) the sample mean, the European Union, and the Euro Area over time. For the accumulated version, each point in time $T^{'}$ represents the sum of the Tax Reform Index $\mathfrak{S}^{A}_{it}$ over all available periods prior to $T^{'}$.

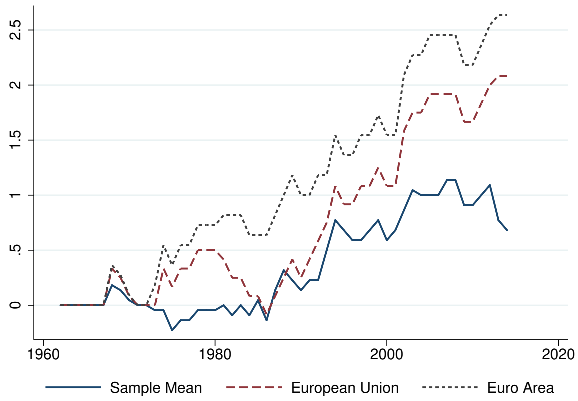
Figure B.2 : Trends in individual tax types, tax bases, 1960–2014



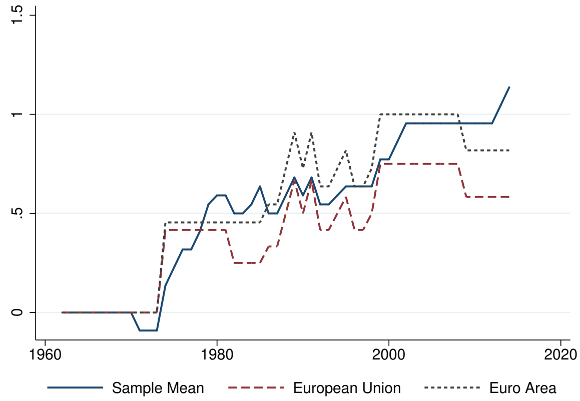
(a) **Corporate tax bases**



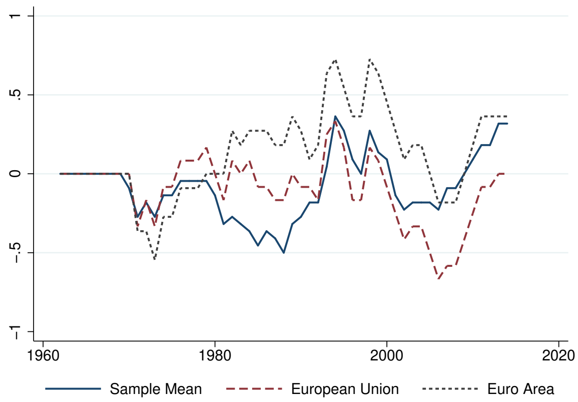
(b) **Personal income tax bases**



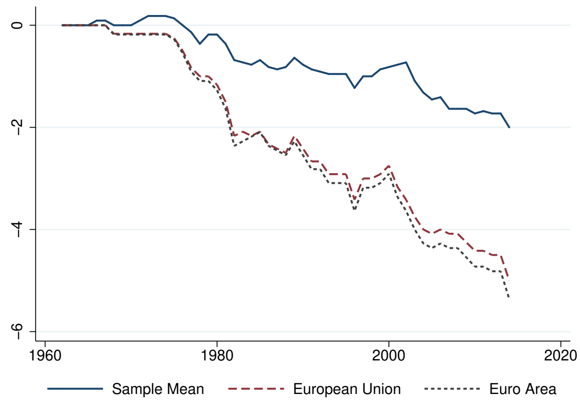
(c) **Value added tax bases**



(d) **Excise tax bases**



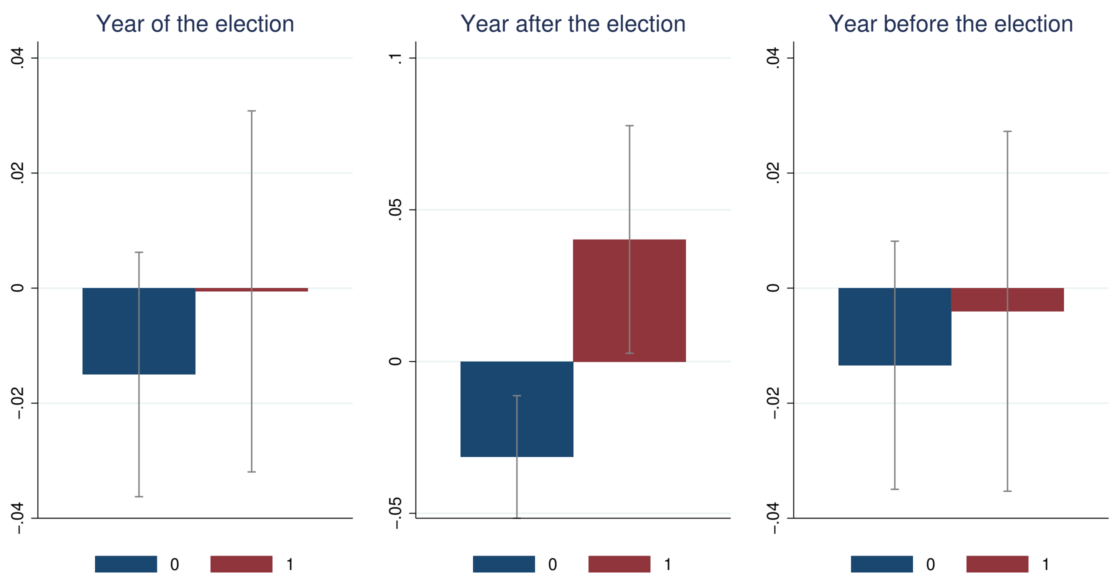
(e) **Property tax bases**



(f) **Social security contributions**

Notes: The figure illustrates the accumulated version of the Tax Reform Index ($\mathfrak{S}^{A}_{iT'}$) for tax bases for individual tax types to compare trends in taxation between the sample mean, the European Union, and the Euro Area over time. For the accumulated version, each point in time T' represents the sum of the Tax Reform Index $\mathfrak{S}^{A}_{it}$ over all available periods prior to T'.

Figure B.3 : Changes in tax rates and election dates, aggregate tax reform index, full sample of advanced and emerging market economies, 1960–2014



Notes: The figure shows changes in tax rates implied by the aggregate Tax Reform Index ($\mathfrak{S}^{A}_{iT'}$) in election years and non-election years. When comparing changes in tax rates in election years, label "1" refers to years with elections, while "0" refers to non-election years. When comparing changes in years before and after elections, "1" refers to the pre- or post-election-year. Vertical lines indicate 95% confidence intervals.

B Supplementary tables

Table B.1 : Summary statistics of variables

Variable	Mean	Std. Dev.	Min.	Max.	Observations
Election	0.305	0.461	0	1	791
Aggregate Indicator (tax rate)	-0.01	0.329	-2	1.667	1166
Aggregate Indicator (tax base)	-0.068	0.334	-1.5	1.5	1166
CIT Indicator (tax rate)	-0.148	0.811	-2	2	1166
EXE Indicator (tax rate)	0.136	0.595	-2	2	1166
PIT Indicator (tax rate)	-0.142	0.851	-2	2	1166
PRO Indicator (tax rate)	0.002	0.275	-2	2	1166
SSC Indicator (rate)	0.055	0.541	-2	2	1166
VAT Indicator (tax rate)	0.039	0.677	-2	2	1166
CIT Indicator (tax rate, non-normalized)	-0.22	1.18	-8	6	1166
EXE Indicator (tax rate, non-normalized)	0.174	0.808	-4	8	1166
PIT Indicator (tax rate, non-normalized)	-0.226	1.266	-9	4	1166
PRO Indicator (tax rate, non-normalized)	-0.003	0.337	-6	2	1166
SSC Indicator (rate, non-normalized)	0.064	0.746	-6	6	1166
VAT Indicator (tax rate, non-normalized)	0.035	0.935	-10	6	1166

Notes: The table shows descriptive statistics of the variables used in our analysis. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

Table B.2 : Baseline results, tax bases

Dependent variable: Tax Reform Index (aggregated, tax bases), $\mathfrak{S}^A_{it}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	-0.0083			-0.0147
	(-0.30)			(-0.38)
Post-Election		0.0395		0.0297
		(1.22)		(1.02)
Pre-Election			-0.0426	-0.0457
			(-1.36)	(-1.02)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.0881	0.0914	0.0898	0.0923
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

Table B.3 : Accounting for observable confounding factors, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	-0.00488 (-0.15)			0.0462 (1.05)
Post-Election		0.0940*** (3.43)		0.114*** (3.27)
Pre-Election			-0.0165 (-0.40)	0.0138 (0.26)
ΔGDP^{pc}	-1.711 (-1.48)	-1.745 (-1.56)	-1.724 (-1.47)	-1.756 (-1.55)
Left-Wing ideology	0.00596 (0.12)	0.00589 (0.12)	0.00625 (0.13)	0.00521 (0.11)
Globalization	-0.000385 (-0.05)	0.0000296 (0.00)	-0.000340 (-0.04)	0.000504 (0.06)
Political institutions	-0.0545 (-0.54)	-0.0932 (-0.91)	-0.0544 (-0.56)	-0.115 (-1.19)
Observations	649	649	649	649
Countries	16	16	16	16
R-Squared (overall)	0.154	0.165	0.154	0.166
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level

Table B.4 : Excluding US midterm elections, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	-0.00725 (-0.26)			0.0277 (0.76)
Post-Election		0.0758*** (3.58)		0.0882*** (3.04)
Pre-Election			-0.0111 (-0.31)	0.0161 (0.37)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.138	0.146	0.138	0.147
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level

Table B.5 : Jack-knifed analysis, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}^A_{it}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Exclude Australia	0.0125 (0.57)	0.0737*** (3.18)	-0.0272 (-0.78)	0.0881** (2.91)
Exclude Austria	-0.0073 (-0.24)	0.0718*** (3.22)	-0.0090 (-0.24)	0.0844** (2.61)
Exclude Canada	-0.0085 (-0.28)	0.0755*** (3.21)	0.0184 (0.77)	0.101*** (3.33)
Exclude Denmark	-0.0119 (-0.40)	0.0859*** (4.36)	-0.0142 (-0.37)	0.0973*** (3.30)
Exclude Spain	-0.0099 (-0.33)	0.0793*** (3.65)	-0.0129 (-0.33)	0.0897** (2.93)
Exclude France	-0.0134 (-0.46)	0.0777*** (3.44)	-0.0101 (-0.26)	0.0879** (2.87)
Exclude United Kingdom	0.0046 (0.17)	0.0712*** (3.24)	-0.0064 (-0.16)	0.0916*** (3.04)
Exclude Germany	-0.0074 (-0.24)	0.0702*** (3.11)	-0.0176 (-0.47)	0.0776** (2.62)
Exclude Greece	-0.0110 (-0.36)	0.0617*** (3.35)	-0.0077 (-0.20)	0.0699** (2.66)
Exclude Ireland	-0.0056 (-0.19)	0.0707*** (3.19)	-0.0119 (-0.32)	0.0827** (2.74)
Exclude Italy	-0.0015 (-0.05)	0.0821*** (3.91)	-0.0238 (-0.63)	0.0943*** (3.05)
Exclude Japan	-0.0194 (-0.67)	0.0769*** (3.39)	-0.0131 (-0.33)	0.0814** (2.66)
Exclude Luxembourg	-0.0076 (-0.26)	0.0772*** (3.42)	-0.0174 (-0.46)	0.0872** (2.91)
Exclude Poland	-0.0094 (-0.33)	0.0773*** (3.44)	-0.0135 (-0.36)	0.0881** (2.87)
Exclude Portugal	-0.0172 (-0.60)	0.0791*** (3.52)	-0.0027 (-0.07)	0.0905*** (3.03)
Exclude United States	-0.0031 (-0.11)	0.0828*** (3.90)	-0.0087 (-0.22)	0.100*** (3.50)

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years. The estimated parameter reported in Column IV refers to the Post-Election dummy.

*** Significant at the 1% level,
** Significant at the 5% level

Table B.6 : Baseline results, reduced version of our tax reform indicator, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.00387 (0.14)			0.0353 (0.98)
Post-Election		0.0619** (2.83)		0.0761** (2.76)
Pre-Election			-0.0168 (-0.51)	0.00426 (0.10)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.137	0.143	0.137	0.144
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years. Results are obtained using the reduced version of our tax reform index that treats each reform equally, regardless of whether the IMF codes the reform as “major” or “minor”.

** Significant at the 5% level

Table B.7 : Baseline results, advanced and emerging market economies, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.00404			0.0709*
	(0.17)			(1.93)
Post-Election		0.0775***		0.114***
		(3.44)		(3.59)
Pre-Election			0.0149	0.0601
			(0.50)	(1.46)
Observations	836	836	836	836
Countries	22	22	22	22
R-Squared (overall)	0.0825	0.0923	0.0830	0.0992
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level,
* Significant at the 10% level

Table B.8 : Disentangling early and regular elections, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election (regular)	-0.0299 (-0.81)			0.00681 (0.13)
Post-Election (regular)		0.0797** (2.33)		0.0866** (2.05)
Pre-Election (regular)			-0.0296 (-0.78)	-0.00617 (-0.12)
Election (early)	0.0466 (1.22)			0.0718 (1.74)
Post-Election (early)		0.0604* (1.80)		0.0721** (2.29)
Pre-Election (early)			0.00624 (0.13)	0.0170 (0.35)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.143	0.145	0.140	0.152
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the relationship between tax reforms and election dates (Equation 2.4), disentangling early and regular elections. t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable "Election year" refers to election dates in t, "Post-Election" shows the coefficient for election dates in $(t-1)$, and "Pre-Election" reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

** Significant at the 5% level,
* Significant at the 10% level

Table B.9 : Corporate income taxation, tax rates

Dependent variable: Tax Reform Index (corporate income taxation, tax rates), $\mathfrak{S}^r_{it}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.0624 (0.64)			0.150 (1.17)
Post-Election		0.103 (1.14)		0.167 (1.45)
Pre-Election			-0.0180 (-0.18)	0.0571 (0.57)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.124	0.125	0.123	0.127
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable "Election year" refers to election dates in t, "Post-Election" shows the coefficient for election dates in $(t-1)$, and "Pre-Election" reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

Table B.10 : Personal income taxation, tax rates

Dependent variable: Tax Reform Index (personal income taxation, tax rates), $\mathfrak{S}_{it}^{r}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.144			0.273
	(0.85)			(1.21)
Post-Election		0.161		0.273**
		(1.32)		(2.24)
Pre-Election			-0.0777	0.0562
			(-0.51)	(0.26)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.107	0.107	0.106	0.110
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

** Significant at the 5% level

Table B.11 : Excises, tax rates

Dependent variable: Tax Reform Index (excises, tax rates), $\mathfrak{S}_{it}^{r}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	-0.0408			-0.0445
	(-0.68)			(-0.65)
Post-Election		0.0675		0.0453
		(0.64)		(0.41)
Pre-Election			-0.0410	-0.0545
			(-0.90)	(-0.78)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.0864	0.0863	0.0863	0.0877
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable "Election year" refers to election dates in t, "Post-Election" shows the coefficient for election dates in $(t-1)$, and "Pre-Election" reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

Table B.12 : Value added and sales taxation, tax rates

Dependent variable: Tax Reform Index (value added and sales taxation, tax rates), $\mathfrak{S}_{it}^{r}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	-0.0480 (-0.56)			0.0456 (0.41)
Post-Election		0.231*** (3.12)		0.248*** (3.36)
Pre-Election			-0.0494 (-0.72)	-0.00855 (-0.10)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.0792	0.0892	0.0793	0.0897
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level

Table B.13 : Property taxation, tax rates

Dependent variable: Tax Reform Index (property taxes, tax rates), $\mathfrak{S}_{it}^{r}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.00580 (0.33)			0.0248 (1.13)
Post-Election		0.0275 (0.72)		0.0381 (0.88)
Pre-Election			-0.00297 (-0.11)	0.0104 (0.36)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.0707	0.0722	0.0708	0.0723
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

Table B.14 : Social security contributions, rates

Dependent variable: Tax Reform Index (social security contributions, rates), $\mathfrak{S}^r_{it}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	-0.129**			-0.109*
	(-2.15)			(-1.97)
Post-Election		0.0194		-0.0171
		(0.28)		(-0.29)
Pre-Election			0.112	0.0670
			(1.59)	(0.90)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.0761	0.0730	0.0772	0.0782
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4). t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

** Significant at the 5% level,
* Significant at the 10% level

Table B.15 : Accounting for government changes, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.000926 (0.03)			0.0441 (1.08)
Post-Election		0.0735*** (3.29)		0.0923*** (3.01)
Pre-Election			-0.0151 (-0.42)	0.0116 (0.25)
Government change		-0.0205 (-0.70)	-0.0154 (-0.50)	-0.0231 (-0.74)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.128	0.136	0.129	0.137
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4) accounting for changes in government. The variable “Government change” assumes a value of one if the incumbent has been replaced by another government, and zero otherwise. t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable “Election year” refers to election dates in t, “Post-Election” shows the coefficient for election dates in $(t-1)$, and “Pre-Election” reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level

Table B.16 : Accounting for government spending, tax rates

Dependent variable: Tax Reform Index (aggregated, tax rates), $\mathfrak{S}_{it}^{A}$				
	(I) Election year	(II) Post-Election	(III) Pre-Election	(IV) Full specification
Election year	0.000643 (0.02)			0.0386 (1.07)
Post-Election		0.0723*** (3.26)		0.0881*** (3.32)
Pre-Election			-0.0146 (-0.42)	0.00879 (0.20)
Government spending (p.c.)	0.0001 (1.46)	0.0001 (1.46)	0.0001 (1.47)	0.0001 (1.45)
Observations	759	759	759	759
Countries	16	16	16	16
R-Squared (overall)	0.128	0.136	0.129	0.137
Prob. > F-Stat	0.000	0.000	0.000	0.000
Country-Level Fixed Effects	Yes	Yes	Yes	Yes
Period Fixed Effects	Yes	Yes	Yes	Yes

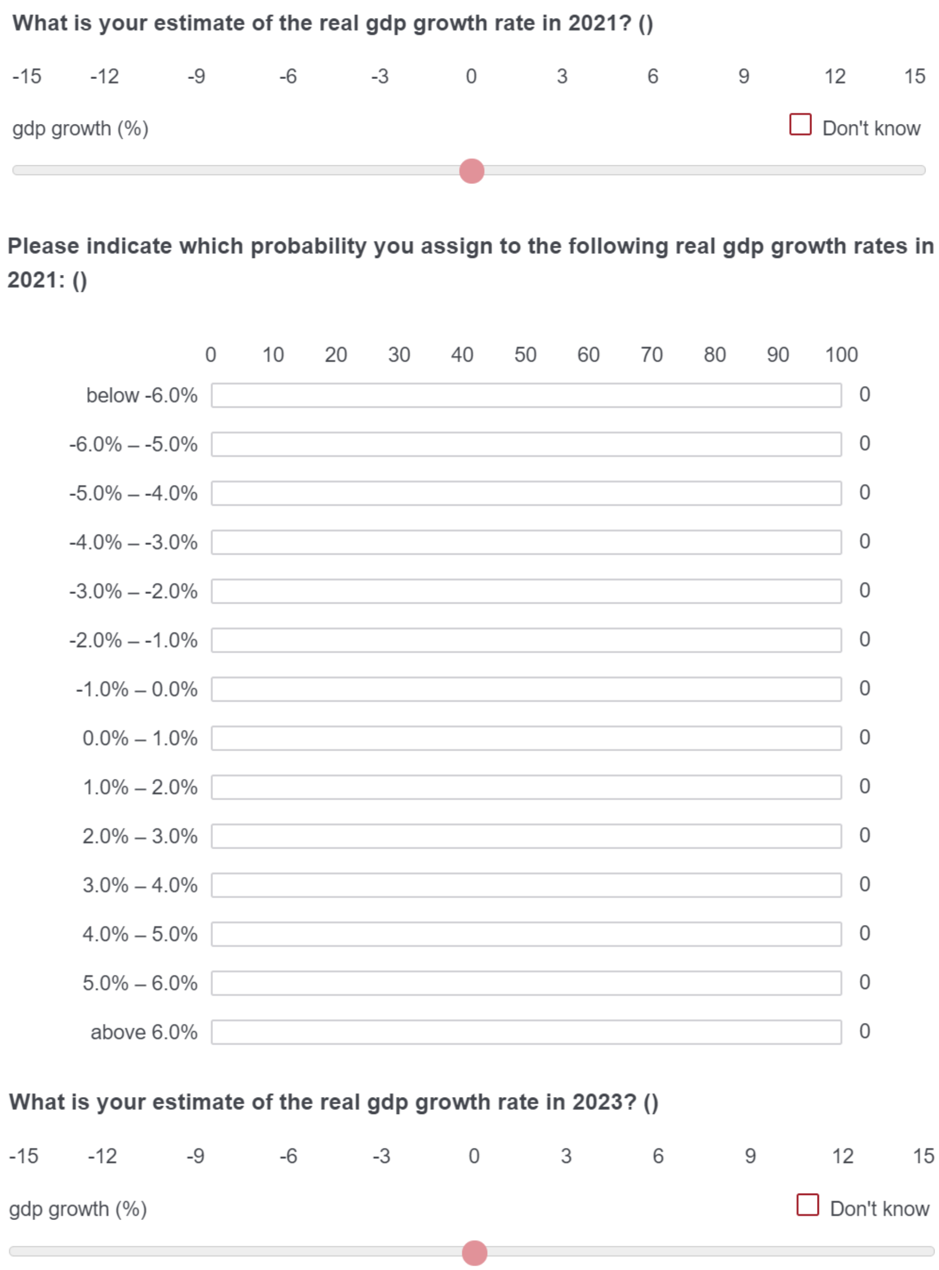
Notes: The table shows the baseline results of our estimations on the relationship between tax reforms and election dates (Equation 2.4) accounting for government spending per capita, taken from the IMF's International Financial Statistics. t values that are obtained using robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. The variable "Election year" refers to election dates in t, "Post-Election" shows the coefficient for election dates in $(t-1)$, and "Pre-Election" reports coefficients for election dates in $(t+1)$. For a description of the Tax Reform Index, see Section 2.2. All estimations include fixed effects for countries and years.

*** Significant at the 1% level

C Appendix to Chapter 3

A Supplementary material: Survey questionnaire

Figure C.1 : Page one: Questions asking about real GDP growth expectations

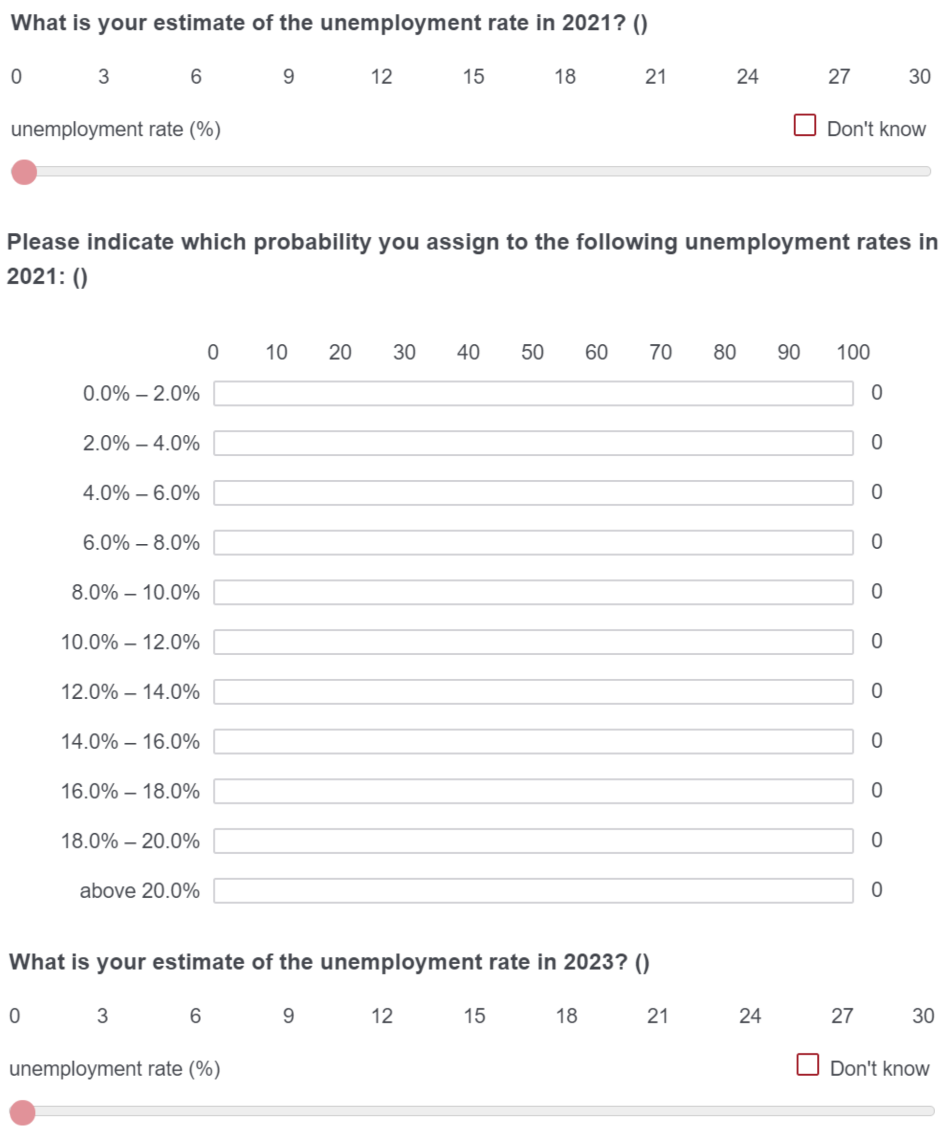


Notes: The figure shows the first page of our survey, asking about experts' expectations regarding real GDP growth. The expert's country is given in parentheses. A detailed description is provided in Section 3.3.2.

Figure C.2 : Page two: Questions asking about inflation rate expectations

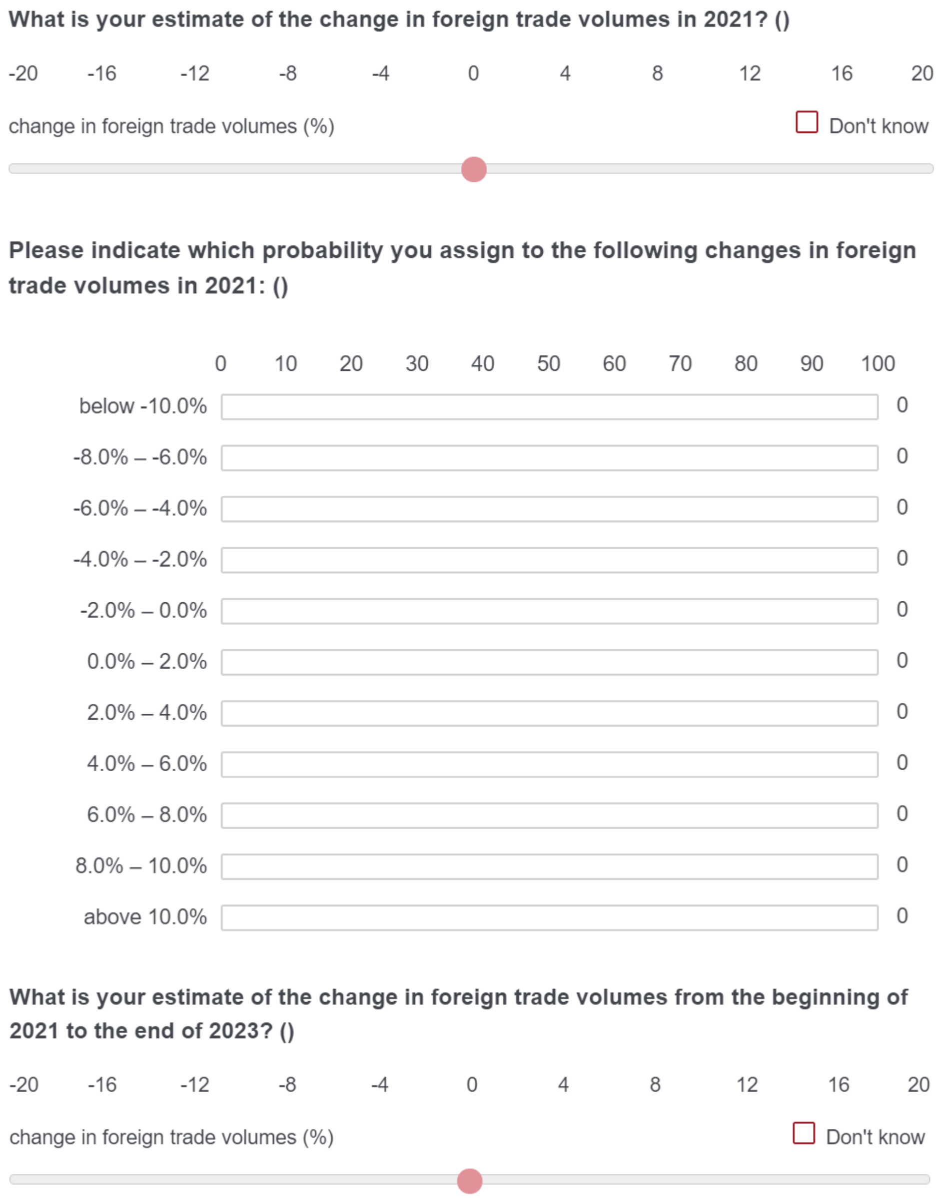
Notes: The figure shows the second page of our survey, asking about experts' expectations regarding inflation rates. The expert's country is given in parentheses. A detailed description is provided in Section 3.3.2.

Figure C.3 : Page three: Questions asking about unemployment rate expectations



Notes: The figure shows the third page of our survey, asking about experts' expectations regarding unemployment rates. The expert's country is given in parentheses. A detailed description is provided in Section 3.3.2.

Figure C.4 : Page four: Questions asking about trade volume expectations



What is your estimate of the change in foreign trade volumes in 2021? ()

-20 -16 -12 -8 -4 0 4 8 12 16 20

change in foreign trade volumes (%) ☐ Don't know

Please indicate which probability you assign to the following changes in foreign trade volumes in 2021: ()

	0 10 20 30 40 50 60 70 80 90 100	
below -10.0%		0
-8.0% – -6.0%		0
-6.0% – -4.0%		0
-4.0% – -2.0%		0
-2.0% – 0.0%		0
0.0% – 2.0%		0
2.0% – 4.0%		0
4.0% – 6.0%		0
6.0% – 8.0%		0
8.0% – 10.0%		0
above 10.0%		0

What is your estimate of the change in foreign trade volumes from the beginning of 2021 to the end of 2023? ()

-20 -16 -12 -8 -4 0 4 8 12 16 20

change in foreign trade volumes (%) ☐ Don't know

Notes: The figure shows the fourth page of our survey, asking about experts' expectations regarding changes in trade volumes. The expert's country is given in parentheses. A detailed description is provided in Section 3.3.2.

B Supplementary analyses and robustness checks

We conduct a series of supplementary analyses and statistical tests to evaluate the robustness of our baseline results.

Minimum numbers of experts per country: We obtain our benchmark estimates using experts from countries for which we have a minimum of three observations to exclude outliers. In Tables C.6 and C.7 in the Appendix, we alter the minimum requirement, examining effects when we exclude experts from countries with less than 10 participants (Table C.6) and countries with less than two participants (Table C.7). Altering minimum requirements has little influence on inferences.

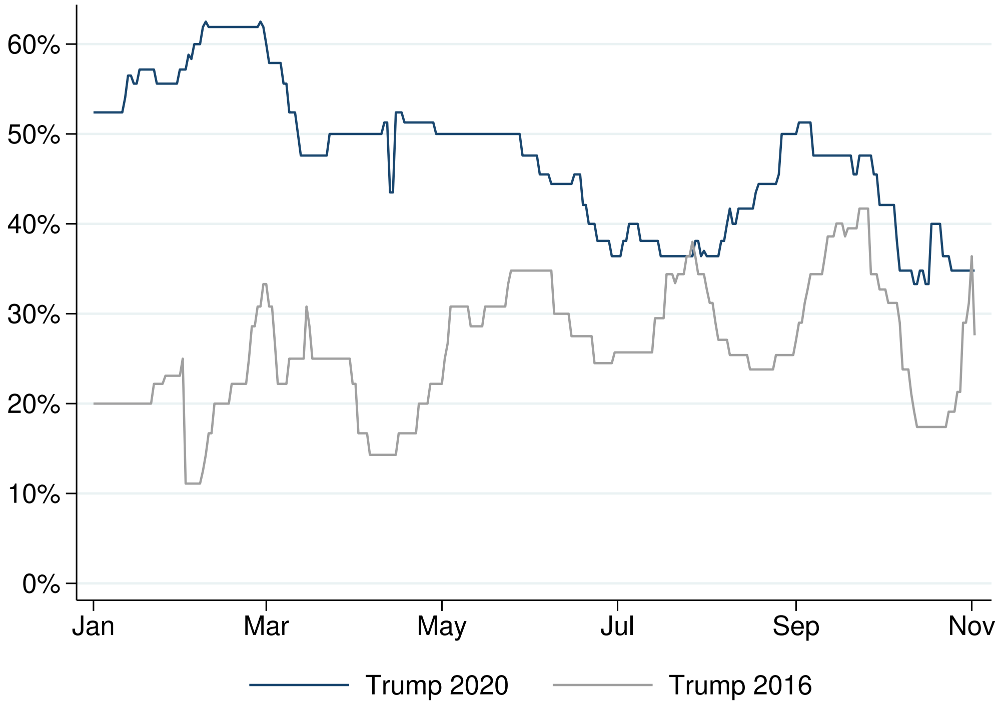
Composition of the treatment and the control group: We exploit the maximum number of observations for our benchmark estimates. We next examine whether inferences change when we require each of the included countries to have at least one participant in the control group and the treatment group. The results, shown in Table C.8, are almost identical to our benchmark estimates.

Experts' premia on past macroeconomic conditions: The random assignment of experts into survey waves should eliminate the confounding influence of past macroeconomic conditions on the expectations of respondents. Our baseline strategy to account for any imbalance of the treatment and control group regarding respondents' host countries that may violate the identifying assumption is to include fixed effects for countries. To more directly take into account the past state of the economy, we compute the "premia" over past macroeconomic conditions each expert implicitly reports when telling us about their expectations. Using the expert-level premium relative to the previous year's levels of our macroeconomic variables (Table C.9) or the average over the Trump period (Table C.10) does not change the inferences.

Changes in the econometric specification: We also investigate whether our results depend on the econometric specification of Equation 3.1. In Tables C.11–C.13, we alter the key assumptions of our benchmark model. Our benchmark estimates are obtained by allowing for arbitrary heteroskedasticity of standard errors. We might expect standard errors to be nested in country clusters, but the number of observations for many countries is low, potentially biasing our estimates toward non-robust errors. If we nevertheless model standard errors to be clustered on the country level, we observe no changes in the inferences (see Table C.11) or cluster errors on the survey-day level (not reported). We account for the time experts used to fill out our survey to account for the effort of experts and expert-specific personality characteristics. The inferences do not change when we exclude this control (Table C.12). As an additional analysis to examine whether the personal characteristics of experts influence their expectations, we augment our benchmark model with socioeconomic and biographic factors (age, education, affiliation, field of study). Given that our randomization process produced balanced samples, experts' characteristics should not influence the inferences. Table C.13 confirms this conjecture.

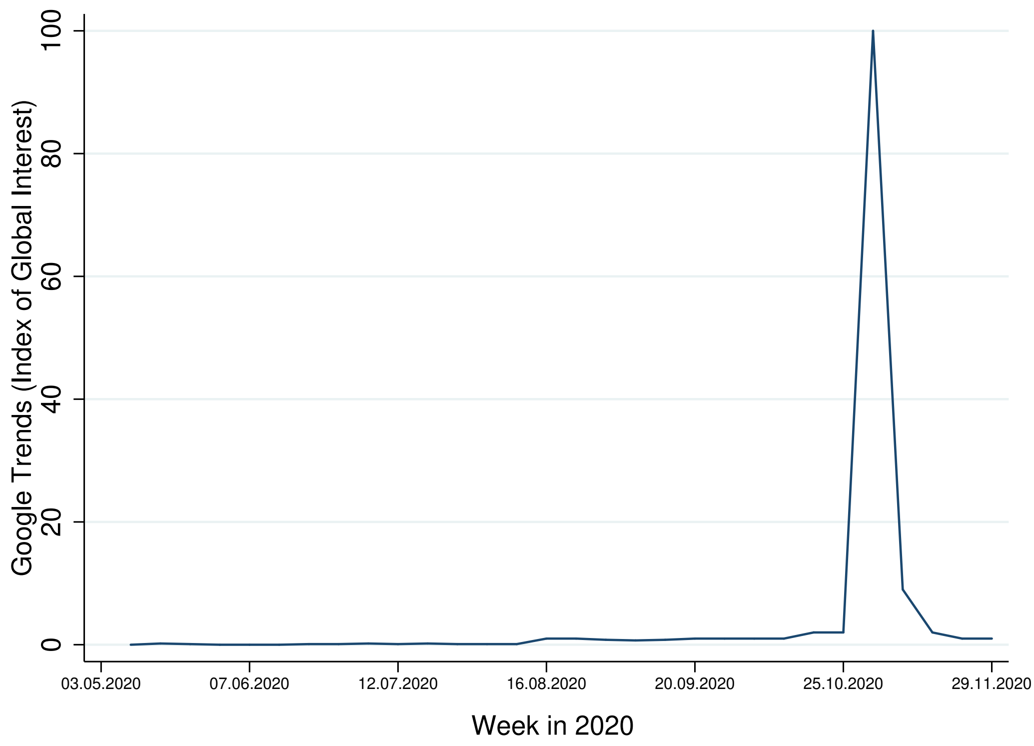
C Supplementary figures

Figure C.5 : Trump's chances of winning the US presidential election implied by odds of bookmakers, 2016 versus 2020



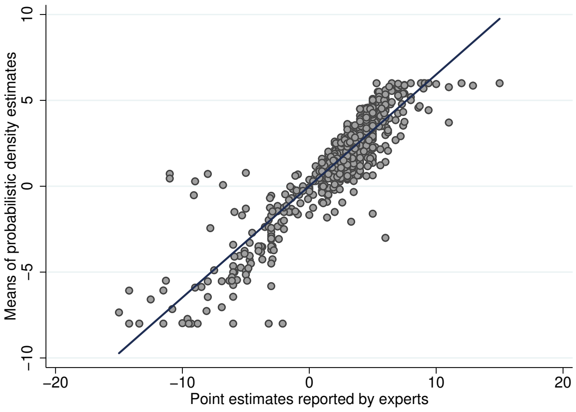
Notes: The figure shows how Trump's chance of winning the election, suggested by bookmakers' odds, has developed. The figure covers the period between 1 January of the election year (2016 and 2020) and election day. Data are taken from Eaton (2020).

Figure C.6 : Global interest in the 2020 US presidential election, Google Trends

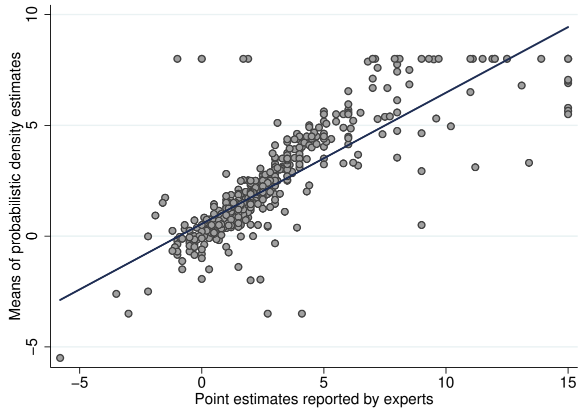


Notes: The figure shows an index reflecting the global search interest in the US presidential election on the internet platform Google. Data is acquired via the internet tool "Google Trends", which shows how Google searches have developed over time. The figure refers to the global interest in the term "US presidential election" (including the referring expressions in the respective national languages).

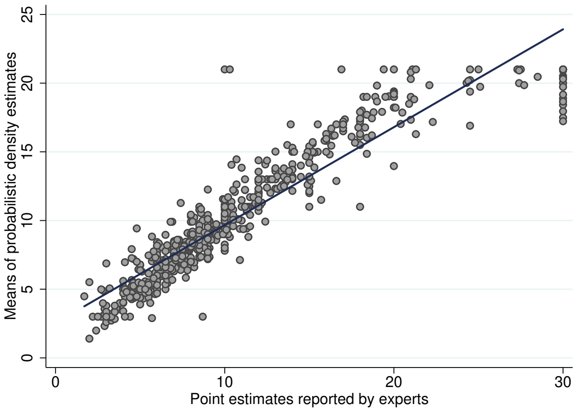
Figure C.7 : Consistency of expectations: Point estimates and means obtained from probabilistic density estimates



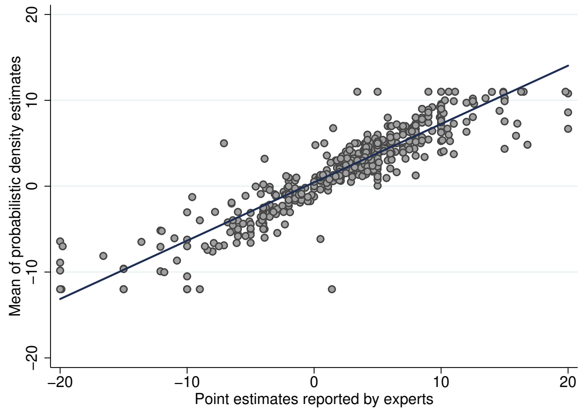
(a) Real GDP growth (in %)



(b) Inflation rate (in %)



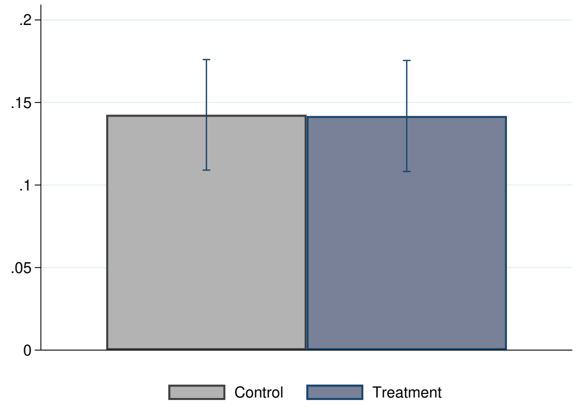
(c) Unemployment rate (in %)



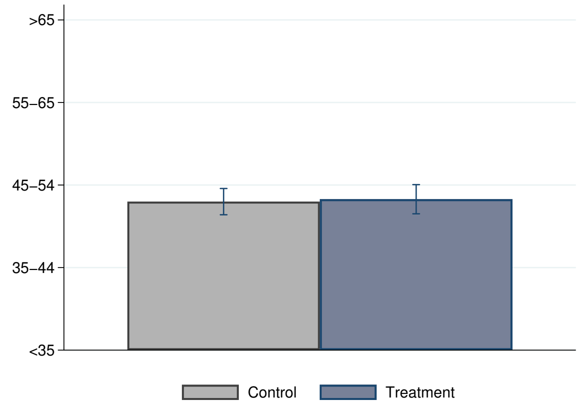
(d) Change in foreign trade volume (in %)

Notes: The figure shows the relationship between the point estimates of experts' expectations and the means retrieved from the probabilities experts assigned to bins. For each bin, experts are asked to quantify the expected chance that the variable will fall into the respective bin category.

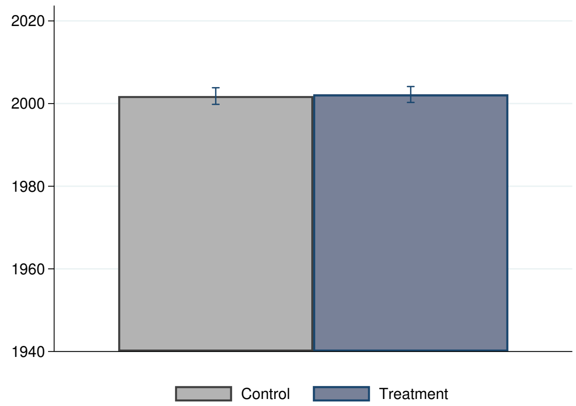
Figure C.8 : Balance tests for gender, age, academic age, and education



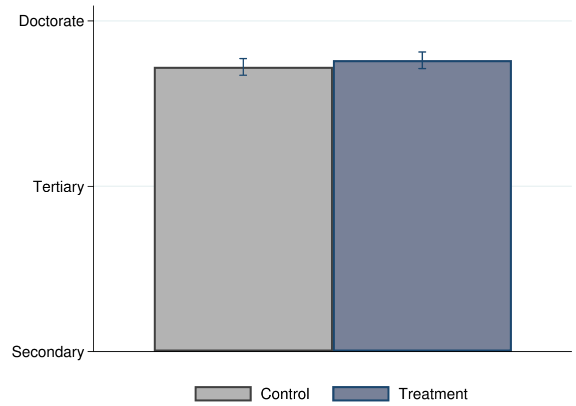
(a) Gender (1=female)



(b) Age (in intervals)



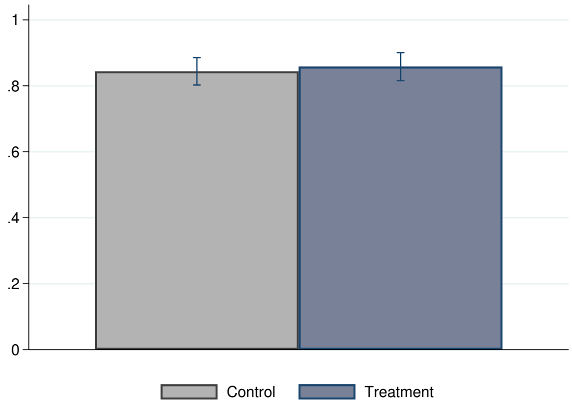
(c) Academic age (year of PhD)



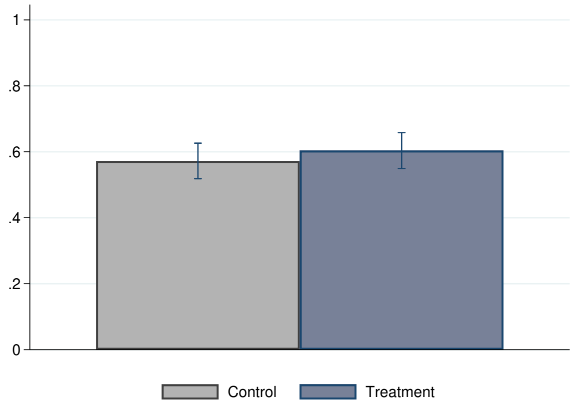
(d) Education

Notes: The figure shows the sample means for experts in our control group (asked prior to the 2020 US presidential election, grey bars) and the treatment group (asked after Joe Biden has been called president, blue bars). Vertical lines represent the 95% confidence interval.

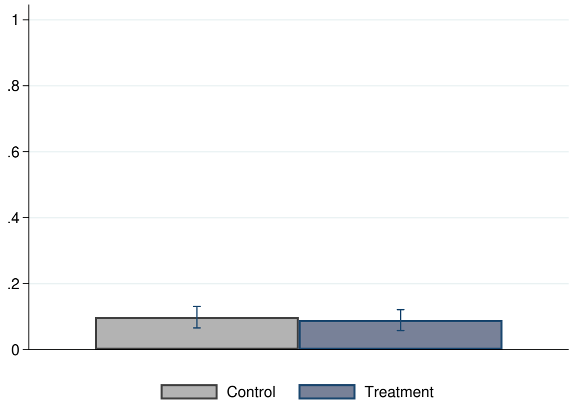
Figure C.9 : Balance tests for experts' field of study and affiliation



(a) Economist by training (1=economist)



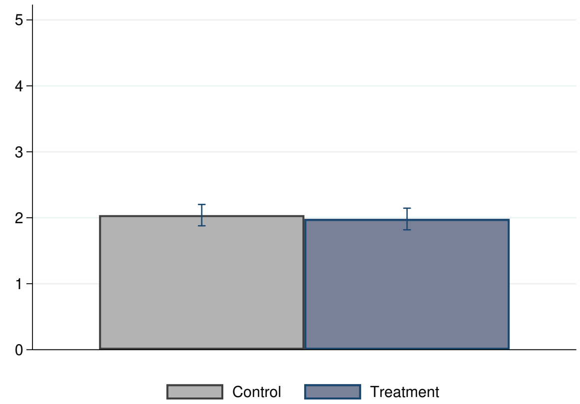
(b) University employee (1=university)



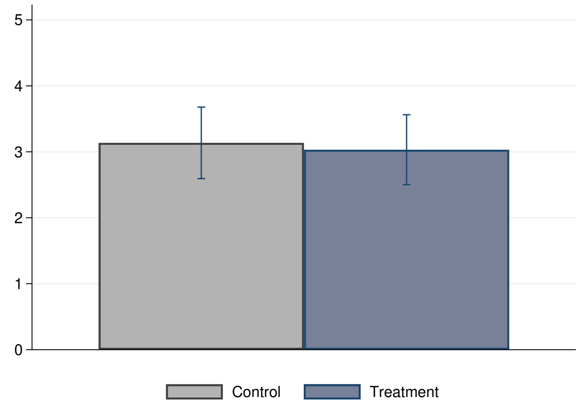
(c) Central bank employee (1=central bank)

Notes: The figure shows the sample means for experts in our control group (asked prior to the 2020 US presidential election, grey bars) and the treatment group (asked after Joe Biden has been called president, blue bars). Vertical lines represent the 95% confidence interval.

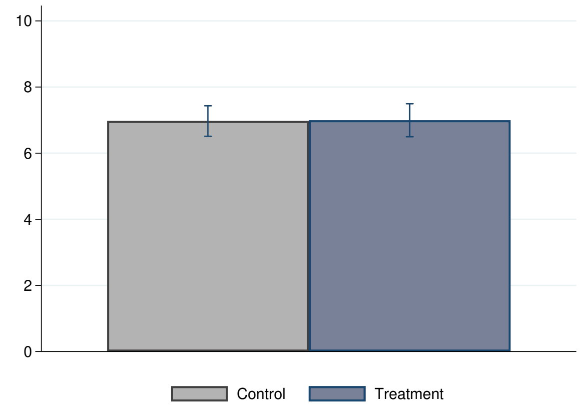
Figure C.10 : Balance tests for the past macroeconomic environment of experts (year prior to election, 2019)



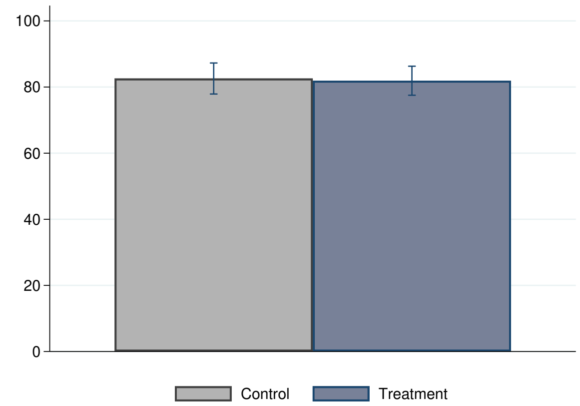
(a) GDP growth in 2019 (in %)



(b) Inflation in 2019 (in %)



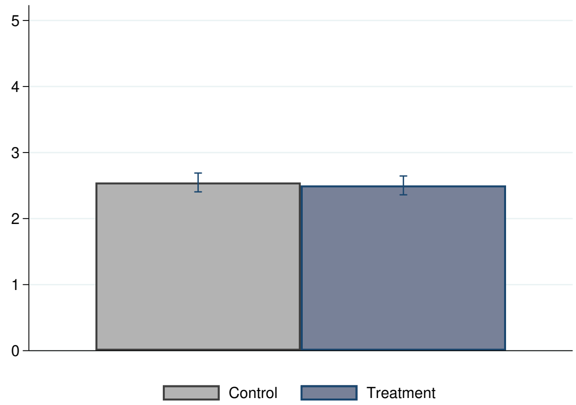
(c) Unemployment in 2019 (ILO est. in %)



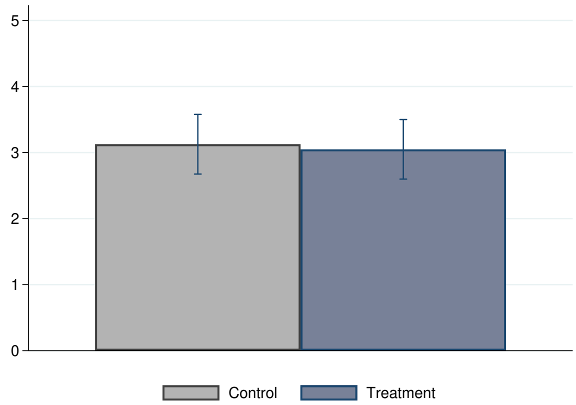
(d) Trade as a share of GDP in 2019 (in %)

Notes: The figure shows the sample means for experts in our control group (asked prior to the 2020 US presidential election, grey bars) and the treatment group (asked after Joe Biden has been called president, blue bars). Vertical lines represent the 95% confidence interval.

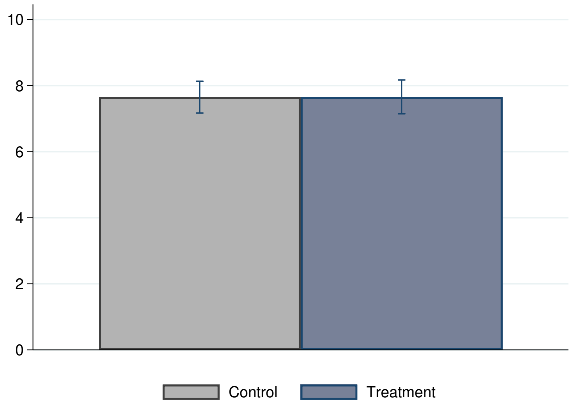
Figure C.11 : Balance tests for the past macroeconomic environment of experts (period of Trump presidency, 2016–2019)



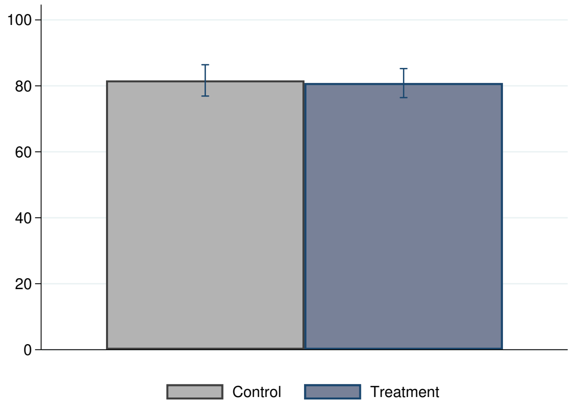
(a) **GDP growth 2016-2019 (in %)**



(b) **Inflation 2016-2019 (in %)**



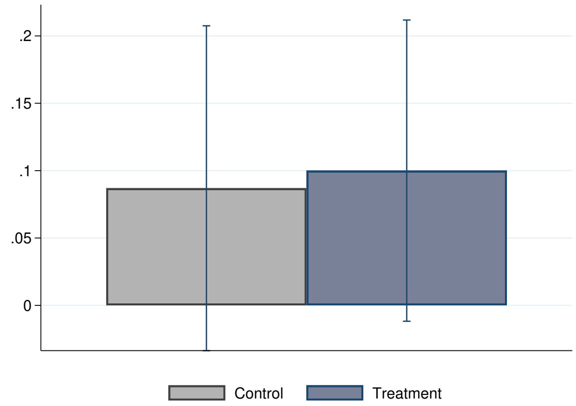
(c) **Unemployment 2016-2019 (ILO est. in %)**



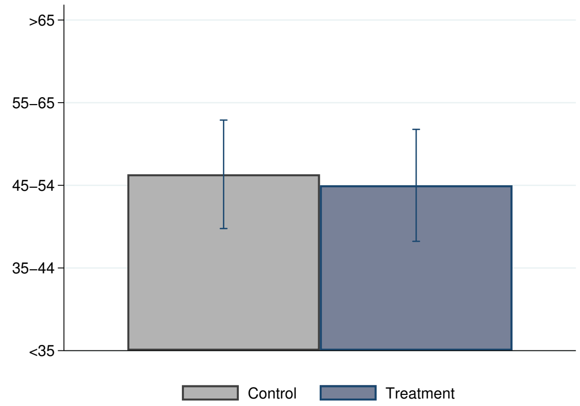
(d) **Trade as a share of GDP 2016-2019 (in %)**

Notes: The figure shows the sample means for experts in our control group (asked prior to the 2020 US presidential election, grey bars) and the treatment group (asked after Joe Biden has been called president, blue bars). Vertical lines represent the 95% confidence interval.

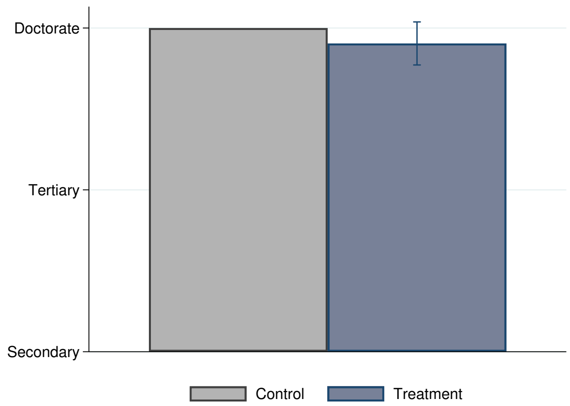
Figure C.12 : Balance tests for US-based experts



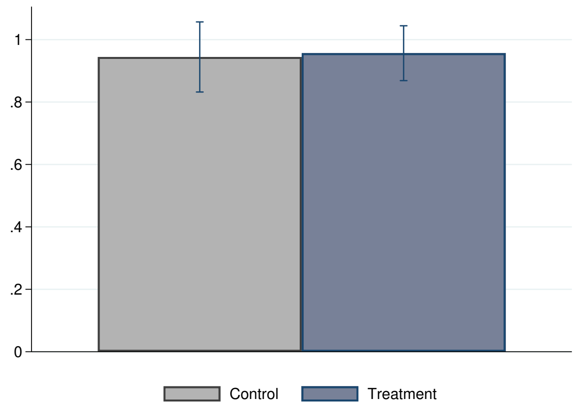
(a) Gender (1=female)



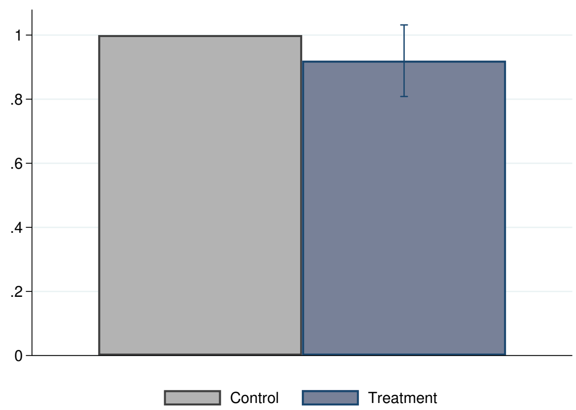
(b) Age (in intervals)



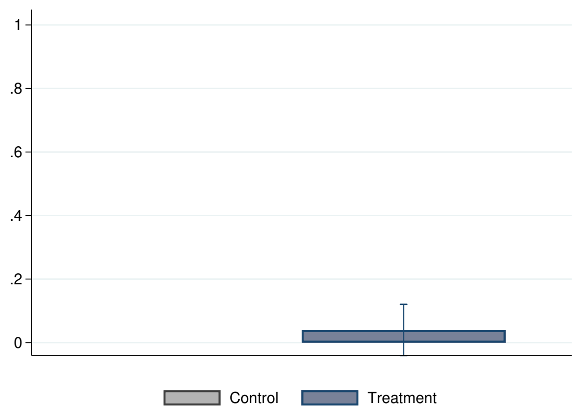
(c) Education



(d) Economist by training (1=economist)



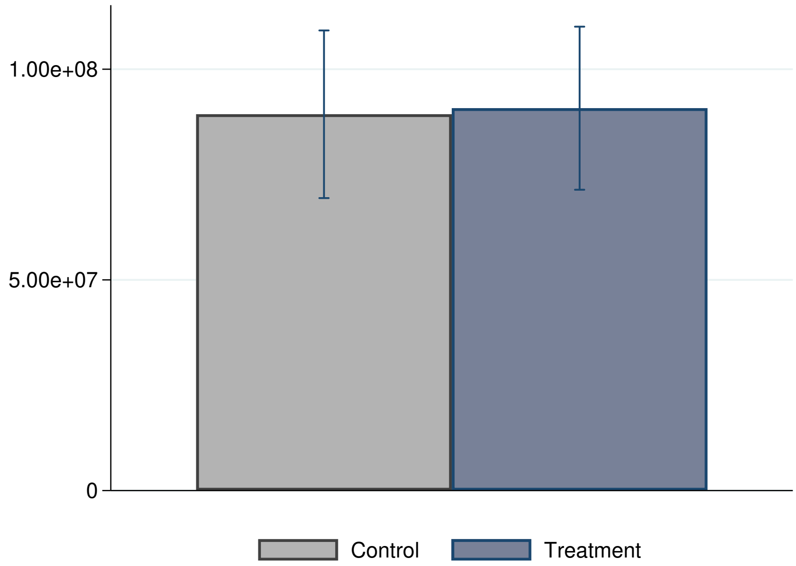
(e) University employee (1=university)



(f) Central bank employee (1=central bank)

Notes: The figure shows the sample means for US experts in our control group (asked prior to the 2020 US presidential election, grey bars) and the treatment group (asked after Joe Biden has been called president, blue bars). Vertical lines represent the 95% confidence interval.

Figure C.13 : Balance tests for country size (total population in 2019)



Notes: The figure shows the sample means for experts in our control group (asked prior to the 2020 US presidential election, grey bars) and the treatment group (asked after Joe Biden has been called president, blue bars). Vertical lines represent the 95% confidence interval.

D Supplementary tables

Table C.1 : Balance tests: Sample means of control and treatment group and t-tests for differences in mean characteristics

(I) **Variable**	(II) **Control (mean)**	(III) **Treatment (mean)**	(IV) **Difference (t)**
	Socio-economic characteristics		
Sex (1 = female)	0.143	0.142	0.001 (0.03)
Age (coded in groups, 1–5)	2.799	2.828	-0.029 (0.24)
Education (coded in groups, 1–5)	3.722	3.762	-0.040 (1.10)
Degree in economics (1 = yes)	0.844	0.858	-0.014 (0.47)
Affiliation: University (1 = yes)	0.572	0.604	-0.032 (0.81)
Affiliation: Central Bank (1 = yes)	0.098	0.089	0.009 (0.39)
	Macroeconomic environment		
GDP growth 2019 (%)	2.040	1.982	0.059 (0.500)
Inflation 2019 (%)	3.135	3.032	0.104 (0.27)
Unemployment 2019 (ILO estimate in %)	6.972	6.995	-0.023 (0.07)
Trade as a share of GDP 2019 (%)	82.575	81.908	0.667 (0.20)
Population 2019 (in million)	89.3	90.7	-1.440 (0.10)

Notes: The table reports the mean levels of key socio-economic characteristics and the past macroeconomic environment of experts included in our sample for the control group (Column II) and the treatment group (Column III). The differences between the means are reported in Column IV, with test statistics of a two-sample t-test reported in parentheses. To guarantee the anonymity of experts, we do not ask respondents for socioeconomic characteristics other than sex, age, and education. Also, the age of respondents is coded in classes between 1–5 to guarantee that individual participants cannot be identified in the data.

Table C.2 : The 2020 US presidential election and economic expectations of experts, effects for experts from host countries that were in trade war with the United States under the Trump administration

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Benchmark results: Full sample of experts			
Treatment (1 = Biden president)	0.984*	-0.0289	-0.566	1.375
	(0.518)	(0.253)	(0.451)	(0.863)
Number of experts	662	665	677	569
Number of countries	68	68	68	68
R-Squared	0.207	0.760	0.794	0.176
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
	Panel B: Sample of experts from countries with US trade-war			
Treatment (1 = Biden president)	1.433*	-0.119	0.140	2.286**
	(0.820)	(0.187)	(0.714)	(1.024)
Number of experts	326	320	334	278
Number of countries	25	25	25	25
R-Squared	0.138	0.509	0.762	0.150
Dist Elec. FE	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day. Panel A presents the results obtained using the full sample of experts as a benchmark. Panel B reports results on the sub-sample of experts whose countries were in a trade war with the United States during the Trump presidency.

** Significant at the 5% level,

* Significant at the 10% level

Table C.3 : The 2020 US presidential election and economic expectations of experts, accounting for the number of active SARS-CoV-2 cases

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	1.252**	-0.0909	-0.521	1.699*
	(0.576)	(0.286)	(0.477)	(0.929)
Number of experts	662	665	677	569
Number of countries	68	68	68	68
R-Squared	0.208	0.760	0.794	0.177
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.445**	-0.161	-0.465	1.965**
	(0.634)	(0.305)	(0.507)	(0.972)
Number of experts	620	620	632	541
Number of countries	67	67	67	67
R-Squared	0.213	0.772	0.792	0.184
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of experts	42	45	45	28
Countries				
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. The regressions augment Equation 3.1 by adding the number of active SARS-CoV-2 cases. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. "Country-FE" are fixed effect on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,

* Significant at the 10% level

Table C.4 : The 2020 US presidential election and economic expectations of experts, excluding experts that participated in the survey after November 8, 2020

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	1.201**	0.0140	-0.488	1.287
	(2.15)	(0.05)	(-1.00)	(1.44)
Number of experts	403	410	411	358
Number of countries	67	67	67	67
R-Squared	0.227	0.820	0.795	0.219
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.415**	-0.143	-0.461	1.888**
	(0.622)	(0.289)	(0.568)	(0.874)
Number of experts	376	379	381	338
Number of countries	66	66	66	66
R-Squared	0.230	0.833	0.793	0.232
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.0145	0.960	-0.371	-3.686
	(1.154)	(0.629)	(0.491)	(2.594)
Number of experts	27	31	30	20
Countries				
R-Squared	0.163	0.346	0.463	0.499
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts, excluding the time since the announcement of the effectivity of the Covid-19 vaccine developed by Pfizer and BioNTech. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. "Country-FE" are fixed effect on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level

Table C.5 : The 2020 US presidential election and economic expectations of experts, narrow band around election day, data from November 2 & 3, 2020 (control group) and November 8, 2020 (treatment group)

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	1.840**	-0.268	-0.224	-0.430
	(0.903)	(0.243)	(0.879)	(1.324)
Number of experts	171	173	170	149
Number of countries	67	67	67	67
R-Squared	0.516	0.916	0.801	0.573
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	2.003**	-0.233	-0.337	0.0897
	(0.973)	(0.241)	(0.958)	(1.345)
Number of experts	161	162	159	142
Number of countries	66	66	66	66
R-Squared	0.523	0.932	0.798	0.616
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.520	-1.018	1.904***	-9.266
	(0.667)	(1.072)	(0.200)	(6.299)
Number of experts	10	11	11	7
R-Squared	0.0109	0.0705	0.585	0.606
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts, using only data from the day before the election (2nd and 3rd of November) and the first day after Biden has been called president (8th of November). Data from the 3rd of November include observations from the time before the first polling station opened. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

*** Significant at the 1% level,
** Significant at the 5% level

Table C.6 : The 2020 US presidential election and economic expectations of experts, restricting the sample to experts from countries with at least 10 participants

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	1.098**	0.266	-0.184	1.160
	(0.556)	(0.202)	(0.513)	(0.981)
Number of experts	485	488	494	406
Number of countries	29	29	29	29
R-Squared	0.150	0.782	0.835	0.133
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.441**	0.185	0.155	2.181**
	(0.679)	(0.220)	(0.625)	(0.998)
Number of experts	407	406	413	343
Number of countries	28	28	28	28
R-Squared	0.152	0.755	0.865	0.150
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of experts	42	45	45	28
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts, using only data from experts for which we have at least 10 host country observations in our survey. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level

Table C.7 : The 2020 US presidential election and economic expectations of experts, restricting the sample to experts from countries with at least two participants

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.882*	-0.0369	-0.618	1.340
	(0.512)	(0.249)	(0.445)	(0.849)
Number of experts	692	696	708	598
Number of countries	85	85	85	85
R-Squared	0.222	0.787	0.809	0.236
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.036*	-0.187	-0.609	1.895**
	(0.564)	(0.270)	(0.510)	(0.831)
Number of experts	650	651	663	570
Number of countries	84	84	84	84
R-Squared	0.226	0.797	0.808	0.245
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of experts	42	45	45	28
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts, using only data from experts for which we have at least two host country observations in our survey. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.8 : The 2020 US presidential election and economic expectations of experts, restricting the sample to experts from countries that are included in the treatment and control group

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.981* (0.518)	-0.0324 (0.253)	-0.566 (0.451)	1.368 (0.864)
Number of experts	655	657	670	561
Number of countries	65	65	65	65
R-Squared	0.206	0.760	0.794	0.172
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.155** (0.572)	-0.187 (0.275)	-0.548 (0.518)	1.943** (0.848)
Number of experts	613	612	625	533
Number of countries	64	64	64	64
R-Squared	0.210	0.772	0.792	0.179
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211 (1.123)	0.974 (0.630)	-0.655 (0.567)	-4.847 (4.620)
Number of experts	42	45	45	28
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts, using only observations for host countries for which we have experts in both the treatment and the control group. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.9 : The 2020 US presidential election and economic expectations of experts, expert-level premia on past macroeconomic performance of their host country (premia relative to previous year)

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.975*	-0.0603	-0.459	1.329
	(0.520)	(0.254)	(0.444)	(0.874)
Number of experts	659	656	638	550
Number of countries	67	67	67	67
R-Squared	0.271	0.906	0.365	0.415
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.145**	-0.226	-0.424	1.908**
	(0.575)	(0.276)	(0.511)	(0.857)
Number of experts	613	612	625	533
Number of countries	66	66	66	66
R-Squared	0.277	0.913	0.366	0.434
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of experts	42	45	45	28
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Premia are calculated by subtracting the observed values of macroeconomic variables in 2019 from experts' expectations for 2021. Data is taken from World Bank (2020). Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. "Country-FE" are fixed effect on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.10 : The 2020 US presidential election and economic expectations of experts, expert-level premia on past macroeconomic performance of their host country (premia relative to average of Trump period, 2016–2019)

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.975*	-0.0277	-0.563	1.305
	(0.520)	(0.255)	(0.454)	(0.866)
Number of experts	659	656	638	550
Number of countries	67	67	67	67
R-Squared	0.265	0.856	0.453	0.226
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.145**	-0.188	-0.544	1.870**
	(0.575)	(0.278)	(0.521)	(0.851)
Number of experts	610	609	617	530
Number of countries	66	66	66	66
R-Squared	0.269	0.865	0.455	0.239
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of experts	42	45	45	28
R-Squared	0.140	0.438	0.356	0.325
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Premia are calculated by subtracting the average of macroeconomic variables over the Trump period for which data is available (2016–2019) from experts' expectations for 2021. Data is taken from World Bank (2020). Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.11 : The 2020 US presidential election and economic expectations of experts, changes in specification i: Baseline results with clustered standard errors

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.984*	-0.0289	-0.566	1.375
	(0.499)	(0.301)	(0.536)	(0.945)
Number of experts	662	665	677	569
Number of countries	68	68	68	68
R-Squared	0.207	0.760	0.794	0.176
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.159**	-0.183	-0.547	1.949**
	(0.537)	(0.311)	(0.622)	(0.793)
Number of experts	620	620	632	541
Number of countries	67	67	67	67
R-Squared	0.211	0.772	0.792	0.184
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.211	0.974	-0.655	-4.847
	(1.123)	(0.630)	(0.567)	(4.620)
Number of experts	42	45	45	28
R-Squared	0.211	0.772	0.792	0.184
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Inferences are based on standard errors that are robust to arbitrary heteroskedasticity and that are clustered within countries. Panel C is based on a single cluster, but we report these results using Huber-White standard errors for comparison. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.12 : The 2020 US presidential election and economic expectations of experts, changes in specification ii: Exclude measure for expert effort (duration in seconds)

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.930*	-0.0476	-0.563	1.373
	(0.518)	(0.250)	(0.451)	(0.862)
Number of experts	673	671	678	569
Number of countries	68	68	68	68
R-Squared	0.207	0.758	0.794	0.176
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	No	No	No	No
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.109*	-0.203	-0.545	1.947**
	(0.576)	(0.272)	(0.518)	(0.846)
Number of experts	630	626	633	541
Number of countries	67	67	67	67
R-Squared	0.212	0.770	0.792	0.183
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	No	No	No	No
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.331	0.967	-0.661	-4.757
	(1.047)	(0.621)	(0.568)	(4.310)
Number of experts	43	45	45	28
R-Squared	0.0967	0.437	0.355	0.203
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	No	No	No	No

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Inferences are based on standard errors that are robust to arbitrary heteroskedasticity and that are clustered within countries. Panel C is based on a single cluster, but we report these results using Huber-White standard errors for comparison. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). "Country-FE" are fixed effect on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.13 : The 2020 US presidential election and economic expectations of experts, changes in specification iii: Include more controls

Dependent variables: Key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	0.984*	-0.0289	-0.566	1.375
	(0.518)	(0.253)	(0.451)	(0.863)
Number of experts	673	671	678	569
Number of countries	68	68	68	68
R-Squared	0.207	0.758	0.794	0.176
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	1.109*	-0.203	-0.545	1.947**
	(0.576)	(0.272)	(0.518)	(0.846)
Number of experts	630	626	633	541
Number of countries	67	67	67	67
R-Squared	0.212	0.770	0.792	0.183
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.331	0.967	-0.661	-4.757
	(1.047)	(0.621)	(0.568)	(4.310)
Number of experts	43	45	45	28
R-Squared	0.0967	0.437	0.355	0.203
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. The specifications include an additional set of controls (age, education, affiliation, field of study). Inferences are based on standard errors that are robust to arbitrary heteroskedasticity and that are clustered within countries. Panel C is based on a single cluster, but we report these results using Huber-White standard errors for comparison. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in 2021 in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

** Significant at the 5% level,
* Significant at the 10% level

Table C.14 : The 2020 US presidential election and economic expectations of experts, long-term expectations for 2023

Dependent variables: Key macroeconomic variables until 2023				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	-0.309	0.0818	-0.308	-0.533
	(0.258)	(0.152)	(0.352)	(0.972)
Number of experts	703	679	675	558
Number of countries	68	68	68	68
R-Squared	0.341	0.790	0.785	0.131
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	-0.270	0.0433	-0.291	-0.494
	(0.291)	(0.172)	(0.412)	(1.020)
Number of experts	652	629	626	529
Number of countries	67	67	67	67
R-Squared	0.347	0.805	0.782	0.135
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.548	0.319	-0.401	-0.970
	(0.516)	(0.278)	(0.373)	(3.202)
Number of experts	51	50	49	29
R-Squared	0.216	0.851	0.421	0.218
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Expectations are measured regarding four key macroeconomic variables for the year 2023: The growth rate of GDP in % (Column I), the rate of inflation in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP from the beginning of 2021 until the end of 2023 in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

Table C.15 : The 2020 US presidential election and economic expectations of experts, long-term expectations for 2023, sample of experts that are also included in the baseline sample

Dependent variables: Key macroeconomic variables until 2023				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Full sample of experts			
Treatment (1 = Biden president)	-0.309	0.0818	-0.308	-0.533
	(0.258)	(0.152)	(0.352)	(0.972)
Number of experts	703	679	675	558
Number of countries	68	68	68	68
R-Squared	0.341	0.790	0.785	0.131
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Excluding experts from the United States			
Treatment (1 = Biden president)	0.0263	-0.00204	-0.376	-0.0553
	(0.221)	(0.181)	(0.438)	(1.041)
Number of experts	581	579	595	499
Number of countries	67	67	67	67
R-Squared	0.407	0.824	0.786	0.136
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Experts from the United States			
Treatment (1 = Biden president)	-0.264	0.411	-0.501	-0.404
	(0.300)	(0.311)	(0.384)	(3.344)
Number of experts	41	44	44	26
R-Squared	0.348	0.867	0.449	0.245
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Expectations are measured regarding four key macroeconomic variables for the year 2023: The growth rate of GDP in % (Column I), the rate of inflation in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP from the beginning of 2021 until the end of 2023 in % (Column IV). Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. “Country-FE” are fixed effect on the country level, “Dist. Elec. FE” are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and “Survey time” denoted the duration (in seconds) experts took to fill out their survey.

Table C.16 : The 2020 US presidential election and economic expectations of experts, effects on experts' uncertainty, alternative measures of uncertainty

Dependent variables: Uncertainty about key macroeconomic variables in 2021				
	$\frac{d}{dt}$GDP (I)	Inflation Rate (II)	Unemployment (III)	ΔTrade Vol. (IV)
	Panel A: Empirical Standard Deviation			
Treatment (1 = Biden president)	2.196*	1.935*	-0.671	0.305
	(1.159)	(1.062)	(1.383)	(1.568)
Number of experts	662	662	662	662
Number of countries	68	68	68	68
R-Squared	0.187	0.191	0.179	0.235
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel B: Mean absolute deviation between second and fourth quintile			
Treatment (1 = Biden president)	1.986	7.742*	-0.250	-5.552
	(7.669)	(4.339)	(6.358)	(5.376)
Number of experts	662	662	662	662
Number of countries	67	67	67	67
R-Squared	0.306	0.535	0.348	0.209
Country-FE	Yes	Yes	Yes	Yes
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes
	Panel C: Empirical variance			
Treatment (1 = Biden president)	83.07*	72.55*	-3.550	23.86
	(43.823)	(38.655)	(47.604)	(45.374)
Number of experts	662	662	662	662
R-Squared	0.189	0.209	0.207	0.213
Dist Elec. FE	Yes	Yes	Yes	Yes
Survey time	Yes	Yes	Yes	Yes

Notes: The table shows the results of our estimations on the effect of the 2020 US presidential elections on the expectations of international experts. Expectations are measured regarding four key macroeconomic variables for the year 2021: The growth rate of GDP in % (Column I), the rate of inflation in % (Column II), the level of unemployment in % (Column III), and the change in trade as a share of GDP in % (Column IV). The table presents results on the effect of the US presidential election on experts' degree of uncertainty about these variables. Estimates are based on a comparable sample of observations. Robust standard errors (adjusted for arbitrary heteroskedasticity) are reported in parentheses. "Country-FE" are fixed effect on the country level, "Dist. Elec. FE" are fixed effects for the distance (in days) between the time experts filled out their survey and the election day, and "Survey time" denoted the duration (in seconds) experts took to fill out their survey.

* Significant at the 10% level

D Appendix to Chapter 4

A Presidential elections

A natural addition to my study is to examine electoral outcomes on different levels of government. Studying the impact of deadly tornadoes on electoral outcomes of local government is intriguing, however, not feasible with my event study design due to the absence of a suitable comparison group. Turning to the federal level, county-level data availability of electoral outcomes is best for presidential elections. The US president as head of the executive has far-reaching powers such as commanding the armed forces, vetoing legislation passed by congress, conducting foreign relations (negotiating treaties and international agreements), and appointing federal judges, ambassadors, executive branch officials, and the like. She also plays a decisive role in disaster relief efforts when state capacities are exhausted. The excursion into county-level presidential election outcomes follows three steps. First, I focus on voters' motivation in presidential elections. Second, I present results on how deadly tornadoes affected voter behavior in presidential elections. Third, I discuss the results pointing out similarities and differences to findings in gubernatorial elections.

> *"It's the economy, stupid"*
> — James Carville, 1992

The phrase, coined in Bill Clinton's 1992 presidential campaign, soon became representative of the political ramifications resulting from the economic conditions during presidential elections. Politicians and analysts have long acknowledged the decisive role of the economy in making presidents. Past incumbent defeats, such as Herbert Hoover's and Jimmy Carter's, can be attributed to economic crises. Clinton's team used the quip successfully to unseat George H W Bush, despite the incumbent's overwhelming job approval by US voters just one and a half years earlier (89%).[1] In addition to the economy as arguably the most important factor in determining presidential election outcomes (*"economic voting"*) (e.g., Erikson, 1989; Lewis-Beck and Stegmaier, 2000; Zaller, 2004; Healy and Lenz, 2017; Lewis-Beck and Martini, 2020), the second influential topic voters base their electoral decision on is foreign policy (e.g., Aldrich et al., 1989, 2006; Page and Shapiro, 2010; Johnstone and Priest, 2017) and the link between the two through, for instance, international trade (Jensen et al., 2017). Nickelsburg and Norpoth (2000) wrote "to maintain public support the chief executive must be 'commander-in-chief' and 'chief economist' in equal measure" (p. 313). The *Financial*

[1] A job approval rating of 89% has been the highest for any sitting US president since Gallup started recording data in the 1940s. The mark was surpassed by one p.p. when his son George W Bush proclaimed the war on terror after 9/11 (Gallup, 2001).

Times, referring to Carville's phrase, stated in February 2022 regarding the next electoral cycle: "It's geopolitics, stupid".

Comparably little is known about the influence of social welfare policies on US presidential election outcomes. The salience of issues owned by either the Democrats or the Republicans is correlated with voter turnout for the respective party (Petrocik et al., 2003). Studies examining individual US presidential elections show a positive correlation between health insurance coverage and the vote share for the Democratic presidential candidate (Hollingsworth et al., 2019). However, they do not find a causal link between social insurance issues and the presidential vote (Alvarez and Nagler, 1998).

Multiple layers of government design, fund, and administer social insurance programs which leads to voters being typically not aware of the program's exact origins and responsibilities. The federal government funds large programs, such as Social Security and Medicare, and manages tax credits under the umbrella of the social insurance system (e.g., Earned Income Tax Credit and Child Tax Credit). Most programs, however, are jointly funded by the federal and state government and administered by states. States possess substantial discretionary power to influence eligibility criteria which results in considerable variability in coverage rates across states. In the example of Medicaid, some states have adopted comprehensive insurance schemes covering many optional health services, while 11 states have not even adopted the expansion under the Affordable Care Act. The relatively low priority of social welfare policies in US presidential elections, along with the involvement of different layers of government in social insurance programs, suggests that voters might not express their change of preferences for more social protection after a deadly tornado in US presidential elections. Voters may only express their increased demand for social protection in state elections.

So far, we have observed significant changes in electoral behavior after a deadly tornado in US gubernatorial elections. To investigate whether the shift in voters' preferences toward the policy platform offered by Democratic candidates also materializes in an increased Democratic vote share on the federal level, I run my event study model with data on US presidential election outcomes on the county level. Pre-trend coefficients are negative and sometimes statistically significant at conventional levels, implying that treated counties vote, on average, more Republican in presidential elections (see Column I, Table D.5). In the first election after the deadly tornado, the coefficient is still negative but elevated compared to the pre-trend coefficients, suggesting relatively more votes for the Democratic candidate. However, the coefficient is indistinguishable from zero at conventional levels. When increasing the treatment intensity by including only counties experiencing a deadly tornado with five or more fatalities less than 12 months before the presidential election in the treatment group, the coefficient for the change in the Democratic two-party vote share turns positive in the election after the treatment (see Column II, Table D.5). Due to the limited number of observations, standard errors are large and treatment coefficients are not statistically significant. Pre-trend coefficients are negative and similar to the previous model specification.

Taken together, the average Democratic two-party vote share in presidential elections is higher in counties experiencing a deadly tornado. However, the evidence is not strong enough to draw a causal link between a deadly tornado and an increase in the vote share for the Democratic presidential candidate. Voter motivation differs between the federal and the state level which leads to diverging voter behavior. Voters' response to local shocks materializes to different degrees across levels of government. Local events do normally not play a decisive role in voters' decision-making on the federal level.

B Supplementary tables

Table D.1 : Sample means of control and treatment group and t-tests for differences in mean Democratic two-party vote shares

(I) Variable	(II) Control (mean)	(III) Treatment (mean)	(IV) Difference (t)
	Panel A: Multiple treatments allowed		
Democratic two-party vote share (%)	48.786	50.579	1.793
			(4.775)
	Panel B: Only first treatments (since 1950)		
Democratic two-party vote share (%)	49.180	52.071	2.891
			(5.705)

Notes: The table reports the mean levels of the Democratic two-party vote share of counties included in my sample for the control group (Column II) and the treatment group (Column III). The differences between the means are reported in Column IV, with test statistics of a two-sample t-test reported in parentheses. In Panel A, the control group is defined as counties which did not experience a deadly tornado since the last gubernatorial election. The treatment group is defined as counties which experienced a deadly tornado since the last gubernatorial election. The control group in Panel B includes counties which did not experience a deadly tornado from 1950 to the election before the deadly tornado and counties which did not experience a deadly tornado in the whole sample period (1950–2020). The treatment group in Panel B only includes counties which experienced a deadly tornado since the last gubernatorial election with that tornado being the first lethal one since 1950.

Table D.2 : Frequency of deadly tornadoes across counties, 1950–2020

(I) **Deadly tornado (count)**	(II) **Frequency**	(III) **Percent**	(IV) **Cumulative**
1	776	55.87	55.87
2	354	25.49	81.35
3	141	10.15	91.50
4	59	4.25	95.75
5	28	2.02	97.77
6	17	1.22	98.99
7	6	0.43	99.42
8	3	0.22	99.64
9	3	0.22	99.86
10	1	0.07	99.93
11	1	0.07	100.00

Notes: The table reports the frequency of deadly tornadoes for counties struck at least once by a deadly tornado between 1950 and 2020. A total of 1,389 counties experienced at least one deadly tornado between 1950 and 2020. Source: National Oceanic and Atmospheric Administration (2022).

Table D.3 : Baseline event study results and multiple treatments

	(I)	(II)	(III)
Deadly tornado (t - 5)	-0.046 [-0.848; 0.756]	-0.074 [-0.875; 0.727]	–
Deadly tornado (t - 4)	-0.118 [-0.861; 0.624]	-0.152 [-0.893; 0.588]	–
Deadly tornado (t - 3)	-0.132 [-0.938; 0.674]	-0.184 [-0.986; 0.619]	-0.102 [-0.826; 0.622]
Deadly tornado (t - 2)	0.098 [-0.720; 0.916]	0.031 [-0.784; 0.846]	-0.146 [-0.890; 0.599]
Deadly tornado (t - 1)	0.074 [-0.785; 0.933]	-0.013 [-0.869; 0.842]	0.008 [-0.777; 0.793]
Deadly tornado (t)	0.480** [0.051; 0.910]	0.430** [0.001; 0.859]	0.546*** [0.140; 0.953]
Deadly tornado (t + 1)	0.182 [-0.322; 0.686]	0.125 [-0.378; 0.628]	0.086 [-0.429; 0.601]
Deadly tornado (t + 2)	0.225 [-0.318; 0.767]	0.146 [-0.396; 0.689]	0.312 [-0.172; 0.797]
Deadly tornado (t + 3)	-0.013 [-0.664; 0.639]	-0.083 [-0.734; 0.569]	-0.008 [-0.559; 0.543]
Observation	37,220	37,220	41,491
County FE	✓	✓	✓
Election FE	✓	✓	✓
First treatment	✓	✓	
Controls		✓	✓
Multiple treatments			✓

Notes: The table reports event study coefficients based on Equation 4.3 and 4.4 with the 95% confidence interval in brackets. The dependent variable is the Democratic two-party vote share (in %) in US gubernatorial elections. Column I reports coefficients for the baseline specification with county and election fixed effects. Column II reports coefficients for the baseline specification with county and election fixed effects and population controls. Column III reports coefficients for the model specification with county and election fixed effects and population controls allowing for multiple treatments.

*** Significant at the 1% level,
** Significant at the 5% level

Table D.4 : Event study results to investigate incumbency effects

	(I)	(II)	(III)
Deadly tornado (t - 5)	-0.120	0.310	-0.653
	[-1.604; 1.364]	[-1.061; 1.682]	[-1.530; 0.224]
Deadly tornado (t - 4)	-0.379	0.247	-0.758
	[-1.633; 0.876]	[-0.836; 1.330]	[-1.792; 0.275]
Deadly tornado (t - 3)	-0.072	-0.191	-0.222
	[-1.454; 1.309]	[-1.390; 1.008]	[-1.291; 0.846]
Deadly tornado (t - 2)	0.177	-0.307	0.541
	[-1.190; 1.544]	[-1.439; 0.825]	[-0.574; 1.656]
Deadly tornado (t - 1)	-0.287	-0.345	0.212
	[-1.646; 1.071]	[-1.491; 0.801]	[-0.986; 1.410]
Deadly tornado (t)	0.733**	0.915***	-0.127
	[0.065; 1.400]	[0.290; 1.539]	[-0.672; 0.419]
Deadly tornado (t + 1)	0.175	0.387	-0.184
	[-0.535; 0.885]	[-0.245; 1.019]	[-0.884; 0.515]
Deadly tornado (t + 2)	0.321	0.015	0.101
	[-0.439; 1.081]	[-0.689; 0.718]	[-0.575; 0.777]
Deadly tornado (t + 3)	-0.065	0.176	-0.688
	[-0.916; 0.785]	[-0.749; 1.101]	[-1.387; 0.011]
Observation	35,434	35,672	35,309
County FE	✓	✓	✓
Election FE	✓	✓	✓
Controls	✓	✓	✓
First treatment	✓	✓	✓
Open seat only	✓		
Democratic incumbency		✓	
Republican incumbency			✓

Notes: The table reports event study coefficients based on Equation 4.3 and 4.4 with the 95% confidence interval in brackets. The dependent variable is the Democratic two-party vote share (in %) in US gubernatorial elections. Column I reports coefficients for the baseline specification with county and election fixed effects and population controls for open seat elections in period t. Column II reports coefficients for the baseline specification with county and election fixed effects and population controls for elections with Democratic party incumbency in period t. Column III reports coefficients for the baseline specification with county and election fixed effects and population controls for elections with Republican party incumbency in period t.

*** Significant at the 1% level,
** Significant at the 5% level

Table D.5 : Event study results to investigate the impact of federal disaster relief and presidential election outcomes

	(I)	(II)	(III)	(IV)
Deadly tornado (*t* - 5)	0.271	-0.013	-0.583*	-0.583*
	[-0.738; 1.281]	[-0.994; 0.969]	[-1.180; 0.013]	[-1.180; 0.013]
Deadly tornado (*t* - 4)	-0.346	-0.406	-0.478	-0.502
	[-1.327; 0.635]	[-1.344; 0.533]	[-1.133; 0.176]	[-1.157; 0.153]
Deadly tornado (*t* - 3)	-0.863	-0.928*	-0.734**	-0.722**
	[-1.938; 0.211]	[-1.957; 0.101]	[-1.440; -0.029]	[-1.430; -0.013]
Deadly tornado (*t* - 2)	-0.163	-0.373	-0.914**	-0.915**
	[-1.237; 0.911]	[-1.379; 0.634]	[-1.652; -0.177]	[-1.658; -0.172]
Deadly tornado (*t* - 1)	-0.105	-0.288	-0.718*	-0.739*
	[-1.228; 1.019]	[-1.331; 0.755]	[-1.511; 0.075]	[-1.539; 0.061]
Deadly tornado (*t*)	0.481*	0.569**	-0.165	0.265
	[-0.001; 0.963]	[0.101; 1.037]	[-0.543; 0.214]	[-1.096; 1.626]
Deadly tornado (*t* + 1)	0.184	0.272	0.131	-0.606
	[-0.380; 0.748]	[-0.302; 0.846]	[-0.310; 0.572]	[-1.907; 0.695]
Deadly tornado (*t* + 2)	-0.237	0.011	-0.065	-0.512
	[-0.825; 0.351]	[-0.574; 0.596]	[-0.592; 0.463]	[-1.900; 0.876]
Deadly tornado (*t* + 3)	0.232	0.345	0.077	0.216
	[-0.490; 0.954]	[-0.333; 1.024]	[-0.493; 0.647]	[-0.855; 1.287]
Observation	35,672	38,968	42,112	36,861
County FE	✓	✓	✓	✓
Election FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
First treatment	✓			
Multiple treatments		✓	✓	✓
No federal disaster relief	✓	✓		
Gubernatorial elections	✓	✓		
Presidential elections			✓	✓
Intense treatment				✓

Notes: The table reports event study coefficients based on Equation 4.3 and 4.4 with the 95% confidence interval in brackets. The dependent variable is the Democratic two-party vote share (in %) in US gubernatorial elections (Columns I & II) and in US presidential elections (Columns III & IV). Column I reports coefficients for the baseline specification with county and election fixed effects and population controls for deadly tornadoes without a federal disaster declaration. Column II reports coefficients for the model specification with county and election fixed effects and population controls for deadly tornadoes without a federal disaster declaration allowing for multiple treatments. Column III reports coefficients for the model specification with county and election fixed effects and population controls for US presidential elections allowing for multiple treatments (multiple treatments allowed). Column IV reports coefficients for the model specification with county and election fixed effects and population controls for US presidential elections when the deadly tornado strikes less than 12 months before the election and causes five or more fatalities (multiple treatments allowed).

** Significant at the 5% level,
* Significant at the 10% level

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