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Catalyst or coincidence? Assessing Joe Biden's impact on the U.S. economy

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Abstract

Since Joe Biden's inauguration in early 2021, U.S. economic performance has been remarkably strong. But is this success attributable to the election of President Biden and his policy agenda or would the economy have followed a similar trajectory without him? In this paper, we investigate the impact of Joe Biden's presidency on a range of macroeconomic indicators by using the synthetic control method. Benchmarking actual U.S. economic development against our synthetic counterfactual, we show that Biden's overall impact on real GDP, investment and trade has been negligible. However, we find evidence of significant effects on private and government consumption. By the end of 2023, real private consumption was 4.8 percentage points higher than in the counterfactual scenario, while real government consumption was 3.3 percentage points lower.

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1 Introduction

As the 2024 U.S. presidential campaign enters its final stretch, the economic policies of President Joe Biden's administration, often referred to as "Bidenomics", have become a focal point of public debate and analysis. Even with Biden not running for reelection himself, the President's economic record is likely to be a major decision driver later this year, with poll after poll showing that economic issues are among the top priorities for American voters.[1] This heightened interest is particularly significant against a backdrop of divergent economic paths in major global economies. While the U.S. economy has grown substantially since late 2020, other countries have faced various degrees of economic challenges and growth rates (International Monetary Fund, 2024). In this evolving landscape, the Biden administration has not been shy to attribute the United States' strong economic performance to its own policy decisions.[2] Since taking office, the President has issued dozens of executive orders and, together with Congress, passed legislation worth several trillion dollars. Proponents argue that his policies have contributed to robust growth, investment and consumer spending, while critics contend that some positive economic indicators may be more attributable to a natural post-pandemic recovery or underlying economic trends, rather than Biden's policies.

All of this raises a compelling question: Does President Biden genuinely deserve credit for the United States' economic performance since late 2020? To explore this question, we use the synthetic control method (Abadie & Gardeazabal, 2003; Abadie et al., 2010, 2015) and construct a counterfactual that we benchmark the U.S. economy under President Biden against. The key advantage of our approach over a simple analysis of U.S. growth rates since the 2020 presidential election or other econometric techniques such as the difference-in-differences framework lies in the suitability of our comparison unit. Drawing from a pool of 22 potential control countries, we are able to accurately replicate the trajectory of the U.S. economy *prior* to the 2020 election. By then comparing the economic development in the United States *after* the election with that of our counterfactual, we are able to uncover the effect Joe Biden has had on the U.S. economy to date. The

[1]See surveys from Newsweek (July 2023), NPR (February 2024) or The Economist/YouGov (June 2024).

[2]See, e.g., Biden (2023): "The economy is growing at a solid clip. [...] The Biden economic plan is working. It's working!" or Biden (2024): "We passed a lot of really good legislation. We knew it was going to take time [...], but it's taken hold now in turning the economy around."

identification relies on the assumption that our counterfactual scenario reflects how the U.S. economy would have performed if Biden had not been in office.

We find that President Biden's impact on the U.S. economy is largely confined to two macroeconomic indicators – private and government consumption – for which we document sizable gaps between the U.S. and its counterfactual. By the end of 2023, real private consumption was 4.8 percentage points higher than in a non-Biden scenario, while real government consumption was 3.3 percentage points lower. For the rest of the variables under study – real GDP, investment and ex- and imports – we find no significant effects. To conduct inference and establish causality, we perform an end-of-sample instability test as well as in-time and in-space placebo tests. Additionally, we carry out a series of robustness tests and find that the effects may lie in a range between 3.9 and 6.5 percentage points for private and -3.3 and -1.3 percentage points for government consumption.

Two caveats bear mentioning. First, the potential presence of small, negative spillover effects implies that our estimates should be interpreted as upper bounds of President Biden's macroeconomic impact. Second, our analysis might not capture the full "Biden effect" since many of his policy provisions are enacted in a gradual way and may take a longer time to materialize.

Do our findings come as a surprise? Perhaps. In August 2022, Congress passed the *Inflation Reduction Act* (IRA), which introduced substantial economic incentives, including tax credits for renewable energy projects and rebates for energy-efficient home improvements. Most observers agree that the IRA provided a boost to output growth, both in the short and in the long run (Bistline et al., 2023). In addition, the *Infrastructure Investment and Jobs Act* of 2021 allocated substantial funds for upgrading the nation's infrastructure, which is likely to yield significant economic benefits. This being said, our paper is mute on the distinct effects of specific items of Biden's policy agenda. Instead, we evaluate the overall economic impact of Biden's administration during his first three years in office.

The remainder of this paper is organized as follows. In the next section, we provide some background information on the 2020 U.S. presidential election and Joe Biden's policy agenda. Section 3 gives a brief overview of the relevant economic literature to date. We lay out our methodological framework, the synthetic control method, and describe how we use it to measure the macroeconomic effects of the election of Joe Biden in Section 4. Section 5 presents our findings, which we then discuss in the subsequent section. A final section concludes.

2 Background

On November 3rd, 2020, Joseph R. Biden was elected as the United States' 46th president, defeating incumbent Donald J. Trump in a closely watched and highly contested election. Biden, a Democrat and former Vice President under Barack Obama, secured 306 electoral votes to Trump's 232, while also winning the popular vote by over 7 million votes.

The peculiarities of the American electoral system notwithstanding, Biden was the frontrunner leading up to the election. Looking at the aggregated betting odds from RealClearPolitics in Figure 1, it is evident that markets clearly favored Biden over Trump as the election approached.[3] In the early months of 2020, the betting odds for both candidates were relatively close, with neither candidate maintaining a decisive lead. However, as the year progressed, Biden's odds began to improve significantly. By mid-year, his betting average had surpassed Trump's, and this lead continued to widen in the months leading up to the election. These betting odds were corroborated by various polling aggregators, which also indicated a steady lead for Biden. For instance, FiveThirtyEight, one of America's leading pollsters, consistently showed Biden with a significant advantage, projecting a win probability of around 90% in the final days before the election (Silver, 2020; Panagopoulos, 2021).

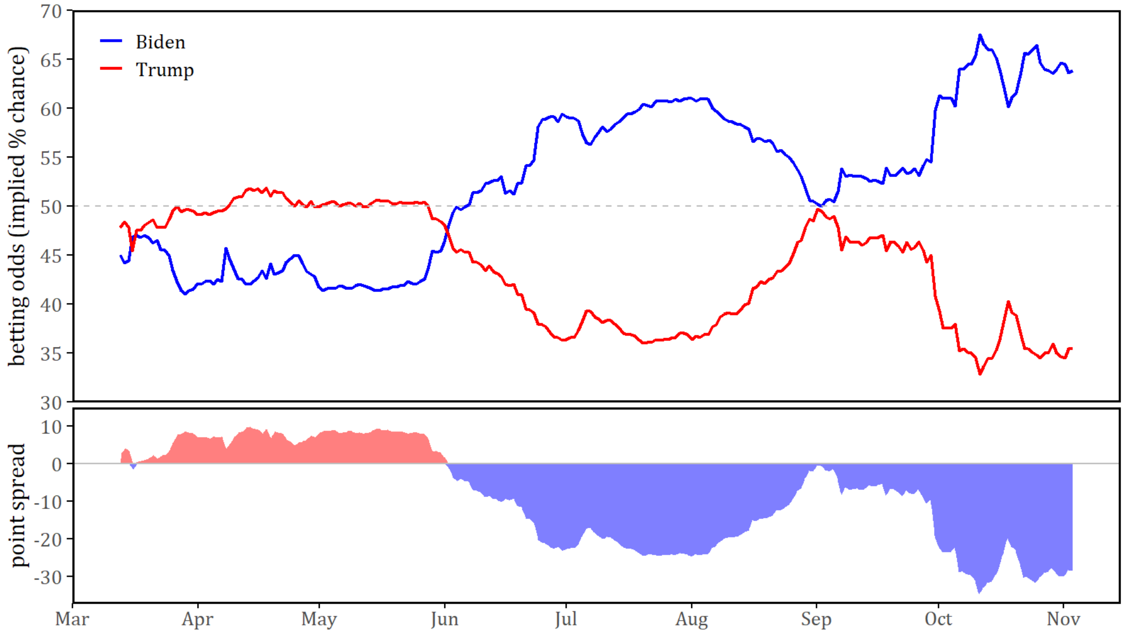
Upon taking office on January 20th, 2021, President Biden moved swiftly to implement his policy agenda through a series of executive orders and actions.[4] Together with the Democratic Party, he has enacted several significant pieces of legislation with potential implications for economic growth. Chief among these is the *Inflation Reduction Act* of 2022, which represents the largest federal response to climate change to date. The IRA includes significant economic incentives and subsidies such as tax credits for renewable energy projects, rebates for energy-efficient home improvements, and incentives for electric vehicle purchases to promote clean energy adoption and reduce greenhouse gas emissions. Moreover,

[3] Betting markets for U.S. presidential elections date back to 1868 and have historically been seen as very reliable predictors of election outcomes. Until 2016, no clear presidential front-runner had lost, with the notable exception of 1948, when the incumbent president, Harry S. Truman unexpectedly defeated his Republican opponent, Thomas E. Dewey (Rhode & Strumpf, 2004, 2013). In 2016, despite being considered the clear favorite by betting markets with implied odds of approximately 78%, Hillary Clinton ultimately lost the electoral college to Donald Trump (Graefe, 2017).

[4] In his first 100 days alone, Biden signed over 60 executive actions, with 24 of them directly reversing Trump administration policies (Kumar, 2021).

the *Infrastructure Investment and Jobs Act* (2021) allocated substantial funds for upgrading the nation's infrastructure and the *CHIPS and Science Act* (2022) was passed to strengthen domestic semiconductor manufacturing and research. Most of these policies were primarily inward-oriented by aiming to boost domestic manufacturing, create jobs, reduce dependence on foreign supply chains as well as invest in American infrastructure and technology (Bistline et al., 2023).

Figure 1: Betting odds 2020 U.S. presidential election



Source: RealClearPolitics (RCP). Time series show RCP betting averages between 13/03/2020 and 03/11/2020.

3 Literature

The general question underpinning our paper and much of the literature described in this section – whether politicians, administrations or governments make a difference to the economy after taking office – has a long tradition in economic research. Early models such as Downs (1957) or Lindbeck and Weibull (1986) posit that politicians and specific policies have negligible effects on economic growth and outcomes. These theories suggest that political actions are primarily driven by electoral incentives or distributional preferences and typically do not have substantive economic impacts. This perspective shifted, however, with the development of citizen-candidate models in the 1990s (Osborne & Slivinski, 1996; Besley & Coate, 1997), which argue that the characteristics and preferences of political candidates significantly influence policy outcomes. Marking a departure from earlier views, these models highlight the importance of individual politicians in shaping economic policies. In a similar vein, Alesina (1988) explores the interplay between macroeconomics and politics, predicting analogous equilibrium outcomes to Osborne and Slivinski (1996) or Besley and Coate (1997). Collectively, these works underscore a growing recognition in economic theory that political actors and their policy preferences are integral to understanding economic outcomes.

First empirical work on whether politicians or parties in power matter for policy outcomes can be found in the cross-sectional studies of Cameron (1978), Cusack et al. (1989) or Blais et al. (1993) who all look at liberal democracies and show that, over time, leftist governing parties increase public spending and employment more than parties on the right. In contrast, the evidence from longitudinal studies of the local U.S. context is somewhat mixed. While Ferreira and Gyourko (2009) as well as Gerber and Hopkins (2011) find no impact of mayoral partisanship on local public spending, a number of more recent studies suggests that partisanship does affect municipal fiscal (de Benedictis-Kessner & Warshaw, 2016) or housing policy (de Benedictis-Kessner et al., 2024).[5]

In broader analyses of the impact of U.S. presidents on the economy, Comiskey and Marsh (2012) as well as Blinder and Watson (2016) find that Democratic presidents have historically performed better than their Republican counterparts; a finding which Blinder and Watson (2016) attribute mostly to outside factors such as oil shocks rather than individual actions or policy measures.[6] Looking beyond tangible policy actions and the effects of presidential terms, Wood et al. (2005) show that presidential statements alone can significantly influence consumer sentiment, leading them to dub the U.S. president the "rhetorical leader of the economy" (Wood et al., 2005, p. 627).[7]

[5]For studies looking beyond the U.S. context, see Pettersson-Lidbom (2008), Solé-Ollé and Viladecans-Marsal (2013), Folke (2014) or Freier and Odendahl (2015). Apart from partisanship, there also exists an extensive literature on the policy impact of politician characteristics such as their occupational background (Hyytinen et al., 2018; Geys et al., 2024), gender (Chattopadhyay & Duflo, 2004; Clots-Figueras, 2012) or ethnicity (Pande, 2003; Dunning & Nilekani, 2013).

[6]Early descriptive evidence that Democratic presidents typically oversee better economic phases can be found in Hibbs (1977), Alesina and Sachs (1988), Hibbs and Hibbs Jr (1989), Alesina and Rosenthal (1995) and Bartels (2008).

[7]There is a related and growing literature on the immediate macroeconomic and financial consequences of news or expected policy changes (see, e.g., Barsky & Sims, 2011; Schmitt-Grohé & Uribe, 2012; Benton & Philips, 2020), which Born et al.'s (2019) study on the economic costs of Brexit (see below) also ties into.

Our paper most directly connects to two recent studies conducted by Born et al. (2019, 2021) that look at the macroeconomic consequences of the 2016 Brexit referendum and the election of Donald Trump, respectively. Employing the same econometric technique as we do below, Born et al. (2019) find a statistically significant, negative effect of Brexit on the U.K. economy. In particular, the authors document a U.K. output loss of 1.7-2.5% two and a half years post-referendum with private consumption and investment largely responsible for the downturn. On the contrary, Born et al. (2021) find no effects of the election of Donald Trump on a broad range of U.S. economic indicators, leading them to conclude that "the impact of Donald Trump on the macroeconomic performance of the U.S. economy has been negligible" (p. 590). More broadly, our analysis relates to the strand of literature looking at the macroeconomic effects of shocks, such as Billmeier and Nannicini (2013) or Funke et al. (2023) who examine the macro-level consequences of economic liberalization and populism.

Finally, our paper stands in the tradition of a broader literature in political science on the individual effects of leaders. Beginning with Carlyle (1841), the *Great Man* theory posits that history is largely shaped by the impact of certain individual decision-makers.[8] In economics, the modern leaders-and-growth literature starts with Jones and Olken (2005). Using random deaths in office as an exogenous shock, they investigate the impact of political leaders on growth and find that leaders matter, particularly in regimes with few constraints on power such as autocracies. Similarly, Yao and Zhang (2015) and Easterly and Pennings (2020) study the growth effects of different leaders at the national and sub-national level with conflicting findings.[9]

We contribute to this literature by extending the current literature on individual leader effects and by providing timely and rigorous evidence on the current U.S. administration. In doing so, we also add to the ongoing debate on whether presidents significantly influence economic performance. To our knowledge, this paper is the first to specifically examine the economic impacts of President Joe Biden's administration and thus fills an important gap in understanding the economic track record of his administration.

[8]For recent discussions of the *Great Man* theory in the realm of political science and leadership, see Spector (2016) or Mouton (2019).

[9]Looking at 650 leaders around the world in both democratic and non-democratic regimes, Easterly and Pennings (2020) find that only a select number of leaders have non-zero effects on growth. Yao and Zhang (2015), on the other hand, present findings by which individual leaders matter for economic growth at the local level in China.

4 Methodology

As Easterly and Pennings (2020) astutely point out, research in the leaders-and-growth field requires the creation of an appropriate benchmark rather than "giving leaders credit for the raw growth average during their tenures" (p. 24) in order to rule out confounding factors. Applied to our context, the key challenge thus lies in the selection of an appropriate control unit which can serve as a credible benchmark for the U.S. economy. The classic difference-in-differences approach, for example, overcomes this problem by choosing a single control unit or a group of control units as a basis for comparison (see, e.g., Card, 1990, or Card & Krueger, 1994). However, since there conceivably exists no single country that approximates the U.S. economy well enough to constitute a suitable control unit, we turn to what Athey and Imbens (2017) describe as "arguably the most important innovation in the policy evaluation literature in the last 15 years" (p. 9): the synthetic control method.

4.1 Synthetic control method

Pioneered by Abadie and Gardeazabal (2003) in their study of the effects of terrorism on the Basque economy, the synthetic control method formally builds on the difference-in-differences framework. After its initial use, the method has been used in an increasing number of studies, including the evaluation of effects of a tobacco control program in California (Abadie et al., 2010), German reunification (Abadie et al., 2015), an educational program in a New York school district (Bifulco et al., 2017), the Brexit referendum (Born et al., 2019) and the Trump presidency (Born et al., 2021).

The basic idea of the synthetic control method is to find a weighted combination of potential control units that accurately replicates the pre-treatment development of the variable of interest for the experimental unit.[10] Using relevant characteristics and past data points, the units that comprise the synthetic control unit are selected by an algorithm based on their pre-treatment similarity to the treated unit. Under the assumption that, absent treatment, the experimental unit would have developed in the same way as the synthetic control, the framework allows us to identify the causal effect by comparing the synthetic counterfactual

[10] In the remainder of this paper, we will use the terms "experiment", "treatment", "event", "shock" and "intervention" interchangeably.

to the actual development of the treated unit post-treatment. The method's key strength is the data-driven and transparent estimation of a counterfactual for the treated unit, which is represented by a linear combination of untreated units.

The algorithm underpinning the synthetic control method can be formally described as follows: Let $J+1$ be the group of units for which observations regarding the variable of interest, Y_{jt}, are available. Here, $j = 1$ is the treated unit, i.e., the unit exposed to the event under study, and $j = 2$ to $j = J+1$ are all units that constitute the pool of potential control countries (the "donor pool"). Observations for all units are available for the time periods $t = 1, \ldots, T$, with the treatment occurring at time T_0. For unit j and time period t, we will define Y_{jt}^N to be the development of the variable of interest in the absence of treatment. Letting $t > T_0$, we analogously notate Y_{1t}^I as the response of the treated unit after the shock. Now, we can describe the treatment effect in period t ($t > T_0$) as

$$\tau_{1t} = Y_{1t}^I - Y_{1t}^N \tag{1}$$

Note that the values for Y_{1t}^I are easily obtained since these are the treated unit's time series values of the variable of interest. For Y_{1t}^N – the hypothetical outcome of the treated unit – the synthetic control can be defined as a weighted average of the control units in the donor pool. Denoting the weights as the $J \times 1$ vector $w = \left(w_2, \ldots, w_{J+1}\right)'$, the synthetic control estimators are

$$\hat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt}, \tag{2}$$

$$\hat{\tau}_{1t} = Y_{1t} - \hat{Y}_{1t}^N \tag{3}$$

Looking at equation 2, the weighting of each potential control unit from the donor pool is given by a scalar w_j ($j = 2, \ldots, J+1$) which simultaneously defines the composition of the synthetic control. Additionally, the weights must be non-negative and sum up to one, meaning $0 \leq w_j \leq 1$ and $w_2 + \ldots + w_{J+1} = 1$.[11] With reference to equation 3, notice that $\hat{\tau}_{1t}$ needs to be close to zero during the

[11] These restrictions prevent extrapolation, wherein the algorithm produces a perfect fit through choosing weights w_j outside of the defined $[0,1]$ interval. For a more detailed discussion of the consequences of extrapolation as a means to construct counterfactuals, see King and Zeng (2006). To prevent interpolation, Abadie et al. (2015) suggest restricting the donor pool to units similar to the treated unit or potentially adding penalty terms to the objective function to account for the differences between experimental and donor units.

pre-treatment period since the path of $\hat{Y}_{1t}^{N}$ should closely mimic that of Y_{1t}. Put differently, our synthetic control unit should behave like a doppelganger of the treated unit and replicate its trajectory *before* the shock. Provided that the methodical assumptions laid out in Section 4.2 are met, any divergence of $\hat{Y}_{1t}^{N}$ from the path of Y_{1t} *after* the occurrence of the event can then be interpreted as the effect of the event and is commonly referred to as the "doppelganger gap" (Born et al., 2019, p. 2724).

To replicate the development of the experimental unit as precisely as possible, the synthetic control method involves the integration of so-called predictors. Predictors can be understood as auxiliary variables that contribute to the explanation of the development of the outcome variable of interest and thus aim to increase the accuracy of the approximation. For example, Abadie and Gardeazabal (2003) use predictors known from growth theory, such as human capital endowment, population density, or the investment rate of a country to approximate GDP growth. For k such predictors, let x_1 describe the $k \times 1$ vector containing the observations for the selected predictors of the treated unit. Analogously, X_0 is defined as the $k \times J$ matrix of predictor values for the J untreated units in the donor pool. Moreover, let V be a non-negative, diagonal matrix that denotes the relative weighting of the predictors, i.e., the relative importance of the variables in x_1 and X_0.

The term $x_1 - X_0 w$ describes the difference between the characteristics of the experimental unit and the synthetic control. The optimal weighting of the control units, w^*, is now chosen so that the mean square deviation is minimized, subject to the non-negativity and unity conditions on weights:

$$(x_1 - X_0 w)' \, V \, (x_1 - X_0 w) \quad \text{s.t.} \quad w_2, \ldots, w_{J+1} \geq 0 \quad \text{and} \quad w_2 + \ldots + w_{J+1} = 1 \tag{4}$$

Note that, since w^* depends on V, the choice of the relative importance of predictors directly influences the composition of the synthetic control unit. We follow the approach of Abadie and Gardeazabal (2003) and Abadie et al. (2010) and select V to minimize the mean squared prediction error of both the outcome variable and covariates for the pre-treatment period.[12]

[12] We select V^* so that the doppelganger tracks the observed outcome variable of the experimental unit best: $w^*(V^*)$. Letting z_1 be a $k \times 1$ vector containing the observed outcome variable for the experimental unit and Z_0 a $k \times J$ matrix containing the values of the same variable for the units of the donor pool, the optimal V is then given by the solution of the arg min function $(z_1 - Z_0 w^*(V))'(z_1 - Z_0 w^*(V))$ and we can normalize the Euclidean Norm of V to one. Abadie et al. (2015, p. 502) show that this approach produces equally precise results as choosing predictor weights through a cross-validation technique using a separate training and validation period.

4.2 Identifying assumption and contextual requirements

Our identifying assumption is that the U.S. economy would have evolved in the same way as its doppelganger, had it not been for the election of Joe Biden. This assumption appears plausible insofar as that (1) the American economy and its doppelganger behaved similarly leading up to the election and that (2) based on economic fundamentals, both had an equal likelihood of experiencing the "treatment" of a Biden election.[13] We verify the first condition in Section 5 below. Concerning the likelihood of treatment, note that the macroeconomic trends in the U.S. and the donor pool economies were similar prior to the election (Chudik et al., 2021) and that the COVID-19-induced economic downturn in the lead-up to the 2020 election is unlikely to have substantially altered the election outcome (Baccini et al., 2021; Mutz, 2021). Therefore, the election of Joe Biden can indeed be perceived as random *in regards to* pre-election macroeconomic developments since economic factors are believed to have been inconsequential for the outcome.

Moreover, the validity of the synthetic control estimator also depends on the *no-anticipation assumption*. Bias may arise if forward-looking economic agents adjust their behavior prior to the intervention, or if specific elements of the intervention are implemented before its official enactment (Abadie, 2021, pp. 409-410). In our context, anticipation effects could play a role since the outcome of the 2020 election was largely expected in the final weeks of the presidential campaign (see Section 2). We discuss the implications this might have for our results in more detail in Section 5.4.2.

Further, special attention needs to be paid to the *selection of the donor pool*. Countries that experience similar shocks to that of the treated unit should be dropped from the donor pool because of their potential to confound the results (Abadie, 2021, p. 409). As briefly discussed above, it is also crucial to restrict the donor pool to countries that exhibit characteristics similar to the affected unit. This restriction serves two important purposes. Firstly, while the synthetic control method inherently prevents extrapolation due to the constraints on weights (see Section 4.1), the risk of interpolation bias remains. This bias can occur when the synthetic control averages out significant discrepancies between the characteristics of the treated unit and those of the donor units and yields a doppelganger

[13]In the synthetic control literature, the first condition is often referred to as the "convex hull condition" (Abadie, 2021, p. 411).

that only superficially matches the treated unit's characteristics.[14] Secondly, in cases where the donor pool is not limited in size or similarity to the treated unit, there is a risk of overfitting. As Abadie et al. (2015) point out, overfitting can occur when the synthetic control method uses idiosyncratic variations from a large sample of unaffected units to replicate the treated unit (Abadie et al., 2015, p. 500). We come back to these considerations when describing our donor pool in the next section.

Another key assumption for unbiased inference is the *non-interference assumption*, which holds that the treatment assignment of one unit has no impact on the control units – there are no spillovers across units (Abadie, 2021, p. 410).[15] If strictly enforced, any control units that could potentially be affected by the treatment should be excluded from the donor pool, raising a potential trade-off between the size of the donor pool and the non-interference assumption. We do not formally test for spillover effects but discuss their potential implications below and in Section 6.

4.3 Model specification and data

To construct our synthetic control for the U.S., we draw on quarterly data from the OECD Economic Outlook database (issue 115, published May 2024). This provides us with a reliable data source for the U.S. and all countries in our donor pool until year-end 2023, the last quarter for which data is available. To reduce the risk of interpolation bias, we deliberately restrict the donor pool to the relatively homogeneous group of OECD countries and only keep countries in our donor pool for which we can obtain contiguous data for our key outcome variable of interest and our chosen predictors. With reference to the previously discussed trade-off between donor pool size and the non-interference assumption, we note that the domestic focus of much of the Biden policies (see Section 2) make the presence of substantial spillover effects rather unlikely. We therefore refrain from restricting our donor pool a priori on the basis of spillover considerations, and instead revert to a qualitative discussion of potential spillover effects

[14]The risk of interpolation bias is likely to be most severe if the relationship between the outcome variable of interest and its predictors is particularly strong and highly nonlinear (see Abadie et al. (2010) for an in-depth discussion).

[15]The trained reader will notice that this, of course, is the stable unit treatment value assumption first put forward by Rubin (1980).

in Section 6. This approach leaves us with a set of 22 countries in our donor pool.[16]

In our baseline specification, we choose real gross domestic product (GDP) as our key outcome variable of interest and normalize the data to the end of the pre-treatment period. We assume the treatment occurs in 2020Q4, the quarter of the Biden election, and define our pre-treatment period as the ten-year window prior to the election, giving us $T = 96$ quarterly observations with $T_0 = 84$. In three alternative specifications, we allow for longer pre-treatment periods starting in 2000Q1 and 2005Q1 and, separately, reassign the treatment date to the inauguration of President Biden in 2021Q1.[17]

As described in Section 4.1, the doppelganger is a weighted average of donor pool countries. The donor countries and their respective weights are chosen in a way that minimizes the pre-treatment distance between U.S. real GDP and that of the doppelganger and also matches the pre-treatment averages of various covariates. In selecting these covariates, we follow Born et al. (2019, 2021) and use private consumption, investment, exports and imports as shares of GDP as well as labor productivity growth and the employment share, such that $K = 6$. Labor productivity growth is defined as the logarithmic difference between quarterly real GDP and quarterly total employment, while the employment share is the ratio between total employment and the working age population.[18]

Using the standard GDP expenditure approach of $Y = C + I + G + X - M$ (where Y describes GDP, C stands for consumption, I denotes investment, G signifies government consumption, X represents exports and M indicates imports), we also decompose GDP and estimate five separate doppelgangers; one for each GDP component. The estimations follow the same exact procedure as described above.

[16]Reassuringly, our donor pool is very similar to those of Born et al. (2019, 2021) who conduct related exercises.

[17]Regarding the dating of treatment, a practical objection would be that both the election as well as the inauguration are "incorrect" treatment dates in so far as the "true" treatment only took place with the passage of, say, the *Inflation Reduction Act* in 2022Q3. Importantly, the synthetic control method alleviates any potential concerns coming from defining the treatment date prematurely. Equations 2 and 3 make clear that the synthetic control method does not limit the time variation in the effect of the intervention, meaning that erroneously backdating the treatment does *not* bias the estimated treatment effect even if pre-treatment periods are captured as post-treatment periods (Abadie, 2021, p. 410).

[18]A summary of all the data used in this paper can be found in the appendix.

5 Results

5.1 Aggregate effect

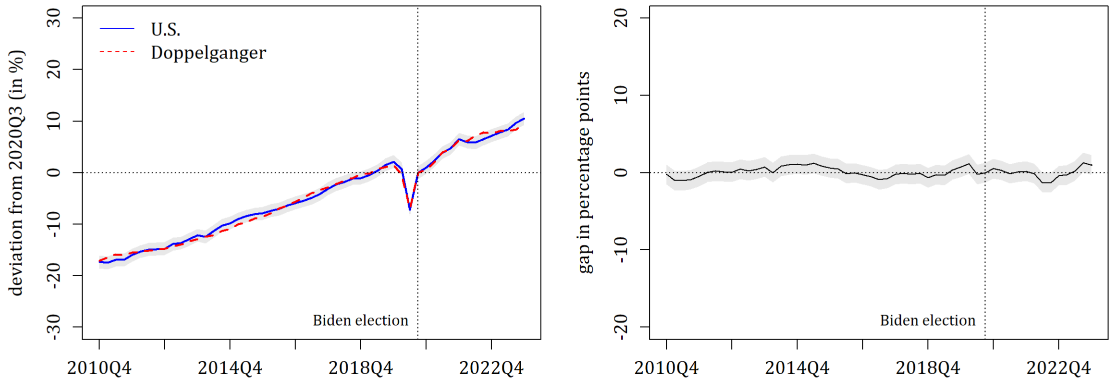
We report the results for our baseline specification in Figure 2. The left panel compares the trajectory of real GDP in the U.S. (blue line) between 2010 and 2023 to that of its doppelganger (red line), while the right panel shows the doppelganger gap. In gray, we plot confidence bands around actual GDP and the doppelganger gap that equal two standard deviations of the pre-election difference between the U.S. and its doppelganger.

Crucially, the GDP paths of the U.S. and its doppelganger track each other closely before the 2020 election: In Figure 2, Panel A, both time series display very similar GDP paths, which translates to the doppelganger gap hovering around zero pre-election (Figure 2, Panel B). Note that during no quarter in the pre-treatment period does the confidence band around the doppelganger gap diverge from zero. In addition to the tight GDP fit, the synthetic control also provides a good match for the six covariates that underpin the optimization procedure (see Table 1). Taken together, these findings suggest that our synthetically created doppelganger represents a useful counterfactual to benchmark the macroeconomic performance under President Biden against. Table 2 shows the five donor countries that make up the synthetic U.S.: Denmark, South Korea and France are the biggest donors, with New Zealand and Ireland contributing, too.[19]

Shifting focus to the post-election periods, we find that the trajectory of real GDP since the election has been quite similar in the U.S. and our counterfactual scenario. In the first four quarters after the election, real GDP developed almost identically in the U.S. and the doppelganger economy. After a small dip at the end of 2021, the U.S. then quickly bounced back and slightly outperformed the doppelganger by the end of 2023. Notably, U.S. GDP has remained within the two-standard-deviation confidence band throughout the whole post-election sample. In other words, although average GDP growth in the U.S. has been strong since the election (see Section 1), this performance is not extraordinary: the U.S. under President Biden has barely outperformed its doppelganger.

[19]While this weight allocation might seem somewhat surprising at first, the purely data-driven approach underlying the synthetic control method means we can refrain from structurally interpreting the results (Abadie, 2021; Born et al., 2021, p. 588). In Section 5.5, we report additional specifications and show that our results are largely robust to alternative donor pool compositions.

Figure 2: Baseline results: U.S. real GDP



(A) Real GDP development **(B)** Doppelganger gap

Note: Actual data (blue line) vs doppelganger (red line). Shaded area are two standard deviations of difference prior to the election.

Table 1: Baseline results: matching of covariates

	U.S.	Doppelganger
Consumption share	67.30	50.00
Investment share	20.30	23.90
Export share	11.90	14.60
Import share	14.60	13.60
Labor productivity growth	0.40	0.30
Employment share	61.60	61.90

Note: All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

Table 2: Baseline results: doppelganger country weights

Australia	< 0.01	Ireland	0.03	Portugal	< 0.01
Austria	< 0.01	Italy	< 0.01	Slovak Republic	< 0.01
Canada	< 0.01	Japan	< 0.01	Spain	< 0.01
Denmark	0.39	Korea	0.30	Sweden	< 0.01
Finland	< 0.01	Luxembourg	< 0.01	Switzerland	< 0.01
France	0.16	Netherlands	< 0.01	United Kingdom	< 0.01
Germany	< 0.01	New Zealand	0.12		
Iceland	< 0.01	Norway	< 0.01		

5.2 GDP decomposition

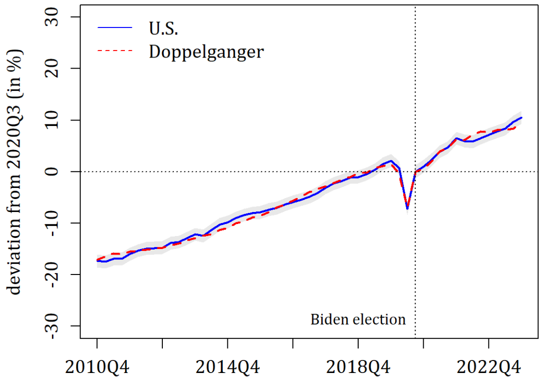
While there seems to be no significant effect of the Biden presidency on aggregate output, it is conceivable that other economic indicators have responded in the wake of the 2020 election. To shed some light on alternative channels of impact, we conduct a simple accounting exercise and decompose GDP into its main components. Following the expenditure approach described in Section 4.3, we look at private and government consumption, ex- and imports as well as investment.

Panel A of Figures 3 and 4 show our baseline GDP results and thus replicate Figure 2. The other panels in Figure 3 compare the actual trajectories of the five decomposition variables to their doppelgangers, with Figure 4 plotting the resulting doppelganger gaps. Reassuringly, the actual and synthetic trajectories for all variables are similar prior to the 2020 election. For ex- and imports, the pre-treatment fit is less tight but still within the two-standard deviation confidence band. We report the summary statistics for covariate balance and donor weighting for all decomposition variables jointly in the appendix (see Table A2).

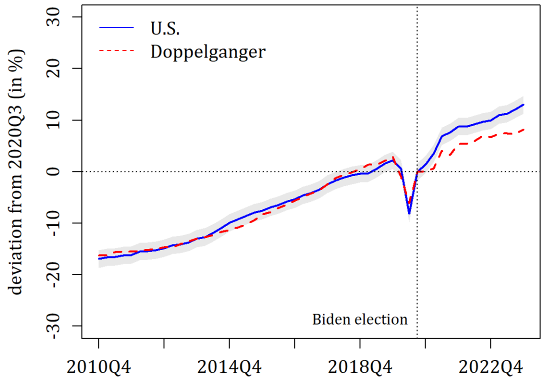
Figure 4, Panel B shows an immediate uptick in private consumption after the 2020 election relative to the doppelganger economy. The effect levels off after four quarters but persists until the end of sample, with real private consumption being around 4.8 percentage points higher in the U.S. than in the synthetic control by the end of 2023. Interestingly, the trend for government consumption exhibits a somewhat inverted pattern. After a quick, relative decline following the election, it stabilizes and rebounds slightly. Three years post-election, real government consumption is 3.3 percentage points lower when compared to the counterfactual (Figure 4, Panel C). For both variables, the effect is large enough to be more than two standard deviations away from zero, providing suggestive evidence for significance.

For investment, the post-election effect is less clear. Figure 4, Panel D shows that the doppelganger gap oscillates between positive and negative values before ultimately jumping up at the end of 2023. Lastly, Panels E and F indicate that trade seems to have decreased following the election of President Biden, although the less accurate pre-treatment fit means that U.S. ex- and imports are less than two standard deviations away from their doppelganger values for all but one post-election period.

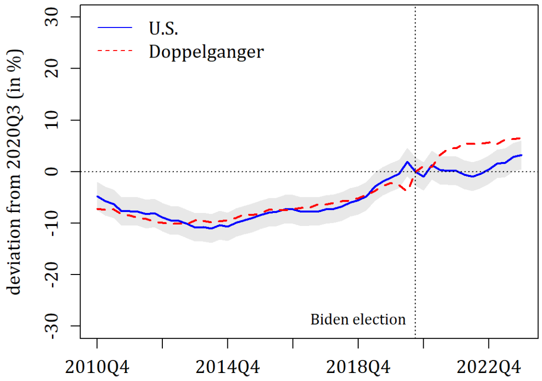
Figure 3: Decomposition results: U.S. macroeconomic trajectories



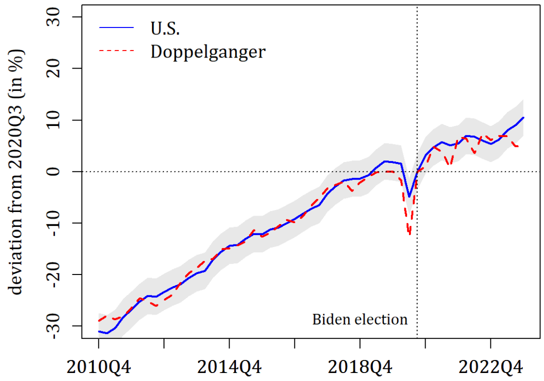
(A) Real GDP



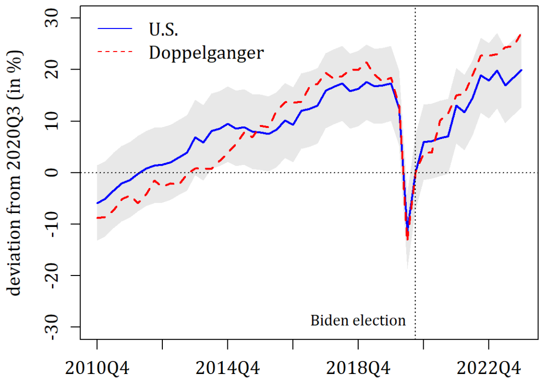
(B) Real private consumption



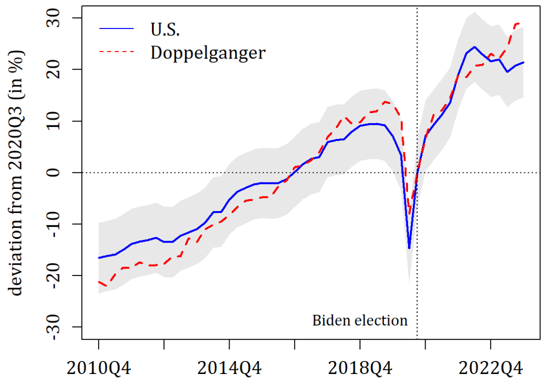
(C) Real government consumption



(D) Real investment



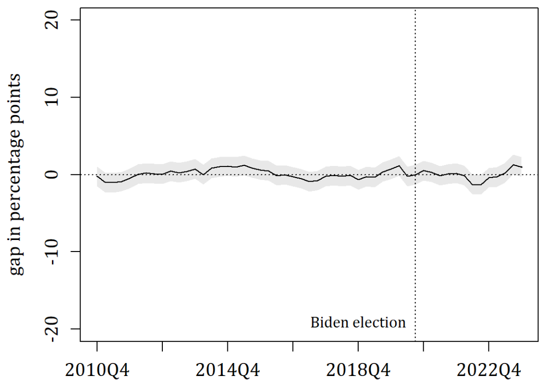
(E) Real exports



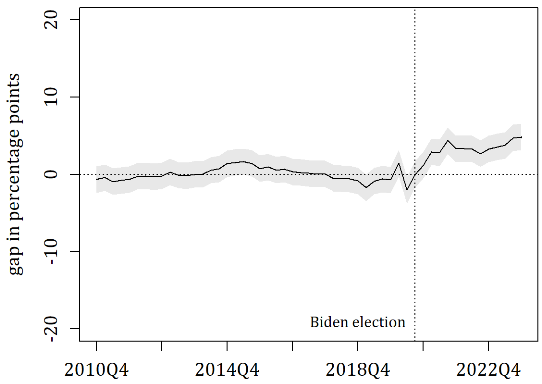
(F) Real imports

Note: Actual data (blue line) vs doppelganger (red line). Shaded area are two standard deviations of difference prior to the election.

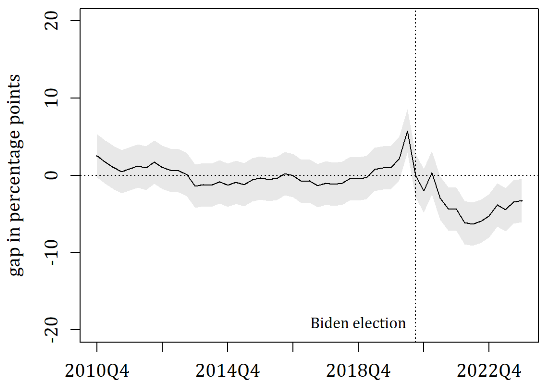
Figure 4: Decomposition results: doppelganger gaps



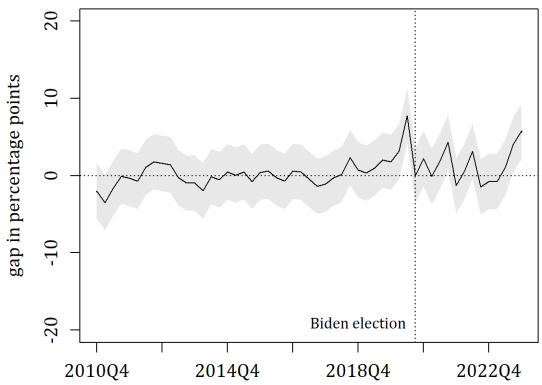
(A) Real GDP



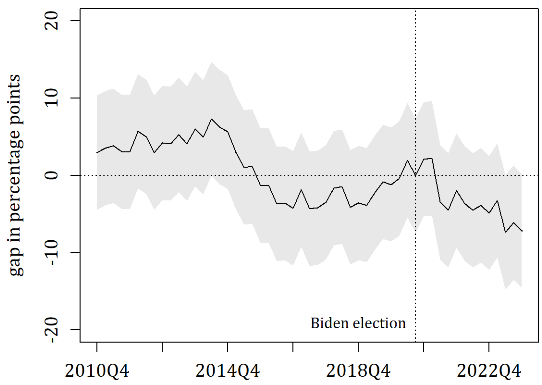
(B) Real private consumption



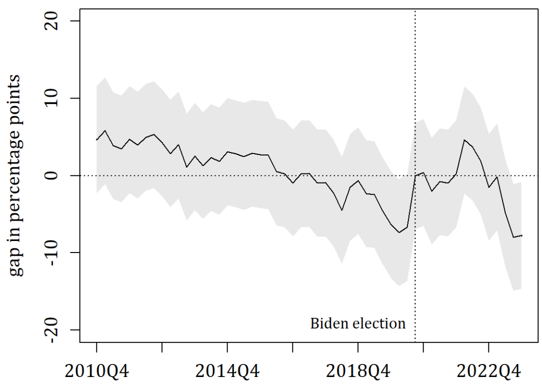
(C) Real government consumption



(D) Real investment



(E) Real exports



(F) Real imports

Note: Shaded area are two standard deviations of difference prior to the election.

5.3 Inference

In this section, we make use of a recent addition to the synthetic control inference toolkit by Hahn and Shi (2017). In particular, we perform Andrews' (2003) end-of-sample instability test to conduct inference for our estimates. We start by noting that the shaded areas around the U.S. trajectories of the key economic variables shown in Figures 2, 3 and 4 represent two-standard-deviation confidence intervals of the doppelganger gap before the 2020 presidential election. As such, they serve as an indicator of the model's fit during that period. At least for two variables (private and government consumption), our results show a post-election divergence of the doppelganger from the actual trajectory of the U.S. that surpasses these bounds significantly. Although this divergence indicates that the deviation is both substantial as well as unusual when compared to the pre-Biden period, these bounds do not constitute a formal test of significance.

The end-of-sample instability test measures whether the post-election doppelganger gap and all pre-election gaps of the same length could stem from the same distribution. More formally, we follow Andrews (2003) and derive the test statistic using a sub-sampling scheme by which we run the synthetic control method over all time periods and then compute the test statistic using the sum-of-squares of the post-treatment errors.[20] We perform this matching procedure on the sample $1, \ldots, T_0$, while successively omitting observations $j, \ldots, j + [m/2] - 1$. Here, m describes the number of post-election quarters, T_0 the time of the treatment, the resampling is done for $j = 1, \ldots, T_0 - m + 1$ and, for each iteration, the test statistic is based on the matching errors from j to $j + m - 1$.

We report the results of this exercise in Table 3 below. For real GDP, investment, exports and imports, we cannot reject the hypothesis of equal performance of the U.S. economy and its doppelganger. For real private and government consumption, however, the test statistics indicate that the effects of the 2020 Biden election are indeed statistically significant, complementing the earlier evidence from our confidence bands. Between these two inference exercises, we can now credibly say that the election of Joe Biden did not have any significant effects on real GDP, investment or trade. Conversely, our results indicate that there is indeed evidence for a "Biden effect" on real private and government consumption.

[20] As highlighted by Ferman and Pinto (2019), Andrews' test – although originally based on stationary data – is asymptotically valid under stationary errors.

Table 3: End-of-sample instability test

Specification	p-value
Real GDP	0.238
Real private consumption	0.024
Real government consumption	0.023
Real investment	0.262
Real exports	0.167
Real imports	0.190

Note: Reported p-values refer to the null hypothesis that the post-election doppelganger gap and all pre-election gaps of the same length derive from the same distribution.

5.4 Causality

5.4.1 In-space placebo test

Following Abadie and Gardeazabal (2003), Bertrand et al. (2004) and Abadie et al. (2010), we run a series of placebo tests in order to judge whether the effects documented above are causal. For the in-space placebo test in this section, we successively apply the synthetic control method to all countries in our donor pool, that is, we re-run the method 22 times, each time classifying one of the donor pool countries as "treated". For each of these iterations, we assign the treatment (the 2020 presidential election) to one of the donor pool countries and thus act as if one of the donor pool countries had experienced the election of Joe Biden.

If the results indicate that the doppelganger gaps reported in Sections 5.1 and 5.2 are large compared to the doppelganger gaps for our donor pool countries, i.e., those countries that did not receive a treatment in the form of the election of Joe Biden, this would provide evidence for a "Biden effect". On the contrary, if the placebo studies reveal that the U.S. post-treatment doppelganger gaps in Sections 5.1 and 5.2 are *not* unusual when checked against the doppelganger gaps for the donor pool countries, we can conclude that our analysis does not provide evidence of a "Biden effect". Simply put, our confidence in the causality of the estimates presented above is low (high) if we can (cannot) observe similarly large estimates when the treatment is assigned to countries that did not experience it.

Turning to Figure 5, the black lines show the original U.S. doppelganger gaps estimated in Sections 5.1 and 5.2. In gray, we plot the placebo estimates for our donor pool countries. We find that the doppelganger gaps for real GDP, investment, exports and imports are not at all unusual when compared with the respective gaps for the donor countries. For real private and government consumption (Panels 5B and 5C), the U.S. doppelganger gaps are large and relatively unusual, suggesting that the 2020 presidential election of Joe Biden did indeed cause significant consumption effects. In results shown in the appendix, we also calculate two measures of the relative pre- and post-treatment fit for the U.S. and the donor countries – the relative root mean squared prediction error (RMSPE) and the maximum absolute prediction error (MAPE) – as alternative ways of illustrating our in-space placebo results. As before, we use T for the sample size and denote the period of treatment, i.e. the 2020 presidential election, as T_0. Defining

$$RMSPE_{pre} = \sqrt{\frac{1}{T_0 - 1} \sum_{t=1}^{T_0 - 1} (x_{1,t} - x_{0,t} w)^2},$$

$$RMSPE_{post} = \sqrt{\frac{1}{T - T_0 - 1} \sum_{t=T_0}^{T} \left(x_{1,t} - x_{0,t} w - x_{1,T_0} + x_{0,T_0} w\right)^2},$$

$$MAPE_{pre} = \max |x_{1,t} - x_{0,t} w|, \quad t \epsilon [1, T_0 - 1],$$

$$MAPE_{post} = \max |x_{1,t} - x_{0,t} w - x_{1,T_0} + x_{0,T_0} w|, \quad t \epsilon [T_0, T],$$

we then compute the relative root mean squared prediction error as

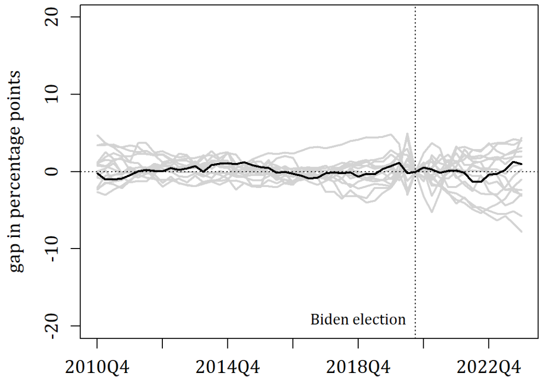
$$\rho_1 = RMSPE_{post} / RMSPE_{pre} \tag{5}$$

and the maximum absolute prediction error as

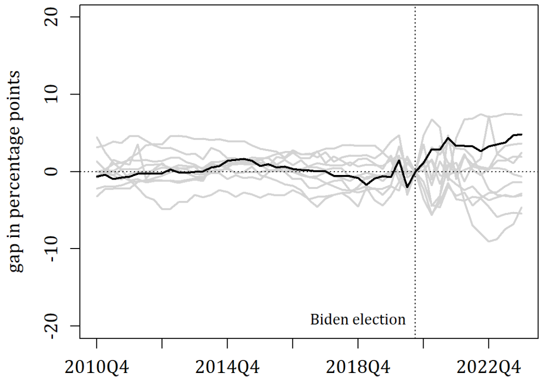
$$\rho_2 = MAPE_{post} / MAPE_{pre}. \tag{6}$$

Figures A1 and A2 display the RMSPE and MAPE ratios ρ_1 and ρ_2 and corroborate our findings from this section, showing that the U.S. stands out with large relative post-treatment doppelganger gaps for private and government consumption only.

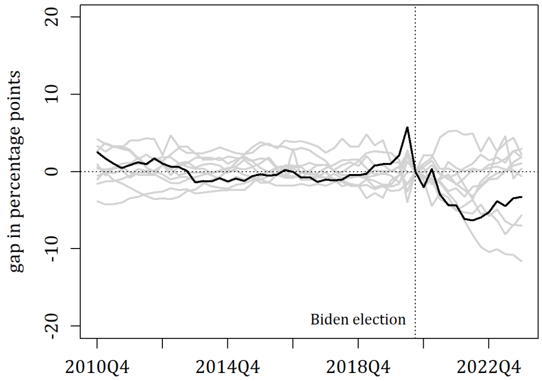
Figure 5: In-space placebo tests



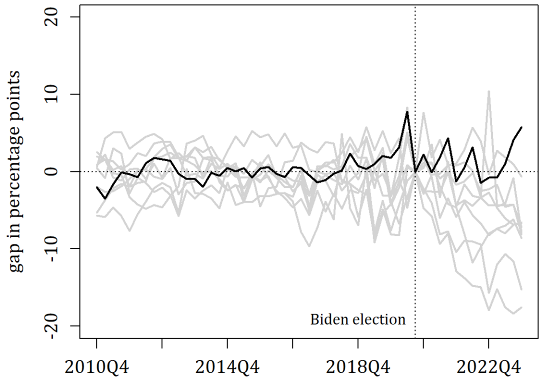
(A) Real GDP



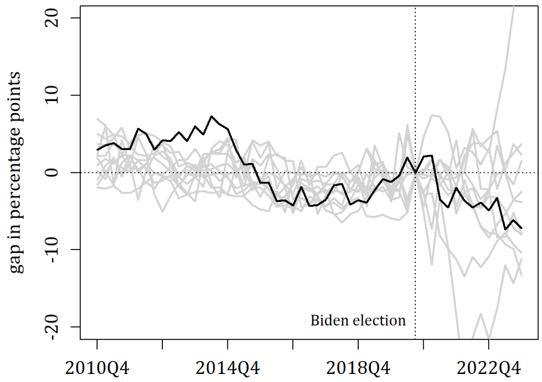
(B) Real private consumption



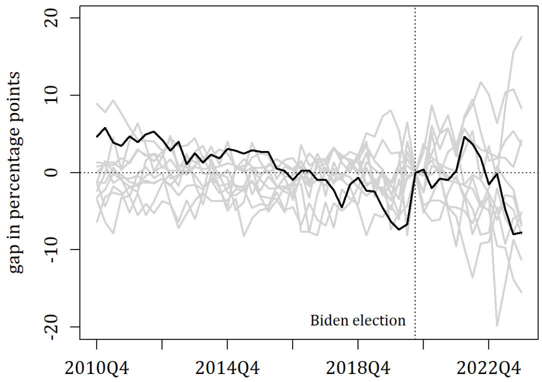
(C) Real government consumption



(D) Real investment



(E) Real exports



(F) Real imports

Note: U.S. doppelganger gap (black line), with gray lines representing country placebo doppelganger gaps estimated by considering fictitious Biden election in all donor pool economies.

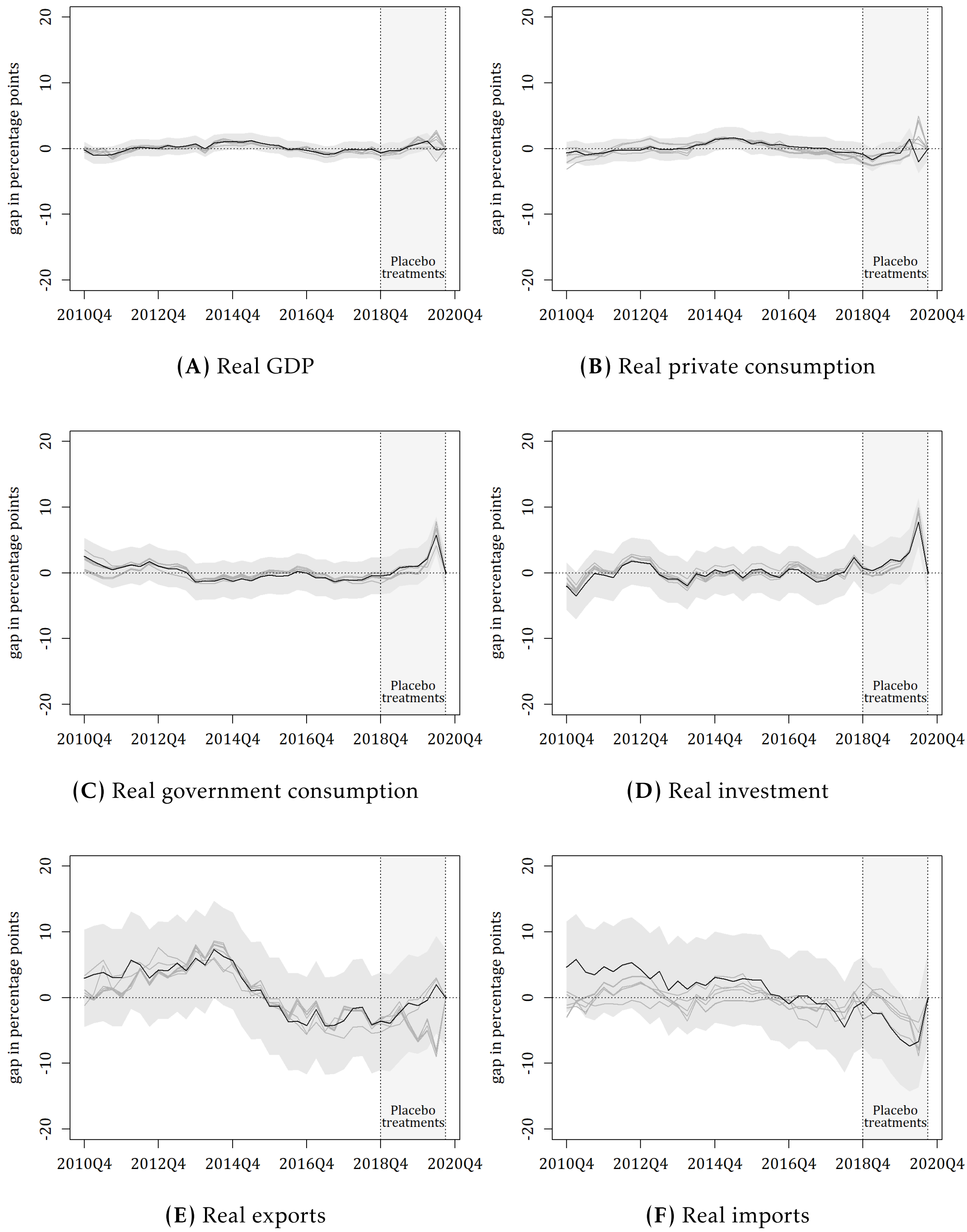
5.4.2 In-time placebo test

Building on Heckman and Hotz's (1989) "pre-program tests", Abadie et al. (2015) suggest in-time placebos as a second way to evaluate the credibility of synthetic control estimates. Instead of reassigning the treatment across space, this test artificially moves back the treatment to a period before the treatment actually took place. Following the same logic as above, if the placebo estimate is large, that is, if a doppelganger gap opens up after the placebo treatment date, this would reduce our confidence that the effects shown in Sections 5.1 and 5.2 are a result of the election of Joe Biden.[21] Conversely, if we find no effect for the placebo treatment date, this would suggest that the estimated effects in Sections 5.1 and 5.2 are indeed attributable to the 2020 presidential election – and not purely driven by a lack of predictive power.

Figure 6 reports the results of our in-time placebo study, for which we separately backdate the treatment to all quarters between 2018Q4 and the actual election in 2020Q4. Each gray line represents the doppelganger gaps obtained for one of these in-time placebo tests, while the black lines show the resulting doppelganger gaps for our baseline specifications outlined in Sections 5.1 and 5.2. Naturally, the placebo doppelganger will (slightly) differ from our baseline doppelganger as we restrict the pre-treatment period and therefore the information used to compute the counterfactuals. Reassuringly, however, the doppelganger gaps produced by the placebo studies lie, with few exceptions, within the confidence bands, closely tracking our baseline doppelganger gaps. These findings broadly confirm the results reported in Sections 5.1 and 5.2 and their predictive power.

[21] Of course, in-time placebo tests are only feasible if there are enough pre-treatment periods where no other structural shocks occurred (Abadie et al., 2015, p. 499). The 2016 presidential election of Donald Trump, for example, would likely be a less than ideal time period to reassign our treatment to, given its potential to represent a significant structural shock to the economy.

Figure 6: In-time placebo tests



(A) Real GDP

(B) Real private consumption

(C) Real government consumption

(D) Real investment

(E) Real exports

(F) Real imports

Note: U.S. doppelganger gap (black line), with gray lines representing in-time placebo doppelganger gaps estimated by considering fictitious Biden election in all quarters between 2018Q4 and the election in 2020Q4.

As highlighted by Abadie (2021), in-time placebos can and should also be used to test for potential anticipation effects. Given the fact that the election of Joe Biden seemed rather likely in the final weeks before the 2020 presidential election (see Section 2), anticipation effects could have played a role in our context. To address this possibility, we also report the placebo test that backdates the election by one quarter. The results of this individual placebo test, which are displayed separately in Figure A3, show no evidence of anticipation effects for any of the variables under study.

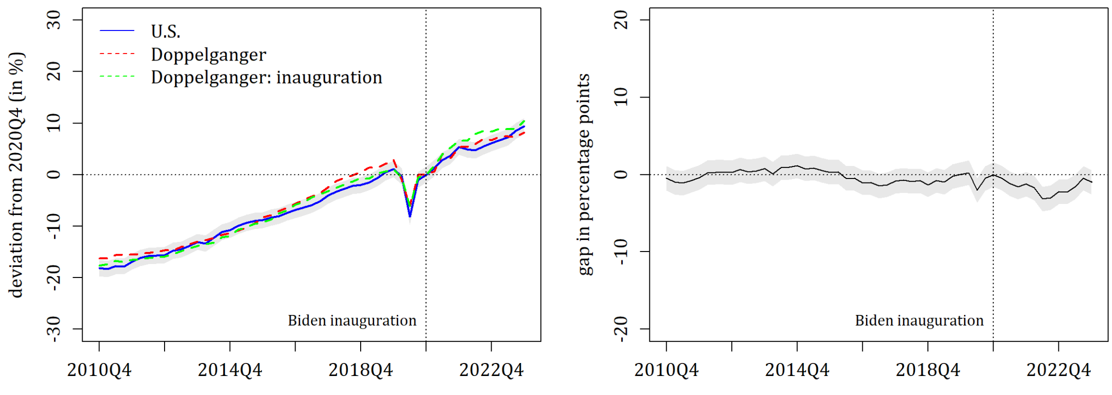
5.5 Robustness

Lastly, we perform a series of tests to check the robustness of our results. The primary objective is to examine whether our findings are sensitive to certain aspects of our baseline specification. We focus on three key points: the dating of the treatment, the composition of the donor pool, and the length of the pre-treatment period. We will discuss each of these aspects in turn below. To preserve readability, this section only documents the results for real GDP, while the robustness tests for the GDP decomposition are reported in Section A.4 of the appendix. If not otherwise noted, the decomposition results are analogous to the findings presented here.

5.5.1 Treatment date

In our baseline specification in Sections 5.1 and 5.2, we use the election of Joe Biden in 2020Q4 as the treatment date. Noting that the inauguration of President Biden only occurred in January 2021, we now consider 2021Q1 as an alternative treatment date. Figure 7, Panel A plots the real GDP path for the U.S. economy, our baseline doppelganger as well as the robustness doppelganger with a treatment date of 2021Q1, whereas Panel B shows the doppelganger gap of the latter. During the pre-treatment period, both doppelganger track the trajectory of U.S. real GDP equally well, meaning that the doppelganger gaps do not expressively diverge from zero. In the post-election period, the real GDP path of the robustness doppelganger leaves the confidence band temporarily but the gap disappears towards the end of our sample. Crucially, the results shown in Figure 7 are similar to the those reported in Section 5.1. In addition, the newly constructed robustness doppelganger provides a similar match for the predictors (see Table 4), indicating that our baseline estimates are generally robust to the choice of treatment date.

Figure 7: Robustness test: treatment date



(A) Real GDP development **(B)** Doppelganger gap

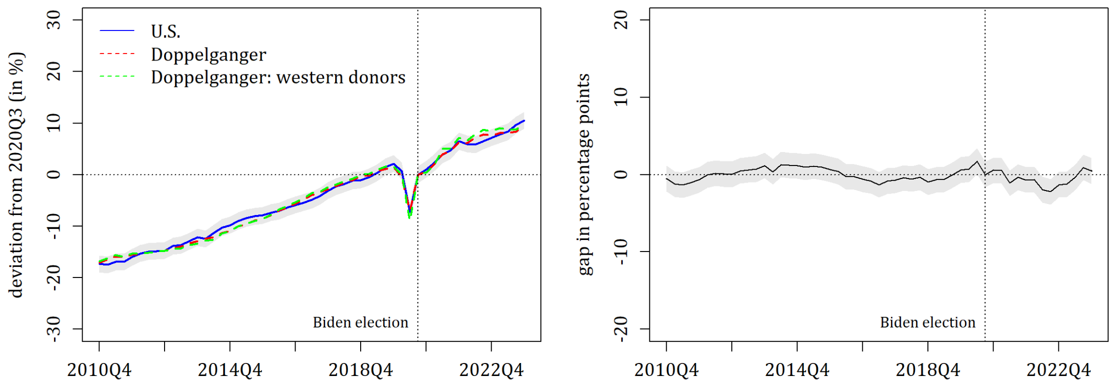
Note: Real GDP is normalized to 2020Q4 as we assume first effects in the quarter of the inauguration 2021Q1. Actual data (blue line), baseline doppelganger (red line), doppelganger based on inauguration treatment (green line). Shaded area are two standard deviations of difference prior to the election.

5.5.2 Donor pool

Coming back to the choice of the donor pool and the related risks of interpolation and overfitting (see Section 4.2), we also test for the sensitivity of our results with regards to the selection of donor pool countries. In this section, we exclude all Eastern and Southern European economies (Italy, Portugal, Slovak Republic, Spain) as well as the Asian countries (Japan and South Korea) from our baseline donor pool to construct a smaller pool of countries that are most similar to the United States. In doing so, we follow Born et al. (2019) who perform an akin robustness check by constructing a "western donor pool".

Figure 8 shows that this a priori restricted donor pool gives rise to almost identical results to those presented in Section 5.1. The doppelganger of the restricted donor pool closely mimics the trajectory of the actual U.S. as well as the original doppelganger, leading to remarkably similar doppelganger gaps for the baseline and robustness specifications.

Figure 8: Robustness test: donor pool



(A) Real GDP development　　**(B)** Doppelganger gap

Note: Actual data (blue line), baseline doppelganger (red line), doppelganger based on restricted donor pool (green line). Shaded area are two standard deviations of difference prior to the election.

5.5.3 Pre-treatment period

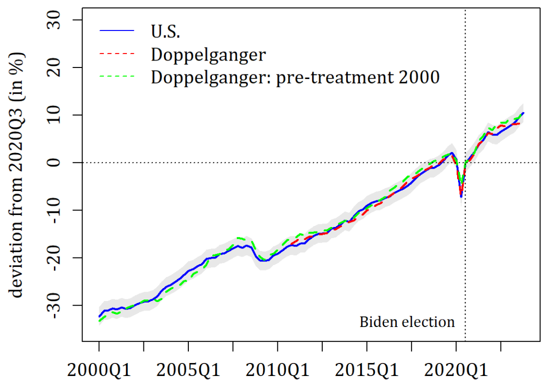
Finally, we take advantage of the fact that we can obtain quarterly data for most of the donor pool countries going back to the year 2000 and test for the robustness of our results with respect to the length of the pre-treatment period. As discussed in Abadie et al. (2010) and Abadie (2021), longer pre-treatment periods are generally preferable to shorter ones, provided that the synthetic control is able to match the treated unit's pre-treatment trajectory of the variable of interest.[22]

Figure 9, Panel A and C indicate that the real GDP paths of the doppelgangers constructed from a five and ten years longer pre-treatment period behave similar to the actual U.S. GDP path and, crucially, to that of our baseline doppelganger. Panels B and D show both doppelganger gaps hovering around zero for the entire pre-treatment period with the exception of the first pre-treatment quarter (2005Q1) for the doppelganger constructed with a pre-treatment period starting in 2005 (see Panel D).[23]

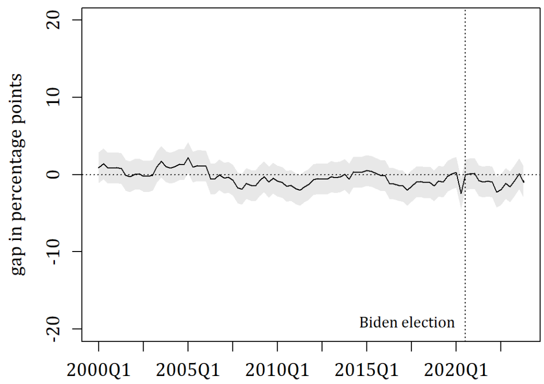
[22] The reason for not using a longer pre-treatment period as our baseline model is the 2008 financial crisis. For some time series, the Great Recession represents a structural break and consequently leads to a worse pre-treatment fit for pre-treatment periods starting before 2008 compared to later sample start dates (see Abadie, 2021, p. 413 for a theoretical discussion of the issue of structural breaks). We also note that our chosen baseline specification still uses a rather large pre-treatment period of 40 quarters, giving us a sufficiently long pre-treatment period.

[23] For reasons discussed in the previous footnote, the results for the decomposition exercises (see Figures A10 to A13) show slightly worse pre-treatment fits than our baseline specification. Nevertheless, our estimates from Section 5.2 remain largely unchanged, albeit with larger confidence bands.

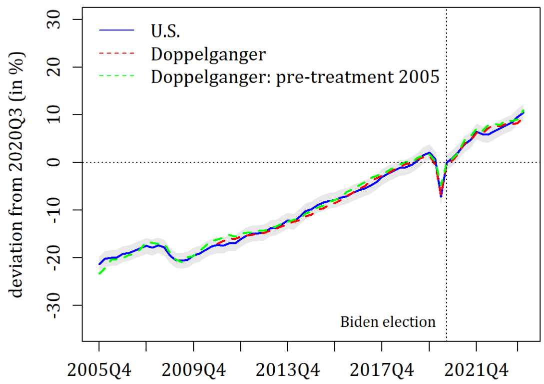
Figure 9: Robustness test: pre-treatment period



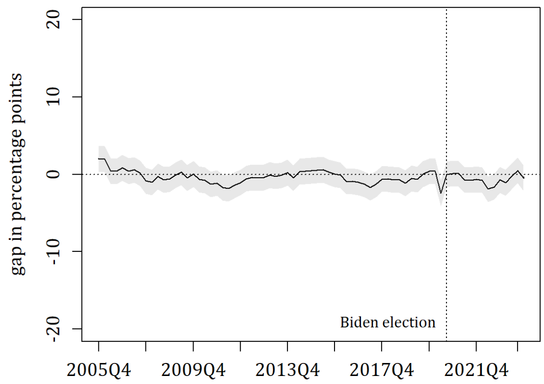
(A) Real GDP development



(B) Doppelganger gap



(C) Real GDP development



(D) Doppelganger gap

Top: Pre-treatment fitting period starting 2000Q1. *Bottom:* Pre-treatment fitting period starting in 2005Q1. *Note:* Real GDP development the U.S. Actual data (blue line), baseline doppelganger (red line), doppelganger based on longer pre-treatment data (green line). *Note:* shaded area are two standard deviations of difference prior to the election.

All in all, the findings in this section underpin the robustness of our baseline specification and the results presented in Section 5. We find that our estimates are mostly insensitive to the definition of the treatment date, the choice of the donor pool as well as the length of the pre-treatment period. Across all three robustness checks, the estimated effects for real private consumption vary between 3.9 and 6.5 percentage points, while the effects for real government consumption range between -3.3 and -1.3 percentage points.

Table 4: Robustness tests: matching of covariates

Predictors	U.S.	Baseline	Period		Treatment	Donor Pool
			2005	2000	Inauguration	Restricted
Consumption share	67.30	50.00	48.90	49.20	47.70	52.20
Investment share	20.30	23.90	23.60	23.50	23.90	21.10
Exports share	11.90	14.60	13.60	11.00	14.30	20.07
Import share	14.60	13.60	12.90	10.60	12.50	19.40
Labor prod. growth	0.40	0.30	0.40	0.40	0.30	0.20
Employment share	61.60	61.90	62.40	63.10	61.90	63.50

Note: All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

Table 5: Robustness tests: doppelganger country weights

Country	Baseline	Period		Treatment	Donor Pool
		2005	2000	Inauguration	Restricted
Australia	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Denmark	0.39	0.57	0.59	0.48	0.54
Finland	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France	0.16	< 0.01	< 0.01	0.10	0.07
Germany	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland	< 0.01	< 0.01	0.04	0.03	< 0.01
Ireland	0.02	< 0.01	< 0.01	0.05	0.03
Italy	< 0.01	< 0.01	< 0.01	< 0.01	*excl.*
Japan	< 0.01	< 0.01	< 0.01	< 0.01	*excl.*
Korea	0.30	0.34	0.30	0.34	*excl.*
Luxembourg	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand	0.12	< 0.01	< 0.01	< 0.01	0.25
Norway	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal	< 0.01	< 0.01	< 0.01	< 0.01	*excl.*
Slovak Republic	< 0.01	0.09	0.08	< 0.01	*excl.*
Spain	< 0.01	< 0.01	< 0.01	< 0.01	*excl.*
Sweden	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom	< 0.01	< 0.01	< 0.01	< 0.01	0.11

6 Discussion

Having presented our main findings above, we now turn to a discussion of our results with respect to their internal and external validity. In the following, we examine potential limitations of our empirical approach by considering factors that could influence the interpretation of the results.

6.1 Internal validity

Recall that our empirical strategy relies on the identifying assumption that the U.S. economy would have evolved in the same way as its doppelganger, had it not been for the election of Joe Biden. Looking at the trajectories of the U.S. and its doppelganger in Section 5, the convex hull condition is satisfied. However, as outlined in Section 4.2, challenges to the identifying assumption could also arise from several other contextual requirements. With respect to the internal validity of our study, three key aspects warrant particular attention: (1) the selection of the donor pool in light of possible spillover effects and potential shocks in other countries, (2) the overlapping of the election of President Biden and the COVID-19 pandemic as well as (3) the limited time horizon after the election.

An important consideration regarding the composition of the donor pool is the presence of *spillover effects* that could violate the no-interference assumption. Being mindful of the trade-off between the no-interference assumption and the donor pool (see Section 4.2), we believe that using similar countries in the donor pool, despite potential spillovers, is preferable to including less similar countries with a lower risk of spillovers. This approach ensures greater comparability and minimizes the risk of interpolation bias, even if it comes at the cost of potentially violating the no-interference assumption. It is worth noting that similar studies, such as Born et al. (2019, 2021), have employed comparable donor pools in their analyses of macroeconomic effects, suggesting that the literature generally comes down on the same side of the trade-off as we do by favoring the use of similar donor countries even at the risk of potential spillover effects.[24]

To get a clearer understanding of the likelihood of spillover effects in our context, we start by noting that the U.S. economy's size and interconnectedness make it a

[24]For example, in Born et al.'s (2019) study of the macroeconomic effects of Brexit, the U.K. synthetic control is largely comprised of deeply integrated economies such as the U.S., New Zealand and European countries. Despite its strong ties to the U.K., the United States makes up over 51% of the doppelganger.

significant driver of global economic conditions. As shown by Kose et al. (2017), the U.S. accounts for nearly a quarter of global GDP and plays a central role in international trade and financial markets. Consequently, policy shocks in the U.S. can have spillover effects that reverberate throughout the global economy. Assuming that the election of Joe Biden impacted other economies through positive spillover effects (in the form of, e.g., higher demand for imports from other countries), our estimated effects would be biased downwards and potentially mask any positive effects. However, we think it is improbable that the absence of a distinct "Biden effect" on GDP, investment or trade can be attributed to positive spillover effects. This is because many of the policies enacted by the Biden administration, such as the *Inflation Reduction Act*, were domestically oriented (see Section 2) and, if anything, more likely to impede foreign economic growth. It thus seems more plausible that the act's provisions to boost domestic manufacturing and production may lead to a reduced demand for imports and a reallocation of resources away from foreign economies, thereby adversely impacting the economies of countries that export them to the U.S. through negative spillovers. Notice also that the potential bias arising from spillover effects is limited to donor countries; spillovers to countries that are part of the donor pool but not part of the doppelganger do not bias the synthetic control estimate (Abadie et al., 2015, p. 504). As documented in Section 5.1, the five countries that make up the synthetic control for the U.S. are: Denmark, France, Ireland, New Zealand and South Korea. These countries are all advanced economies with varying degrees of economic ties to the United States. In particular, U.S. trade accounts for for 3.0% of Danish, 2.9% of French, 15.3% of Irish, 3.9% of New Zealand and 11.5% of South Korean GDP (UN Comtrade, 2024). Given the relatively large share of U.S. trade with respect to Irish and South Korean GDP, we run an additional robustness test in which we exclude Ireland and South Korea from the donor pool. The results (see Figures A8 and A9) reaffirm our previous finding from Section 5.5.2 that our baseline estimates are robust to the choice of the donor pool and – to some extent – dispel concerns regarding spillovers biasing our results. Nonetheless, the remaining donor countries still exhibit some form of economic integration with the United States, meaning we cannot entirely dismiss the possibility of spillover effects. This potential presence of spillover effects serves as a caveat when interpreting our results, acknowledging that they may overstate the true impact of the Biden presidency on U.S. macroeconomic outcomes. Given the potential presence of negative spillover effects, the estimates in Section 5 serve as an upper bound (in absolute terms) on the magnitude of the macroeconomic effect of the Biden presidency.

Another concern with respect to the donor pool is the *suitability of the control countries*. If the countries comprising the synthetic control were subject to a similar policy intervention or shock during any of the post-treatment periods, this would impair the synthetic control's capability to serve as a credible counterfactual and cast serious doubt on our identifying assumption (Abadie et al., 2010, pp. 498-499). We verify that during the post-treatment period, none of the five donor pool economies experienced large, idiosyncratic shocks that could challenge our identifying assumption. Focusing on the political sphere, no legislative elections were held between the end of 2020 and 2023 in Ireland and South Korea. In Denmark, the left-leaning bloc retained its majority in the 2022 general election, while New Zealand held a general election in 2023Q4 which led to the formation of a new government on November 27$^{\text{th}}$, 2023. Lastly, the French elections in 2022 saw the governing *Ensemble* coalition remain the largest bloc in a hung parliament. Given these electoral dynamics, it is improbable that any of the five donor countries experienced idiosyncratic shocks during the post-treatment period that would significantly call our identifying assumption into question. With the exception of New Zealand, no donor country underwent a change in government. Moreover, the late timing of New Zealand's governmental transition renders it unlikely to have exerted a substantial economic impact in the 2023 calendar year. There is also no evidence for other, non-political shocks with ramifications for macroeconomic development.

A second threat to the internal validity of our results may come from the *co-incidence of the 2020 presidential election and other shocks*, such as the COVID-19 pandemic. As pointed out by Bohn et al. (2014, p. 262), any other major events around the time of treatment with implications for the variable of interest may prevent the synthetic control method from uncovering the true treatment effect.[25] Specifically, we must ascertain whether the treatment effects identified in Section 5 are solely attributable to the election of Joe Biden or whether concurrent events could be responsible for these effects, too. The response to the COVID-19 pandemic, in particular, presents a significant potential confounding

[25]Although similar, this identification challenge differs from previously discussed concerns regarding the absence of shocks in the donor pool during post-treatment periods. Much like the continuity assumption in regression discontinuity designs (Imbens & Lemieux, 2008, p. 618), our concern here lies with competing interventions coinciding with the treatment under study.

factor. Given that the pandemic response in the U.S. was largely decided at the state level, it is crucial for our empirical approach to disentangle any economic effects attributable to President Biden's (economic) policies from those driven by the U.S. response to the COVID-19 pandemic. If, on average, the U.S. had less stringent lockdowns and quicker reopening phases than other countries, it is conceivable that the effects documented in Section 5 are, at least partly, due to the states' laxer pandemic responses, and not President Biden himself.[26]

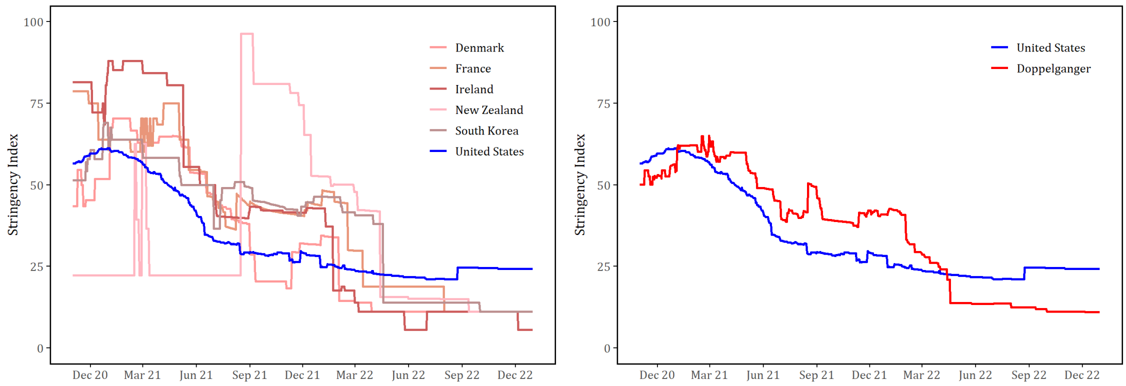
To validate our identification strategy, we consult the Oxford COVID-19 government response tracker by Hale et al. (2021) and compare the stringency of the containment and closure policies in the U.S. and our five donor economies. In particular, we aggregate the stringency index for the 50 U.S. states as well as the District of Colombia and calculate an average weighted stringency score for the U.S.[27] Figure 10, Panel A plots the stringency indices for the five donor economies as well as the average weighted stringency for the U.S. between November 2020 and the end of 2022 (the last day for which data is available), while Panel B compares the U.S. average stringency to that of its doppelganger. Overall, the evidence suggests that the stringency of the pandemic response in the United States and the five donor countries was relatively similar, though there were some notable differences over time. While the distance between the U.S. stringency index and that of its doppelganger was less than ten index points during most of the sample period, the U.S. average did consistently remain below that of its doppelganger from early 2021 to mid-2022. Notably, the stringency of the government response in the U.S. and the donor economies was very similar around the time of the 2020 U.S. presidential election and inauguration, which – at least in part – allays concerns that distinctly stringent pandemic responses present a competing shock to the election and could bias our results. Nonetheless, the relatively laxer pandemic response in the U.S. over time means we cannot fully rule out that the effects documented in Section 5 could be somewhat contaminated by the varying levels of pandemic response stringency in the U.S. and the donor countries.

A final consideration pertains to the potential *time lags between policy implementation and observable economic outcomes*. Economic policies, particularly those

[26]It is worth highlighting that these concerns only relate to the post-treatment period starting in 2020Q4; any differential effects before then would be picked up by the doppelganger.

[27]We use the states' quarterly real GDP (Bureau of Economic Analysis, 2024) to weigh their respective stringency scores on a given day and combine the weighted data to an aggregated score for the U.S. This procedure is similar to Hallas et al. (2021) who use population weights to obtain an average weighted stringency index for the U.S.

Figure 10: Stringency of COVID-19 government response



(A) U.S. and donor countries **(B)** U.S. and doppelganger

Source: Oxford COVID-19 Government Response Tracker (Hale et al., 2021). Time series show stringency index for selected countries between 01/11/2020 and 31/12/2022. U.S. data is average GDP-weighted stringency index for the 50 states and the District of Columbia, while doppelganger data is average stringency index for the five donor countries, weighted by the synthetic control weights in Table 2.

aimed at structural changes or long-term growth, can take considerable time to materialize. It is therefore plausible that our assessment may not capture the complete picture of Biden's impact. This is especially true given the fact that President Biden's most significant policy item, the *Inflation Reduction Act*, was only passed in 2022Q3. This leaves us with only five post-IRA observations to discern any potential effect – a relatively short time span. Additionally, many of the IRA's provisions for clean energy investments and healthcare reforms are rolled out in a staggered way and may take years to fully manifest in macroeconomic indicators such as GDP or trade volumes (117th U.S. Congress, 2022). Given these considerations, our analysis may be constrained in its ability to capture the full extent of the "Biden effect" on the U.S. economy. Future research with a longer post-treatment time horizon may be better positioned to evaluate the long-term impacts of the Biden administration's economic policies. Additionally, different methodologies such as structural economic models or time-series analysis techniques that explicitly account for dynamic effects could provide valuable insights into the temporal dimension of policy impacts. In light of these limitations, our results should be interpreted as preliminary evidence of the short-term effects of the Biden presidency, with the understanding that the full economic implications of his administration's policies may only become apparent over a more extended period.

6.2 External validity

The results of our study indicate that President Joe Biden's impact on the macroeconomic performance of the United States has been modest. While our findings echo the results of Born et al. (2021), who present evidence for a negligible "Trump effect", the external validity of our results warrants careful consideration. Although our study provides insights into the economic effects of the Biden presidency, it is crucial to recognize that the impact of political leaders on economic outcomes can vary significantly across different contexts and individuals. This perspective is supported by seminal work in the field of leader effects. As noted in Section 3, Jones and Olken (2005) as well as Easterly and Pennings (2020) demonstrate that individual leaders can have substantial impacts on economic growth but also stress the heterogeneity of leader effects across different political systems and economic environments. Furthermore, recent research by Funke et al. (2023) emphasizes the idiosyncratic nature of (populist) leaders' economic impacts. Their study reveals that the economic consequences of populist leadership can differ markedly based on various factors, including the specific policies implemented, the economic conditions at the time of taking office, and the institutional framework of the country. These studies underscore the importance of considering each leader and their economic impact individually, rather than attempting to generalize findings across different contexts. The unique combination of a leader's personal characteristics, policy choices, and the prevailing economic and institutional environment creates a complex interplay that can lead to diverse outcomes. In light of this, the external validity of our study on President Biden's economic impact is inherently limited. While it provides valuable insights into the specific case of the Biden administration in the United States, caution should be exercised when attempting to extrapolate these findings to other leaders or political contexts.

7 Conclusion

As the economy is poised to be a decisive factor in the upcoming 2024 U.S. presidential election, our paper takes stock of President Biden's economic record using the synthetic control method. By constructing a counterfactual scenario that simulates the trajectory of the U.S. economy in the absence of Biden's presidency, we isolated the potential economic effects attributable to his policies.

Throughout this exercise, we find neither positive nor negative effects of "Bidenomics" on the U.S. economy on aggregate. During the three years following the 2020 presidential election, real U.S. GDP, investment, exports and imports have not developed markedly different than in a counterfactual, non-Biden scenario. It is important to emphasize that this finding does not imply that the U.S. economy under President Biden's administration did not perform well. Rather, it suggests that the positive economic developments observed are not directly attributable to President Biden's actions.

While we do not observe a significant effect of the current administration on aggregate output, we do find evidence for a Biden effect when decomposing GDP. Notably, we observe that real private consumption is around 4.8 percentage points higher in the U.S. than in its synthetic control, while real government consumption is 3.3 percentage points lower three years post-election. Using Andrews's (2003) end-of-sample instability test and standard placebo tests, we present evidence that these effects are both significant as well as causal. We also show that our estimates are largely robust to the choice of the donor pool and the length of the pre-treatment period used to construct the synthetic control as well as the definition of the treatment date. Potential negative spillover effects that could impede economic growth in our donor pool mean that our estimates are best interpreted as an upper bound (in absolute terms) of the true "Biden effect". Additionally, our assessment is only a snapshot in time and, given the gradual implementation of many of President Biden's policy items, might not fully capture the totality of their long-term economic impacts.

Our paper contributes to the broader literature on the economic effects of political leadership and offers valuable insights for policymakers and scholars alike. As the administration's policies continue to unfold, ongoing analysis will be crucial to fully understanding their long-term implications. Building on our findings, we propose two promising avenues for future research. First, subsequent research could look at more granular indicators of economic activity to discern the economic consequences of Biden's policies at the sub-aggregate level. In the context of Biden's *Infrastructure Investment and Jobs Act* or the *Inflation Reduction Act*, this could involve examining sector-specific impacts of clean energy and infrastructure investments, such as changes in employment, productivity, and innovation at the sectoral level. Second, we think that – on a more general level – another interesting avenue for future research is the systematic application of the synthetic control method to evaluate the economic performance of past U.S. presidents. While the synthetic control method has been employed to assess the impact of the Trump administration (Born et al., 2021), its application to other presidencies remains limited. Many of the studies mentioned in Section 3 employ diverse methodologies and have a slightly different focus of analysis. While these approaches are valid in their own right, they complicate direct comparability. Extending the synthetic control analysis to a broader range of presidential terms would provide a consistent methodological framework for cross-presidential comparisons and enhance our understanding of how different administrations have influenced economic outcomes.

Looking ahead to the presidential election on November 5th, 2024, our findings provide a nuanced perspective on the economic legacy of the Biden administration by highlighting the importance of looking beyond headline figures to understand the true impact of economic policies. As the political discourse continues to revolve around economic issues, our study serves as a reminder that the relationship between presidential actions and economic outcomes is often more subtle and multi-faceted than campaign rhetoric might suggest. Ultimately, our analysis contributes to a broader understanding of the dynamics between political leadership and economic performance, which will remain a critical area of inquiry as future administrations navigate their own policy agendas.

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A Appendix

A.1 Data sources

To give a structured overview and aid the replicability of our results, we report our raw data sources below. Unless otherwise specified, the data was obtained from the OECD Economic Outlook database (May 2024 edition).

Table A1: Summary of data sources

Section	Variable	Original name	Source
2	Betting odds	RCP betting average 2020 U.S. President	RealClearPolitics (2020)
5	Quarterly real GDP	Gross domestic product, volume, market prices	
5	Quarterly real consumption	Private final consumption expenditure, volume	
5	Quarterly real investment	Gross fixed capital formation, total, volume	
5	Quarterly real exports	Exports of goods and services, volume	
5	Quarterly real imports	Imports of goods and services, volume	
5	Quarterly real government consumption	Government final consumption expenditure, volume	
5	Quarterly employment	Total employment, labour force survey basis	
5	Annual population	Working-age population, age 15-74	
6	Annual GDP share of ex- and imports	Trade flows: exports / imports	UN Comtrade (2024)
6	Stringency index	All C indicators, plus H1 which records public information campaigns	Oxford COVID-19 Government Response Tracker (Hale et al., 2021)
6	Quarterly Real GDP by U.S. state	Real GDP (millions of chained 2017 dollars)	Bureau of Economic Analysis (2024)

Note: Annual population is linearly interpolated to the quarterly frequency.

A.2 GDP decomposition: summary statistics

Table A2: Decomposition results: matching of covariates and doppelganger country weights

Country weights	P-Consumption based	Investment based	Export based	Import based	G-consumption based
Australia	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Denmark	0.24	< 0.01	< 0.01	0.53	< 0.01
Finland	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Germany	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland	< 0.01	< 0.01	0.18	< 0.01	< 0.01
Ireland	0.02	< 0.01	< 0.01	< 0.01	< 0.01
Italy	< 0.01	< 0.01	0.33	< 0.01	0.05
Japan	< 0.01	0.21	< 0.01	< 0.01	0.26
Korea	0.41	0.16	0.24	0.01	< 0.01
Luxembourg	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand	0.24	0.60	0.25	0.42	0.19
Norway	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal	0.09	0.02	< 0.01	0.04	0.50
Slovak Republic	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Spain	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Sweden	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01

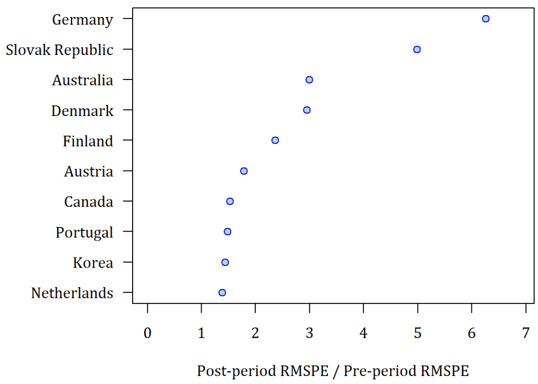
Predictors	Actual U.S.	P-Consumption based	Investment based	Export based	Import based	G-consumption based
Consumption share	67.30	52.40	57.70	56.00	53.30	61.40
Investment share	20.30	24.60	24.70	22.20	21.50	2< 0.01
Exports share	11.90	13.70	13.70	16.20	15.20	27.40
Import share	14.60	12.80	13.50	15.20	14.50	27.00
Labor productivity growth	0.40	0.30	0.20	0.20	0.20	< 0.01
Employment share	61.60	63.80	68.20	61.80	64.90	62.10

Top part: Country weights for the baseline doppelgangers in the main text. *Bottom part:* Values shown for the baseline doppelgangers in the main text. All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

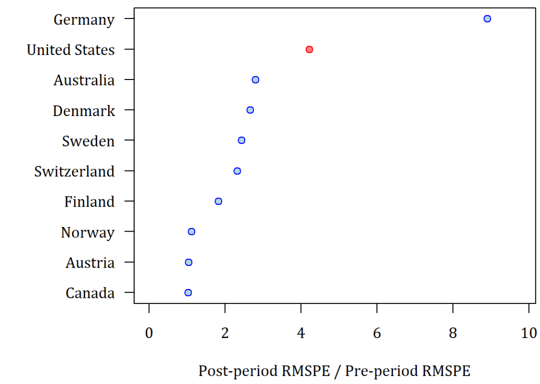
A.3 Inference: details

A.3.1 Relative doppelganger gap measures

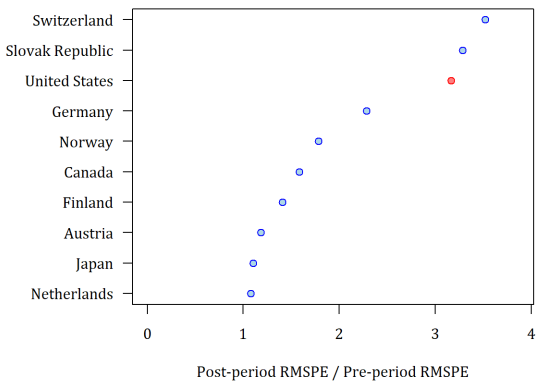
Figure A1: Post-/pre-treatment RMSPE ratios



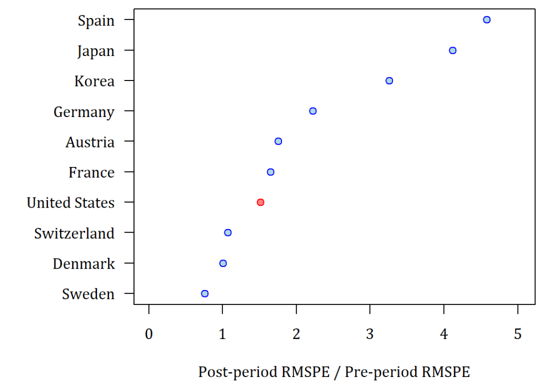
(A) Real GDP



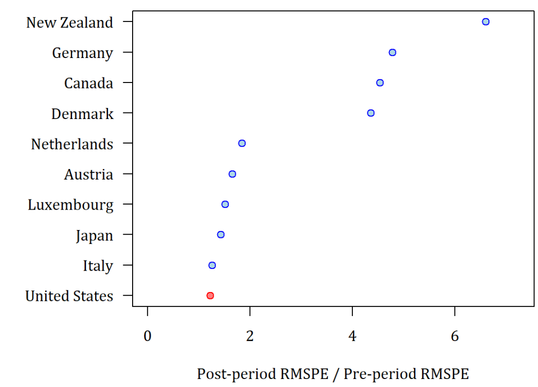
(B) Real private consumption



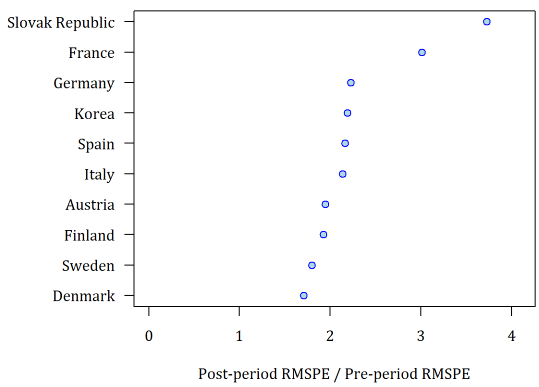
(C) Real government consumption



(D) Real investment



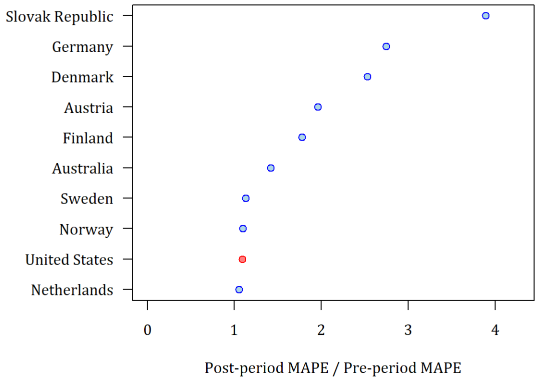
(E) Real exports



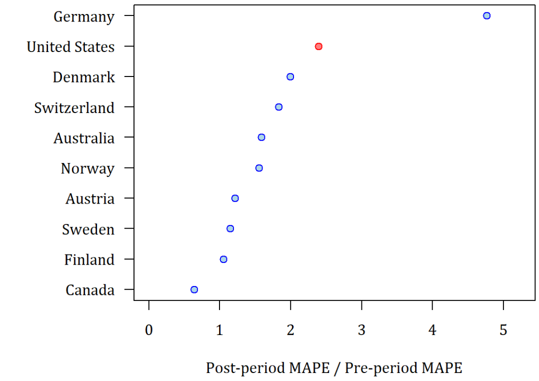
(F) Real imports

Note: Panels show the ten largest root mean squared prediction errors ρ_1 for the U.S. and the donor pool countries. If the RMSPE for the U.S. is among the highest ten, it is highlighted in red.

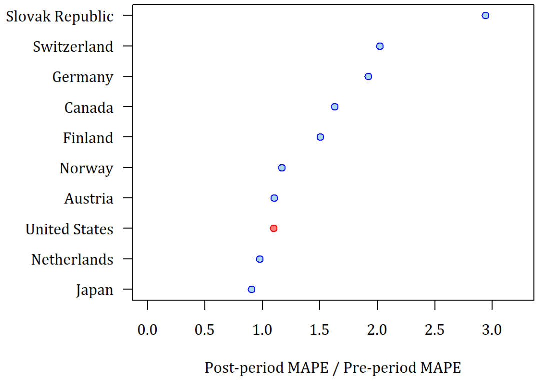
Figure A2: Post-/pre-treatment MAPE ratios



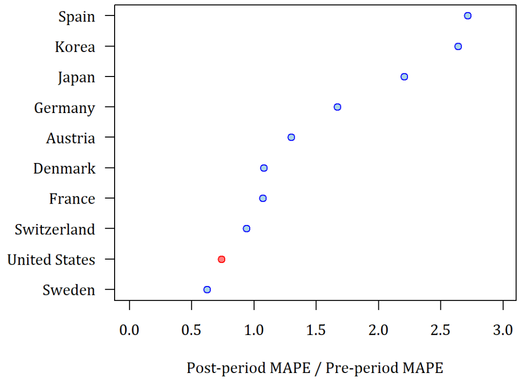
(A) Real GDP



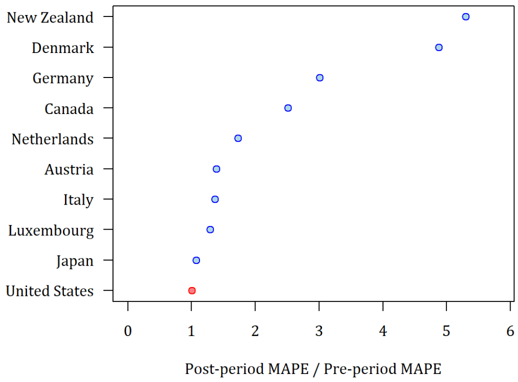
(B) Real private consumption



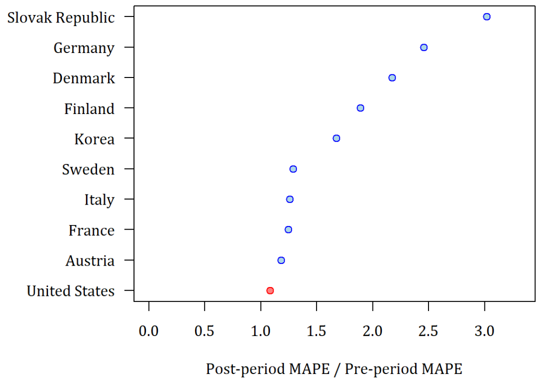
(C) Real government consumption



(D) Real investment



(E) Real exports

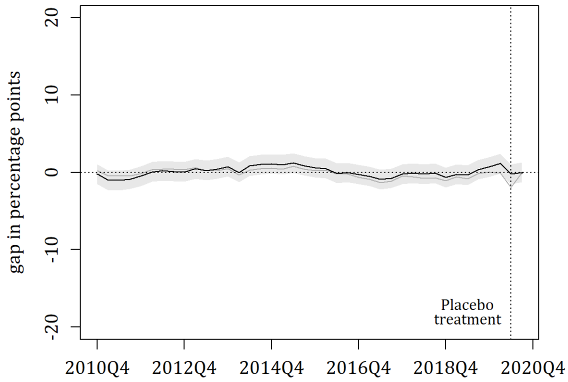


(F) Real imports

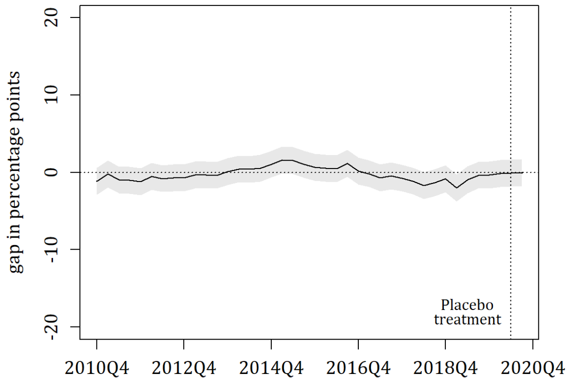
Note: Panels show the ten largest relative maximum absolute prediction error ρ_2 for the U.S. and the donor pool countries. If the MAPE for the U.S. is among the highest ten, it is highlighted in red.

A.3.2 Anticipation effects

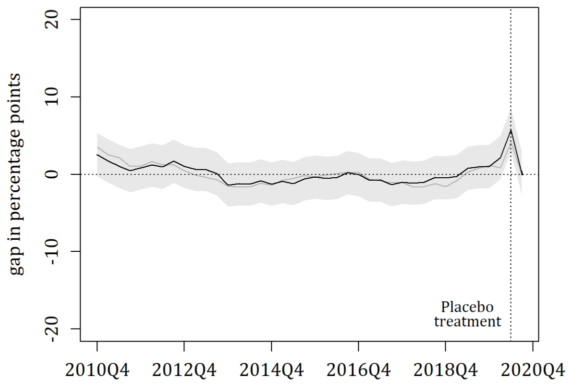
Figure A3: In-time placebo test: anticipation effects



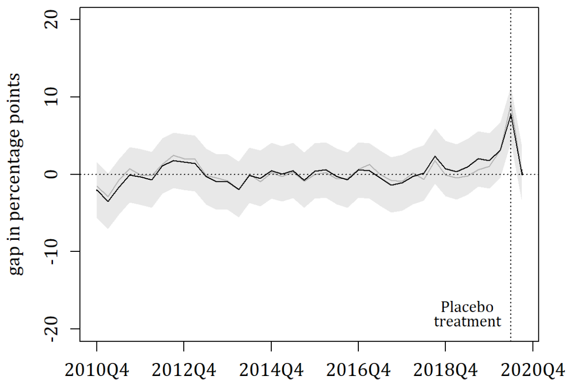
(A) Real GDP



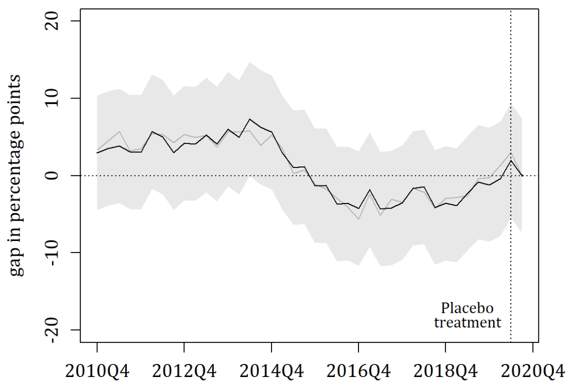
(B) Real private consumption



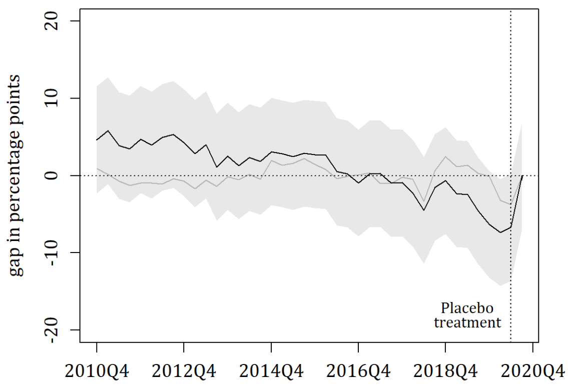
(C) Real government consumption



(D) Real investment



(E) Real exports



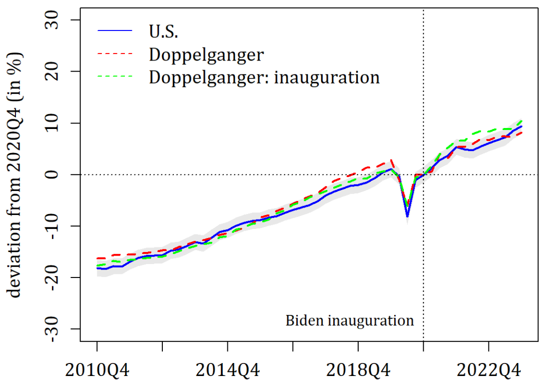
(F) Real imports

Note: U.S. doppelganger gap (black line), with gray line representing in-time placebo doppelganger gaps estimated by considering fictitious Biden election in 2020Q3.

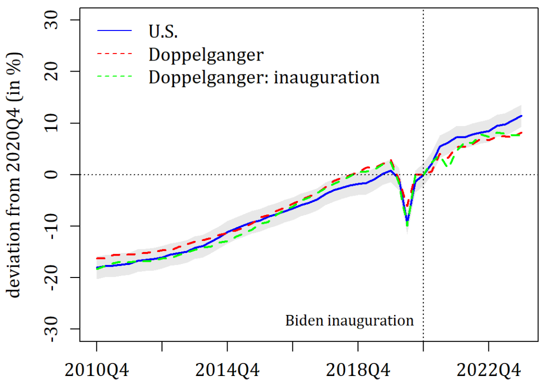
A.4 Robustness: details and summary statistics

A.4.1 Treatment date

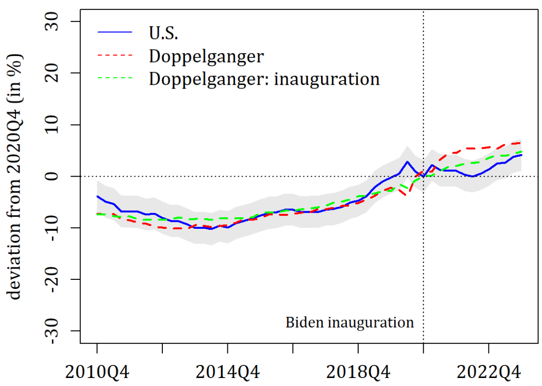
Figure A5: Treatment date robustness: decomposition results (U.S. macroeconomic trajectories)



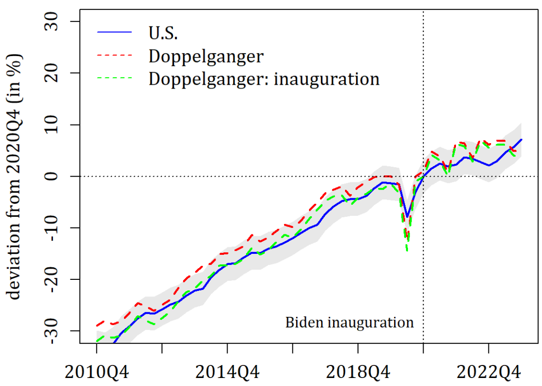
(A) Real GDP



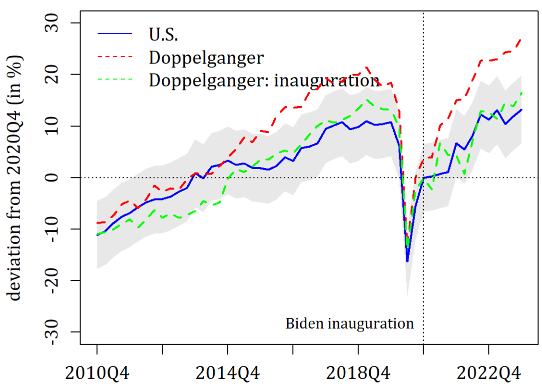
(B) Real private consumption



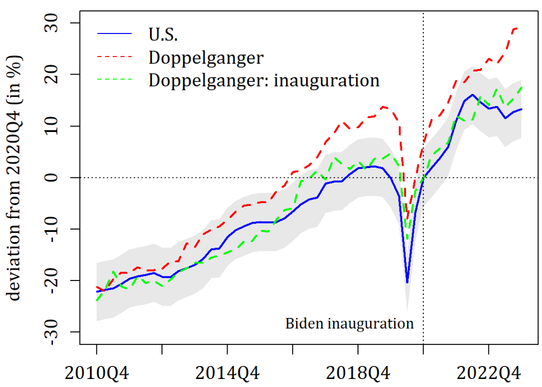
(C) Real government consumption



(D) Real investment



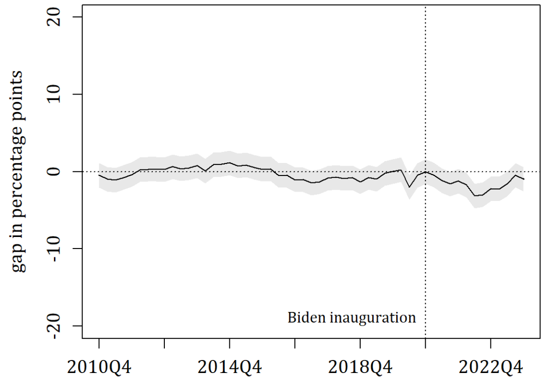
(E) Real exports



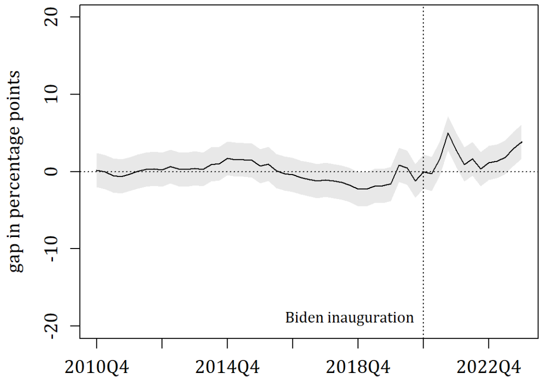
(F) Real imports

Note: Actual data (blue line), baseline doppelganger (red line), doppelganger based on inauguration treatment (green line). Shaded area are two standard deviations of difference prior to the election.

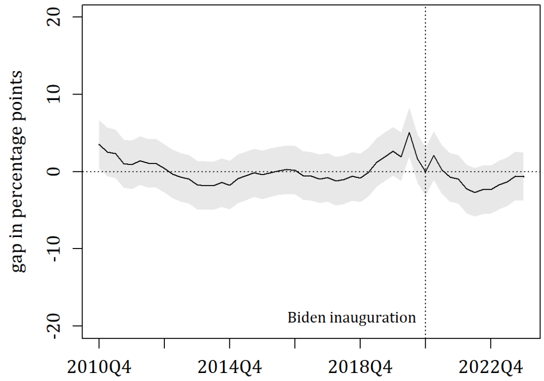
Figure A4: Treatment date robustness: decomposition results (doppelganger gaps)



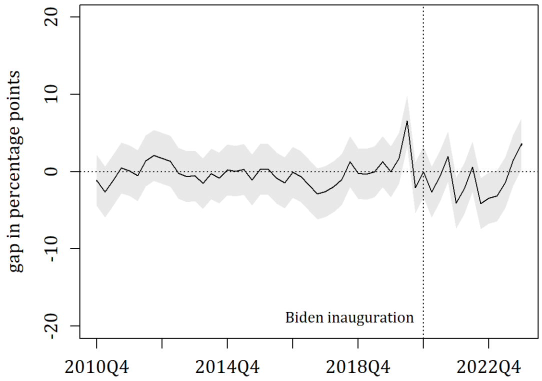
(A) Real GDP



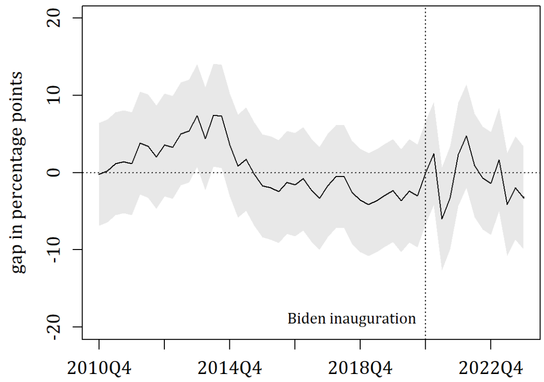
(B) Real private consumption



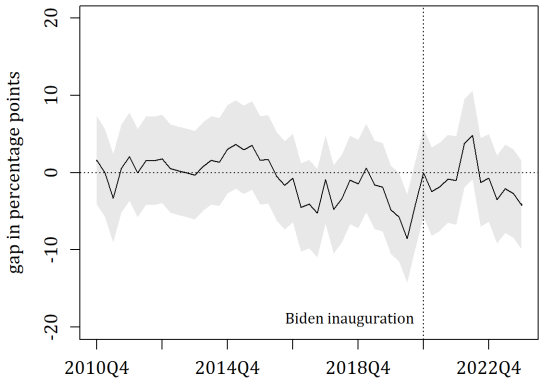
(C) Real government consumption



(D) Real investment



(E) Real exports



(F) Real imports

Note: Shaded area are two standard deviations of difference prior to the election.

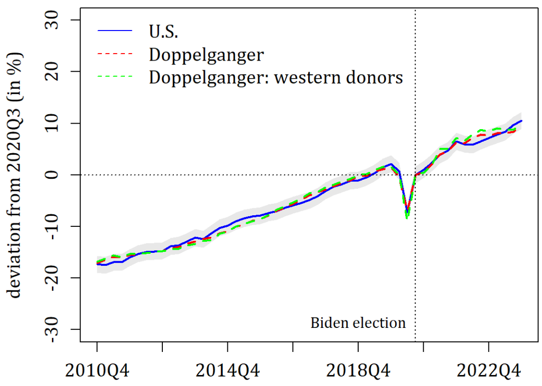
Table A3: Treatment date robustness: decomposition results (matching of covariates and doppelganger country weights)

Country weights		Private consumption based	Investment based	Export based	Import based	Government consumption based
Australia		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada		< 0.01	0.02	< 0.01	< 0.01	0.01
Denmark		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Finland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France		< 0.01	< 0.01	< 0.01	0.01	< 0.01
Germany		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland		< 0.01	< 0.01	0.01	-< 0.01	0.26
Ireland		< 0.01	< 0.01	< 0.01	0.02	< 0.01
Italy		0.21	< 0.01	0.37	< 0.01	0.40
Japan		< 0.01	0.08	0.12	< 0.01	0.29
Korea		0.23	0.27	< 0.01	0.67	0.02
Luxembourg		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand		0.56	0.63	0.50	0.08	< 0.01
Norway		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal		< 0.01	< 0.01	< 0.01	0.22	0.02
Slovak Republic		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Spain		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Sweden		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Predictors	Actual U.S.					
Consumption share	67.30	57.80	56.90	59.90	53.00	56.30
Investment share	20.30	23.90	25.40	21.60	26.10	20.40
Exports share	11.90	18.60	13.80	22.80	13.90	14.10
Import share	14.60	17.90	13.70	21.70	13.40	13.10
Labor productivity growth	0.30	0.20	0.30	0.10	0.30	0.00
Employment share	61.60	64.00	67.80	62.20	62.20	61.70

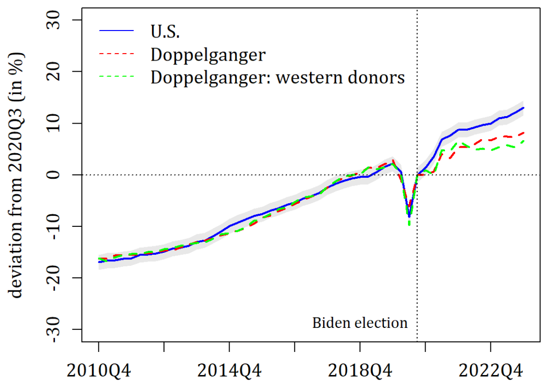
Top part: Country weights for the baseline doppelgangers in the main text. *Bottom part:* Values shown for the baseline doppelgangers in the main text. All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

A.4.2 Donor pool

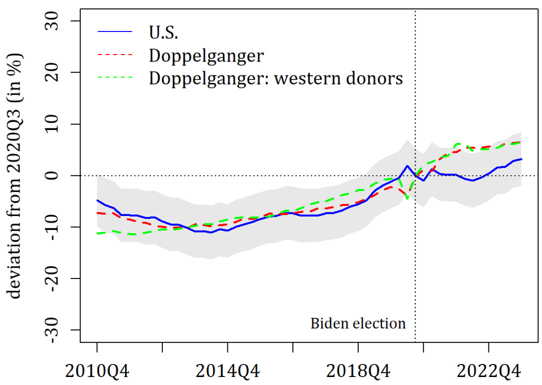
Figure A6: Donor pool robustness: decomposition results (U.S. macroeconomic trajectories)



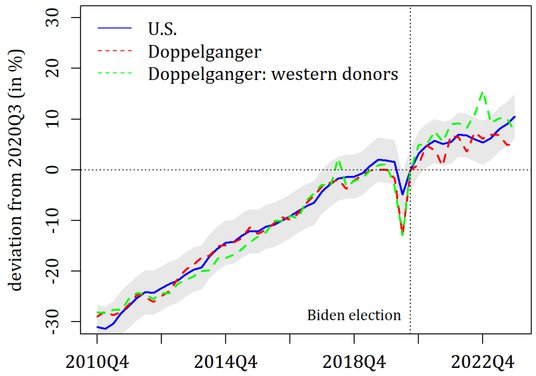
(A) Real GDP



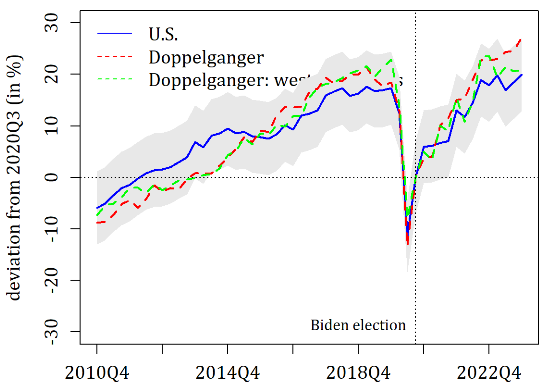
(B) Real private consumption



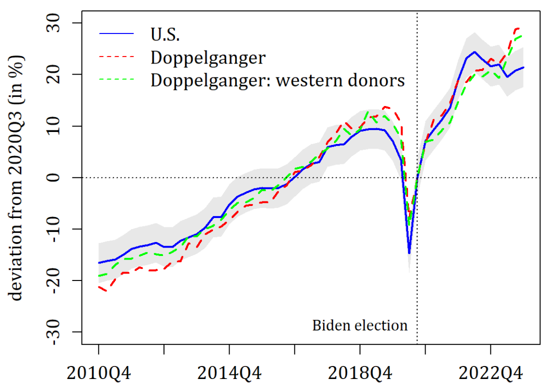
(C) Real government consumption



(D) Real investment



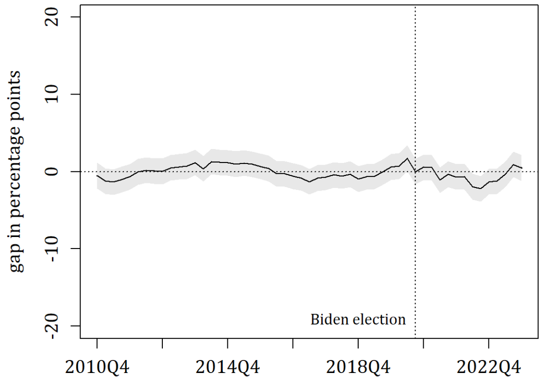
(E) Real exports



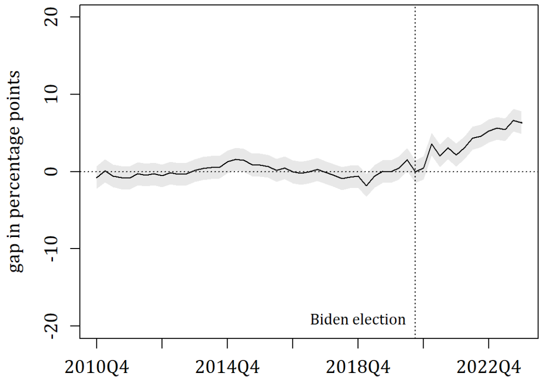
(F) Real imports

Note: Actual data (blue line), baseline doppelganger (red line), doppelganger based on restricted donor pool (green line). Shaded area are two standard deviations of difference prior to the election.

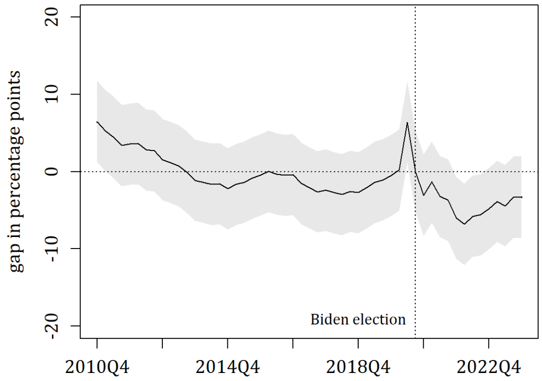
Figure A7: Donor pool robustness: decomposition results (doppelganger gaps)



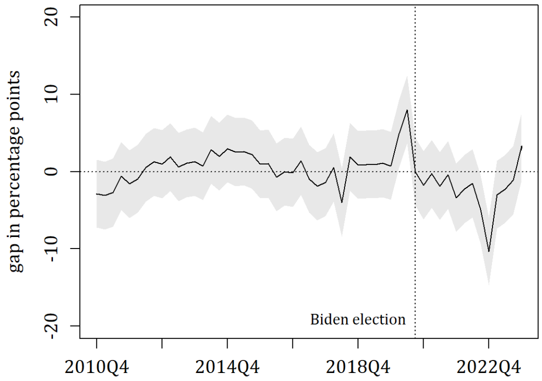
(A) Real GDP



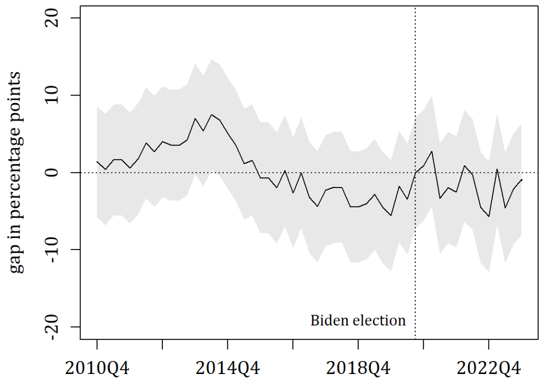
(B) Real private consumption



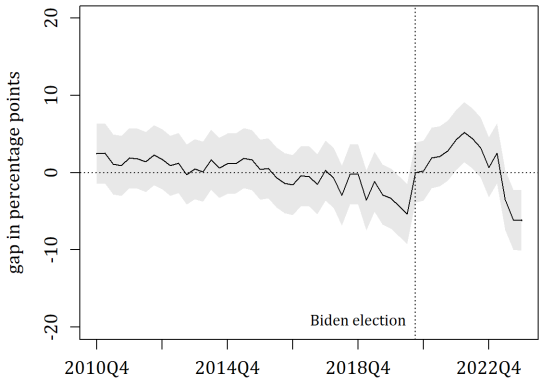
(C) Real government consumption



(D) Real investment



(E) Real exports



(F) Real imports

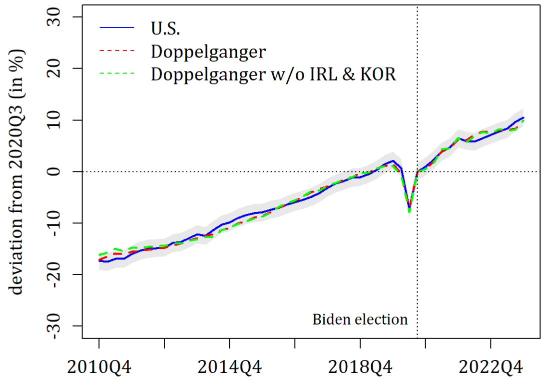
Note: Shaded area are two standard deviations of difference prior to the election.

Table A4: Donor pool robustness: decomposition results (matching of covariates and doppelganger country weights)

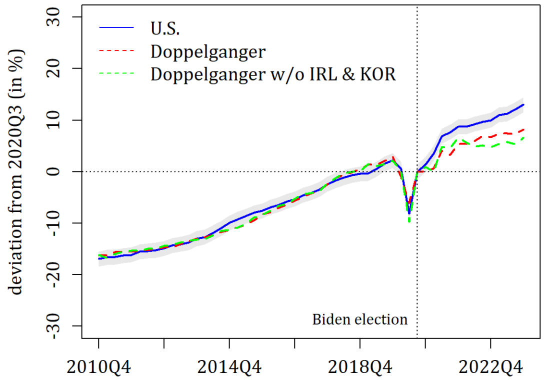
Country weights		Private consumption based	Investment based	Export based	Import based	Government consumption based
Australia		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Denmark		0.52	0.31	< 0.01	0.51	0.28
Finland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France		0.04	0.28	0.38	0.17	0.28
Germany		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland		< 0.01	0.07	0.06	< 0.01	0.31
Ireland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Italy	*excluded*					
Japan	*excluded*					
Korea	*excluded*					
Luxembourg		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand		0.28	0.33	0.15	0.12	0.13
Norway		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal	*excluded*					
Slovak Republic	*excluded*					
Spain	*excluded*					
Sweden		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom		0.16	< 0.01	0.41	0.20	< 0.01
Predictors	Actual U.S.					
Consumption share	67.30	53.50	53.80	58.80	53.10	51.90
Investment share	20.30	20.70	21.80	20.40	20.30	20.70
Exports share	11.90	18.20	19.40	32.50	20.90	14.80
Import share	14.60	17.60	19.10	33.00	20.50	14.60
Labor productivity growth	0.40	0.20	0.20	0.10	0.20	0.20
Employment share	61.60	64.20	63.20	62.10	62.10	64.00

Top part: Country weights for the baseline doppelgangers in the main text. *Bottom part:* Values shown for the baseline doppelgangers in the main text. All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

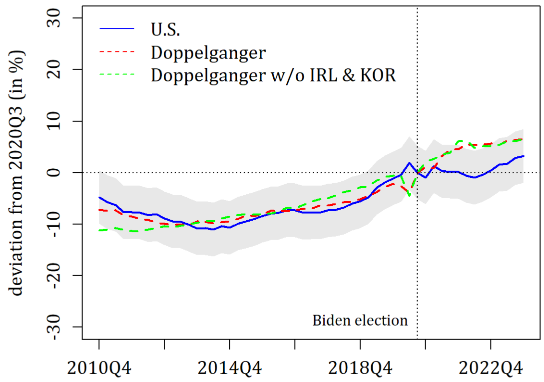
Figure A8: Spillover robustness: decomposition results (U.S. macroeconomic trajectories)



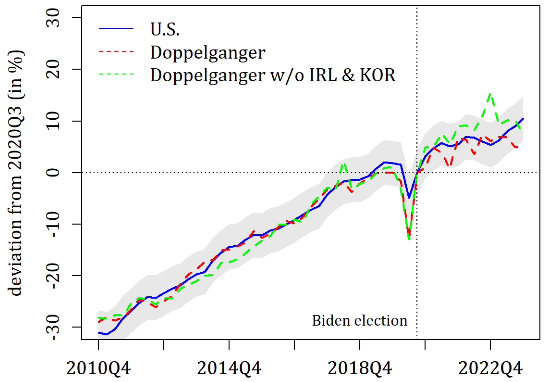
(A) Real GDP



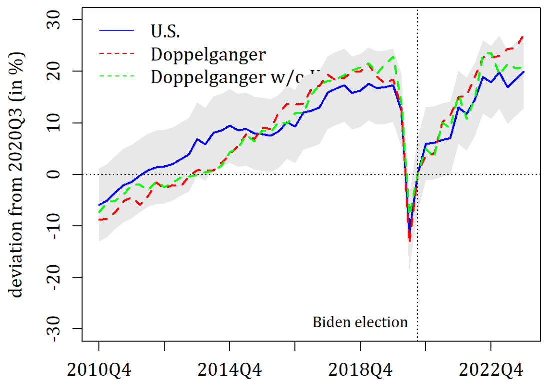
(B) Real private consumption



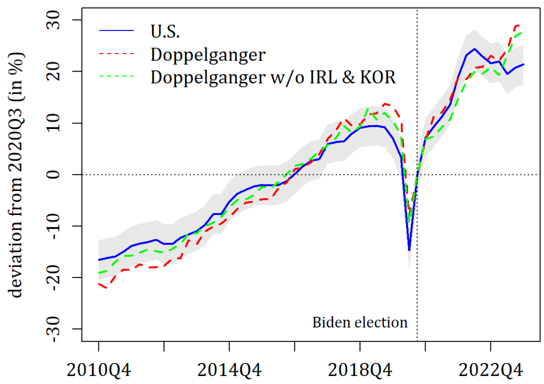
(C) Real government consumption



(D) Real investment



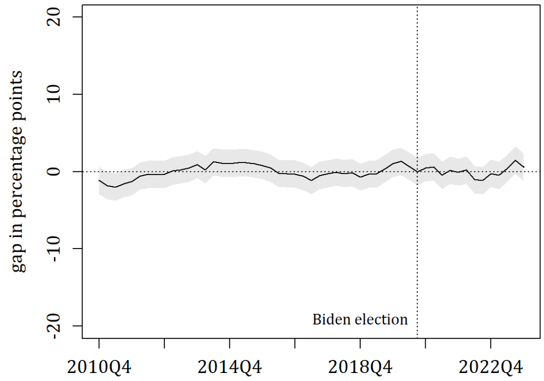
(E) Real exports



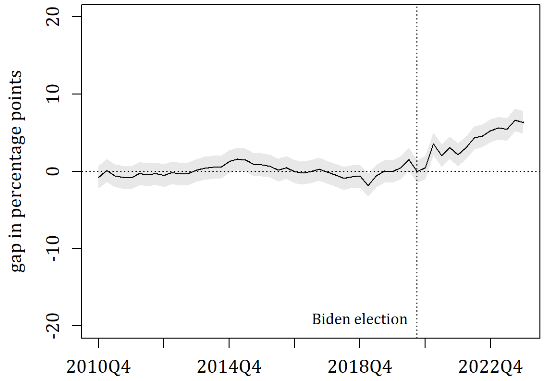
(F) Real imports

Note: Actual data (blue line), baseline doppelganger (red line), doppelganger based on restricted donor pool (green line). Shaded area are two standard deviations of difference prior to the election.

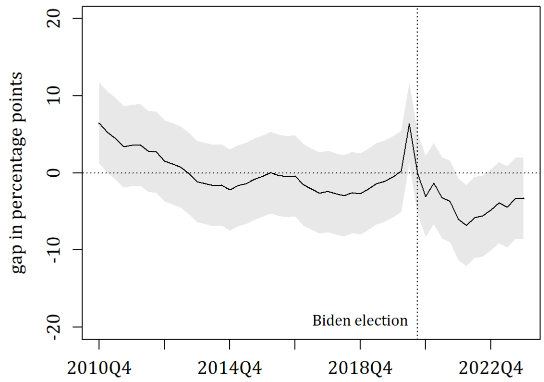
Figure A9: Spillover robustness: decomposition results (doppelganger gaps)



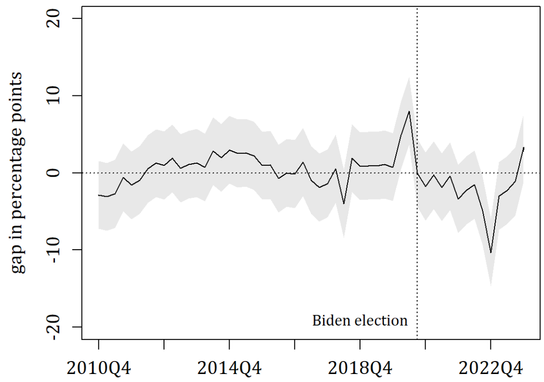
(A) Real GDP



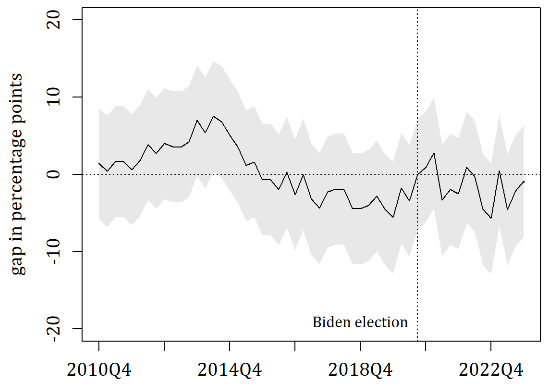
(B) Real private consumption



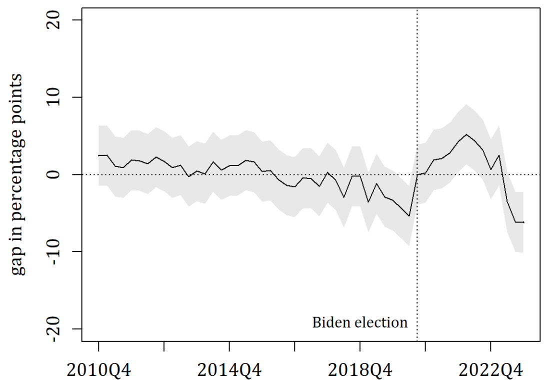
(C) Real government consumption



(D) Real investment



(E) Real exports



(F) Real imports

Note: Shaded area are two standard deviations of difference prior to the election.

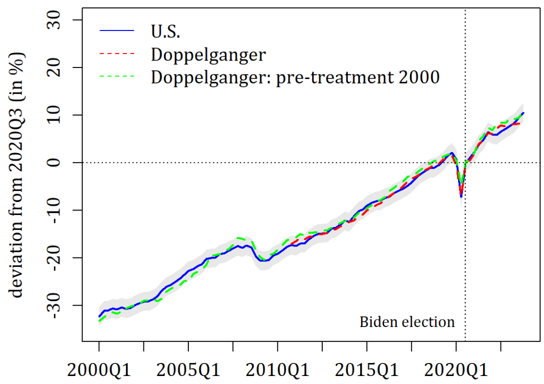
Table A5: Spillover robustness: decomposition results (matching of covariates and doppelganger country weights)

Country weights		Real GDP based	Priv. consumption based	Investment based	Export based	Import based	Gov. consumption based
Australia		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Denmark		0.62	0.52	0.31	< 0.01	0.51	0.28
Finland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France		0.07	0.04	0.28	0.38	0.17	0.28
Germany		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland		< 0.01	< 0.01	0.07	0.06	< 0.01	0.31
Ireland	*excluded*						
Italy		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Japan		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Korea	*excluded*						
Luxembourg		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand		0.20	0.28	0.33	0.15	0.12	0.13
Norway		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Slovak Republic		0.10	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Spain		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Sweden		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom		< 0.01	0.16	< 0.01	0.41	0.20	< 0.01
Predictors	Actual U.S.						
Consumption share	67.30	51.00	53.50	53.80	58.80	53.10	51.90
Investment share	20.30	21.00	20.70	21.80	20.40	20.30	20.70
Exports share	11.90	22.40	18.20	19.40	32.50	20.90	14.80
Import share	14.60	21.40	17.60	19.10	33.00	20.50	14.60
Labor productivity growth	0.40	0.20	0.20	0.20	0.10	0.20	0.20
Employment share	61.60	62.40	64.20	63.20	62.10	62.10	64.00

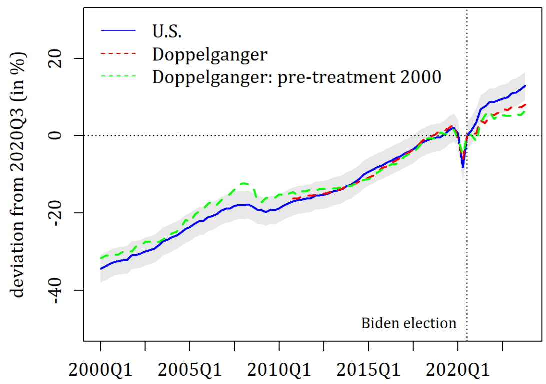
Top part: Country weights for the baseline doppelgangers in the main text. *Bottom part:* Values shown for the baseline doppelgangers in the main text. All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

A.4.3 Pre-treatment period

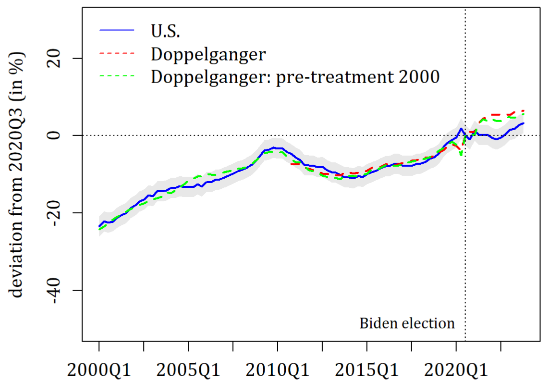
Figure A10: 2000Q1 pre-treatment period robustness: decomposition results (U.S. macroeconomic trajectories)



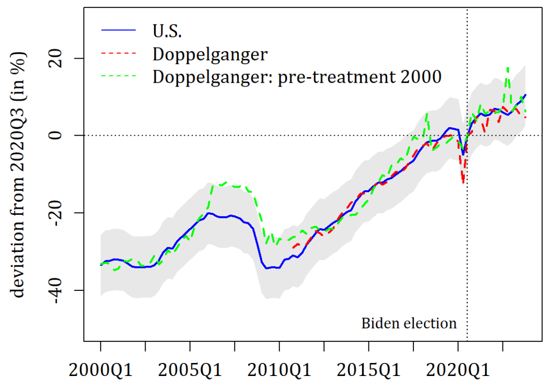
(A) Real GDP



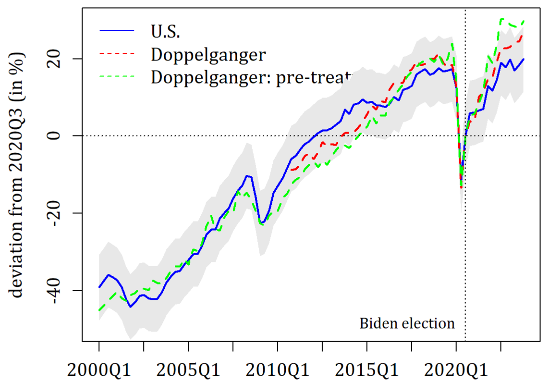
(B) Real private consumption



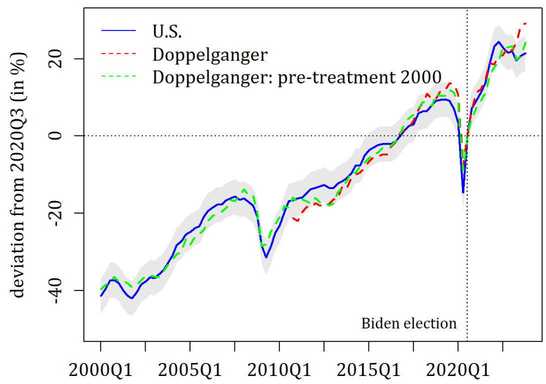
(C) Real government consumption



(D) Real investment



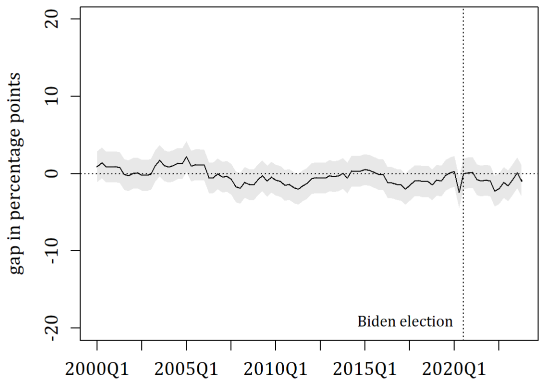
(E) Real exports



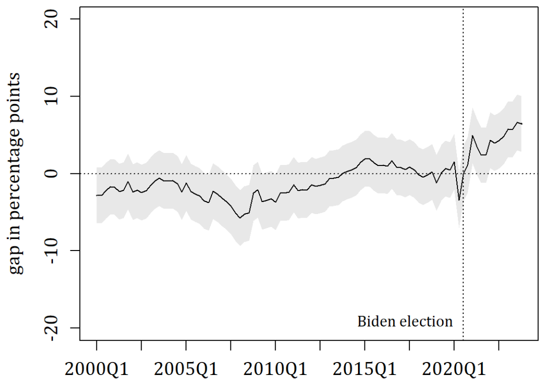
(F) Real imports

Note: Actual data (blue line), baseline doppelganger (red line), doppelganger based on longer pre-treatment data (green line). Shaded area are two standard deviations of difference prior to the election.

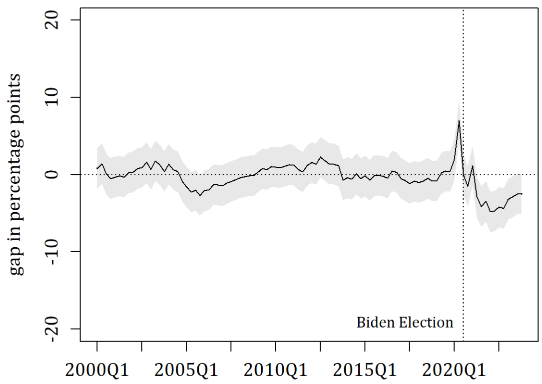
Figure A11: 2000Q1 pre-treatment period robustness: decomposition results (doppelganger gaps)



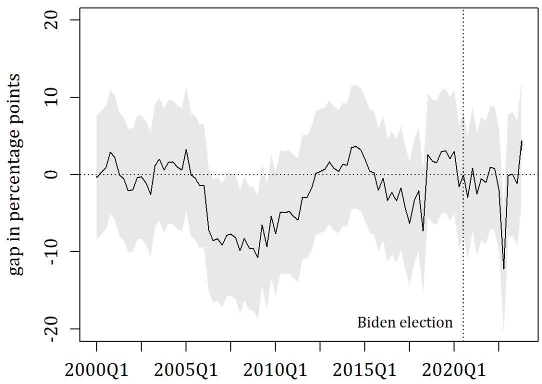
(A) Real GDP



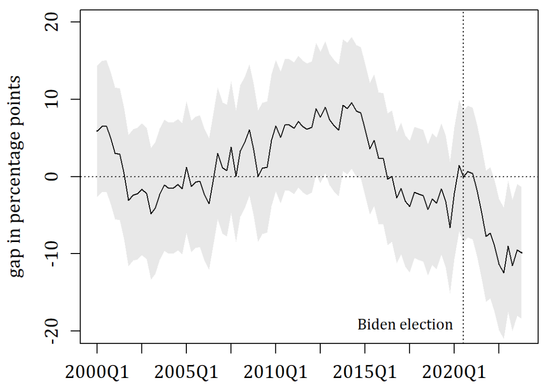
(B) Real private consumption



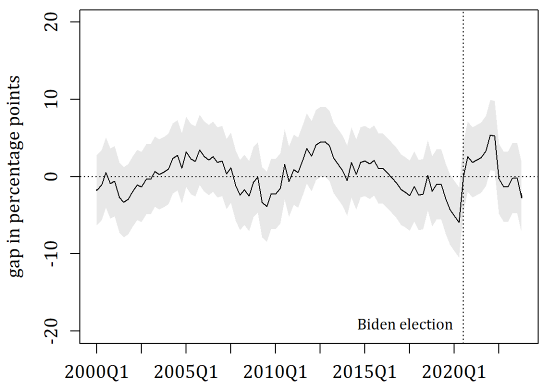
(C) Real government consumption



(D) Real investment



(E) Real exports



(F) Real imports

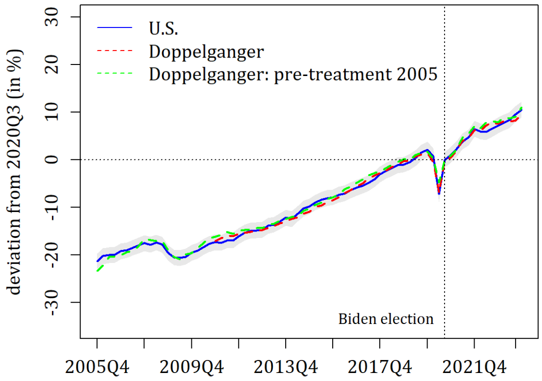
Note: Shaded area are two standard deviations of difference prior to the election.

Table A6: 2000Q1 pre-treatment period robustness: decomposition results (matching of covariates and doppelganger country weights)

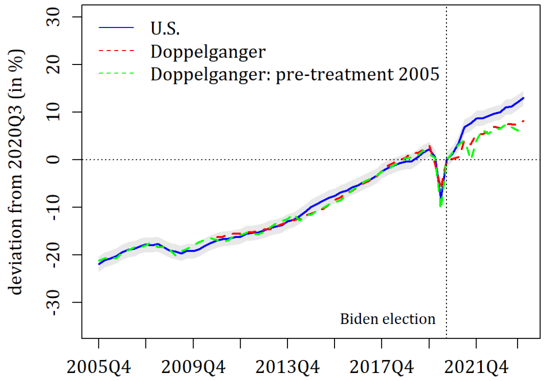
Country weights		Private consumption based	Investment based	Export based	Import based	Government consumption based
Australia		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Denmark		0.56	0.58	< 0.01	< 0.01	< 0.01
Finland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Germany		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland		0.09	0.05	0.13	0.03	0.01
Ireland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Italy		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Japan		< 0.01	< 0.01	< 0.01	0.33	< 0.01
Korea		0.21	0.27	0.15	0.19	0.16
Luxembourg		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Norway		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal		< 0.01	< 0.01	0.34	0.45	0.73
Slovak Republic		0.13	0.10	0.01	< 0.01	0.10
Spain		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Sweden		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom		< 0.01	< 0.01	0.37	< 0.01	< 0.01
Predictors	Actual U.S.					
Consumption share	67.00	49.90	49.50	60.50	58.90	61.80
Investment share	20.20	22.70	23.20	20.60	23.70	21.50
Exports share	10.70	15.70	13.10	27.50	16.30	34.50
Import share	13.90	15.40	12.70	28.50	17.70	37.10
Labor productivity growth	0.40	0.40	0.40	0.20	0.20	0.30
Employment share	63.10	63.20	63.10	62.60	62.10	58.40

Top part: Country weights for the baseline doppelgangers in the main text. *Bottom part:* Values shown for the baseline doppelgangers in the main text. All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.

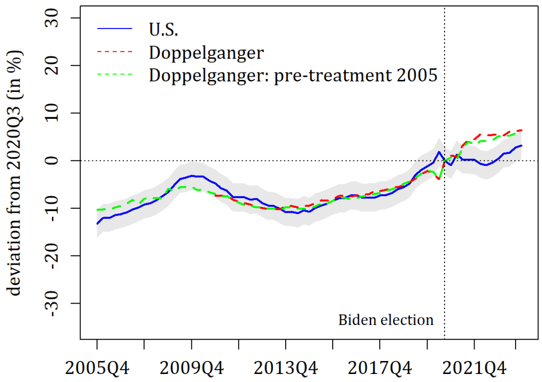
Figure A12: 2005Q1 pre-treatment period robustness: decomposition results (U.S. macroeconomic trajectories)



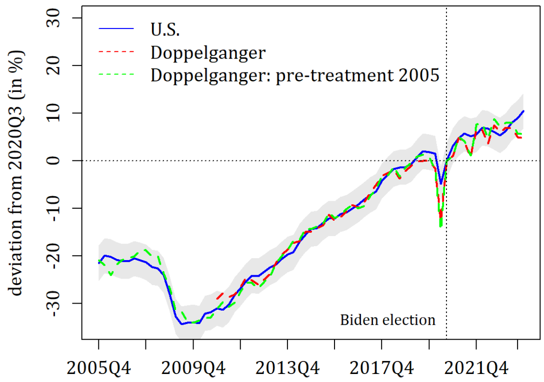
(A) Real GDP



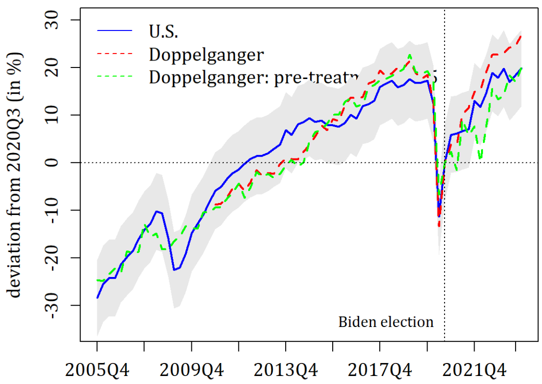
(B) Real private consumption



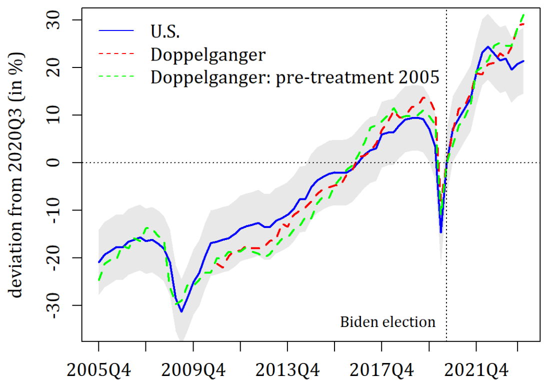
(C) Real government consumption



(D) Real investment



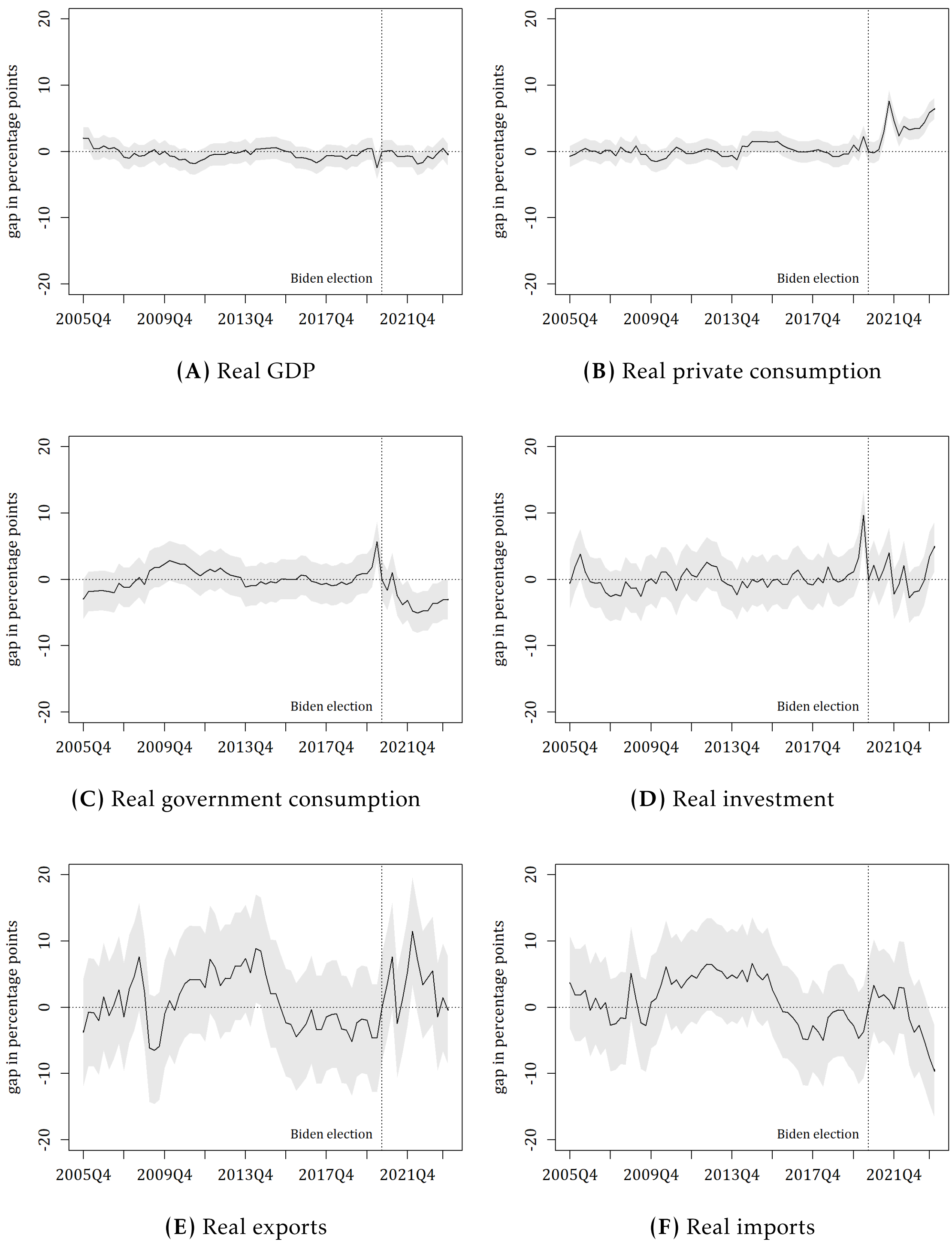
(E) Real exports



(F) Real imports

Note: Actual data (blue line), baseline doppelganger (red line), doppelganger based on longer pre-treatment data (green line). Shaded area are two standard deviations of difference prior to the election.

Figure A13: 2005Q1 pre-treatment period robustness: decomposition results (doppel-ganger gaps)



(A) Real GDP

(B) Real private consumption

(C) Real government consumption

(D) Real investment

(E) Real exports

(F) Real imports

Note: Shaded area are two standard deviations of difference prior to the election.

Table A7: 2005Q1 pre-treatment period robustness: decomposition results (matching of covariates and doppelganger country weights)

Country weights		Private consumption based	Investment based	Export based	Import based	Government consumption based
Australia		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Austria		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Canada		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Denmark		< 0.01	< 0.01	< 0.01	0.29	< 0.01
Finland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
France		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Germany		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Iceland		< 0.01	0.03	0.11	0.16	0.16
Ireland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Italy		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Japan		0.30	0.26	< 0.01	< 0.01	0.23
Korea		< 0.01	< 0.01	0.19	0.25	0.06
Luxembourg		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Netherlands		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
New Zealand		0.57	0.70	0.69	< 0.01	0.01
Norway		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Portugal		0.13	< 0.01	0.01	0.30	0.54
Slovak Republic		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Spain		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Sweden		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Switzerland		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
United Kingdom		< 0.01	< 0.01	< 0.01	< 0.01	< 0.01
Predictors	Actual U.S.					
Consumption share	67.30	59.30	58.60	57.10	53.90	59.80
Investment share	20.10	23.10	23.70	24.30	22.00	20.70
Exports share	11.40	17.20	15.10	15.00	14.40	21.60
Import share	14.40	16.90	14.40	14.30	14.70	22.70
Labor productivity growth	0.40	0.20	0.20	0.30	0.30	0.10
Employment share	62.30	67.40	69.00	68.20	62.70	62.50

Top part: Country weights for the baseline doppelgangers in the main text. *Bottom part:* Values shown for the baseline doppelgangers in the main text. All numbers are in percent. Labor productivity growth is the log difference between quarterly real GDP and quarterly total employment; employment share is the ratio between total employment and the working age population.