# A New Global Series of Corporate Tax Shocks*

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**Abstract**

We propose a methodology to identify tax reforms motivated by long-term objectives—economic growth, fiscal consolidation, and inequality reduction—and thus exogenous to the current economic conditions and persistent in nature. The method consists of a non-parametric filter of structural breaks that disentangles persistent from transitory tax shocks. We validate the filter by (i) using a statistical model of tax reforms calibrated to match empirical moments and (ii) by comparing the identified reforms with available narrative approaches. We apply our methodology to study reforms to the corporate income tax worldwide for the period 1960 and 2020. We document new global facts on corporate tax reforms and explore their short and long-run macroeconomic implications.

## 1 Introduction

What is the effect of permanent corporate tax reforms?[1] The study of long-run motivated corporate tax reforms faces two significant challenges. The first challenge is a lack of data: corporate tax reforms happen rarely. In the US, for example, only six changes to the corporate income tax rate occurred in the last 60 years. Thus data limitations preclude from establishing a systematic relationship when using information from a single country. The second challenge is the identification of fiscal shocks in general and corporate tax shocks in particular across a large panel of countries. Narrative methods, while effective, are time-intensive and difficult to maintain for a larger panel.

This paper proposes a new methodology that exploits cross-country data to circumvent these challenges. First, we significantly expand the number of observations by assembling a data set with 40 countries for the last 60 years of statutory corporate tax rates. Second, following Romer and Romer (2010), we decompose changes in the statutory tax rate between tax changes motivated by long-run considerations (e.g., sustainability, efficiency, or redistribution) from tax changes motivated by short-run objectives (e.g., business cycle stabilization) using non-parametric methods that identify multiple structural breaks in time-series. The premise is that tax changes with long-run objectives generate permanent changes in the statutory corporate tax rate. In this way, we tackle the omitted-variable bias by focusing on permanent corporate tax reforms—reforms with clear long-run motives, such as growth

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[1]This chapter represents joint work with Isaac Baley and Andrés Blanco

or fairness concerns, that remain in place for long periods of time, which ensures that the drivers of corporate tax reforms.

Our method consists of a non-parametric filter of structural breaks employed in the price and wage setting literature. Standard filtering techniques rely on the assumption that the underlying data generating process is normal. Corporate tax changes do not exhibit normality. Sometimes corporate tax changes are lumpy. Sometimes they are gradual. The variation in the underlying adjustment process renders these standard approaches unable to replicate important moments of the data generating process and to identify structural breaks in the data. To overcome this obstacle, we resort to the most appropriate alternative: non-parametric methods that identify multiple structural breaks in time-series (see, for instance, Carlstein (1988) and Bai and Perron (1998)). These methods allow us to identify the structural breaks in the underlying corporate tax code, without employing time-intensive narrative approaches.

This study of permanent corporate tax reforms proceeds in three steps. First, we calibrate a statistical model of tax reforms, using simulated method of moments estimator, to match the empirical moments observed in corporate tax regimes across countries. We show that our filter effectively identifies structural breaks with limited false positive and false negatives. Second, we study whether permanent corporate tax reforms occur randomly across different economic states and benchmark our results with available narrative approaches. We show that the permanent corporate tax reforms are not systematically related to other macroeconomic aggregates, and coincide with series obtained through narrative approaches by Romer and Romer (2010) and others.

Third, we use our new shock series to estimate the economic effect of permanent corporate tax reforms using local projections specification employed in Romer and Romer (2010). We find that a 1 pp permanent corporate tax cut has significant effects on output, consumption, investment, and consumer price inflation. A 1pp increase in the permanent corporate tax rate, output responds with a lag, rising above baseline one year after the reform, cresting at 10 bp after three years before returning to baseline five years after the reform. Consumption responds with a longer lag, rising above baseline after three years and cresting at a 10 bp before returning to baseline five years after the reform. Most notably, investment responding on impact, achieving a persistent 30 bp increase after five years. This persistent rise suggests that the economy's capital stock is adjusting to its new steady state level in accordance with the new user cost of capital implemented by the tax reform. Lastly, we show that inflation increases by 26bp on impact before returning to baseline in the next period.

We evaluate the robustness of our estimates in two steps. First, we show that our results are not sensitive to omitted variable bias by including controls in alternative specifications. Second, we examine whether economies respond in anticipation to permanent tax changes. As permanent, long-run motivated tax changes tend to be pre-announced outcomes of extended legislative processes, we should anticipate anticipatory effects to be present. As expected, we observe anticipation in output, consumption, investment, and inflation. Our baseline specification controls for these anticipatory effects through the inclusion of multiple lags of the treatment and outcome variables, following Romer and Romer (2010).

Our work contributes to two main strands of the literature. First, it contributes to the non-parametric filtering literature, following Carlstein (1988) and Bai and Perron (1998). Non-parametric has been used to identify structural shocks in underlying macroeconomic data for decades. Specifically, we contribute to a recent segment of the literature that applies non-parametric filters to study pricing and wage setting literature, as in Stevens (2019). This project is the first use of non-parametric filters to identify fiscal policy shocks to our knowledge.

Second, our work contributes to our understanding of the macroeconomic consequences of fiscal pol-

icy. Empirical research on the effect of fiscal policy shocks occurs along two methodological fronts. The first strand uses structural vector autoregressions (SVAR) to identify exogenous fiscal policy shocks, originating the pathbreaking work of Blanchard and Perotti (2002). These methods achieve identification through the use of zero or sign restrictions in the SVARs transition matrix, as in Blanchard and Perotti (2002) and Uhlig (2005), respectively. The second approach uses non-structural assumptions to identify fiscal policy shocks. Specifically, this policy contributes to the portion that leverages permanent tax changes to identify permanent shocks to fiscal policy without employing strong structural assumptions. The work of Romer and Romer (2010), Cloyne (2013), Hussain and Liu (2019), Gil *et al.* (2019), Pereira and Wemans (2015), use narrative approaches to identify long-run motivated reforms. This work builds on the original work of Ramey and Shapiro (1998), which uses narrative methods to identify exogenous shocks to military buildups. The empirical strength in narrative methods lies in their ability to identify the timing of announced fiscal policy shocks. Ramey (2011) shows that narrative approaches predict the structural shocks of SVAR estimators. Our work provides novel evidence that nonparametric methods coincide with these narrative methods and offers a new path for future work in the estimation of fiscal policy shocks across countries.

# 2 A New Measure of Permanent Corporate Tax Reforms

In this section we propose a new measure of permanent corporate tax reforms. First, we present our data. Second, we discuss the scientific motivation of our approach. Third, we implement our methodology.

## 2.1 Data Description

Our data consists of an annual unbalanced cross-country panel for 36 countries for the period 1960–2019. To assemble the cross-country panel on statutory corporate tax reforms, we gather several dispersed sources. Data on statutory corporate tax rates comes from Vegh and Vuletin (2015) for the corporate income and personal income taxes; depreciation allowances from the Centre for Business Taxation Database of Oxford University and its update by Asen and Bunn (2019); and capital gain taxes from Spengel, Endres, Finke and Heckemeyer (2014). See Appendix 5 for details.

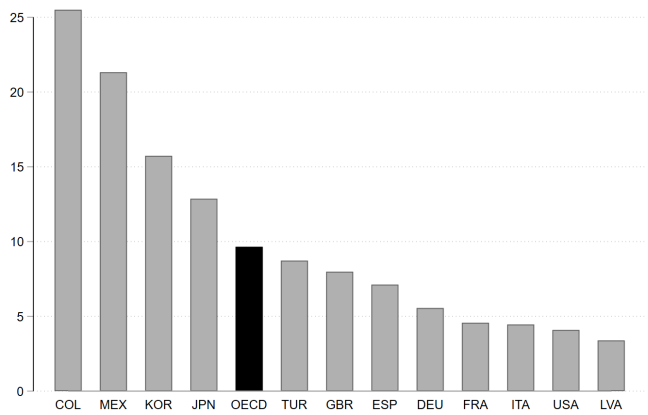
Among OECD countries, corporate income tax revenue in 2018 accounted for an average of 10% of total tax revenue, ranging from 3.4% in Latvia to 25% in Colombia (Panel A in Figure I). The importance of corporate taxation remains large, despite a generalized falling trend in tax rates over the last four decades (Panel B in Figure I); in particular, the median corporate income tax rate has decreased from 42% in 1980 to 25% in 2020. At the country level, corporate tax reforms happen infrequently and are very persistent. In the US, for instance, only two reforms in the corporate income tax rate have occurred in the last 40 years, in 1986 and 2018.

## 2.2 Conceptual Framework

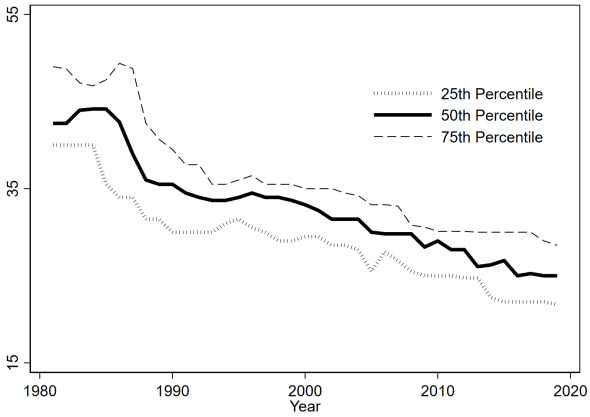
Through its narrative approach, Romer and Romer (2010) provide a framework to classify the motives behind tax changes, and in particular, corporate tax changes. They identify four broad categories of motivations for tax changes: (i) offsetting a change in government spending; (ii) offsetting some factor other than spending likely to affect output in the near future; (iii) dealing with an inherited budget deficit; and (iv) achieving some long-run goal, such as higher normal growth, increased fairness, or a smaller role for government.

**Figure I** – Corporate Taxes in OECD Countries

(A) Corporate Income Tax Revenue
(% of total tax revenue)




(B) Corporate Income Tax Rate
(%, highest marginal rate)




Source: OECD Revenue Statistics Database. Corporate income tax revenue includes corporate income tax and capital gains tax revenue. Data for the largest OECD countries in terms of GDP and the countries with the lowest and the highest value in the sample.

The first two categories are considered "endogenous", as the motivations are likely to correlate with developments affecting current output and inflation, critical variables for automatic stabilization policies. For instance, tax cuts designed to lift the economy from a recession or to finance a transitory increase in expenditure are endogenous to the state of the economy. The latter two categories—long-run fiscal sustainability and growth and redistribution—are considered "exogenous," because they are motivated either by past decisions or societal preferences, and thus are not systematically correlated with the current state of the economy. We take the stand that tax reforms in other countries can be similarly classified into these categories. Our aim is to isolate the exogenous tax changes purely motivated by long-run goals. Given their long-run motivations, we label these reforms as "permanent," as they are expected to remain in place for a long period of time.[2]

Let us illustrate the type of "exogenous and permanent" tax changes motivated by long-run goals with examples from three countries. Figure II plots the corporate income tax statutory top marginal rate for the US, Chile, and Germany between 1960 and 2020. One common observation across the three time series is the lumpy nature of tax changes—tax rates remain fixed for long periods, which are then followed by large changes.

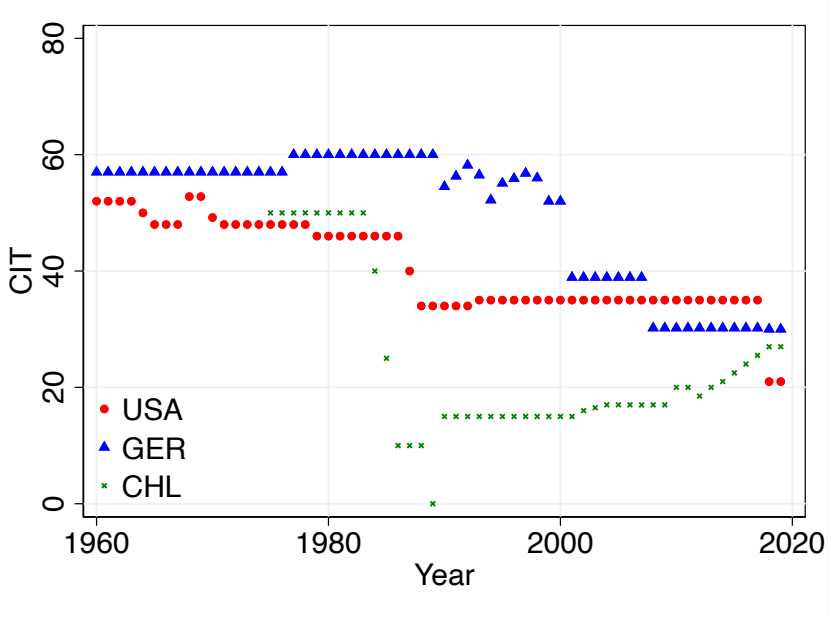
Focusing on the US (red dotted series), there are several adjacent periods of particular interest. In 1964, the Kennedy Administration lowered corporate tax rates with the *Tax Reduction Act*.[3] The motivation for this tax decrease was to improve long-run growth, and thus we consider it as exogenous and permanent. Followed by this tax cut, the tax rate increased in 1968 and 1969 throught the *Revenue and Expenditure Control Act* to finance government spending channeled to the Vietnam War, a purely transitory and endogenous motivation. With the end of the war, the tax rate reverts back to its pre-1968 level and remains in place for the next 8 years. The *Tax Revenue Act of 1978* and *Tax Reform Act of 1986* lowered again the corporate income tax, with its main objective to generate long-run growth.[4]

[2] As in Romer and Romer (2010), we stress that we do not use the word "exogenous" in its strict econometric sense, but as a stance for "valid" in our analysis. We believe that our classification into "permanent" and "transitory" tax changes are, in fact, more appropriate.

[3] Annual Budget Message to the Congress, Fiscal Year 1965. Source: Romer and Romer (2010).

[4] As Romer and Romer (2010) analized; for the the *Tax Revenue Act of 1978* "There is no evidence that the Ways and Means Commitee felt that a recession was in the offing; it merely felt that growth would fall from it very high levels in 1976

**Figure II** – Corporate Income Tax Rate




Notes: Top statutory marginal corporate income tax rate in US, Germany and Chile between 1960 and 2020. See Appendix 5 for details.
Source: Vegh and Vuletin (2015)

These episodes clearly highlight the aim of our strategy: eliminate the contamination induced by the tax increase for war-financing from the long-run perspective of the Kennedy tax cut.

Following the students' protests in 2011–2012, on September of 2014 the Chilean Congress passed the Law No. 20780 that increases the corporate income tax rate from 20% to 27%.[5] The objective of this reform was to finance a variety of social programs, including an educational reform and to help close the income gap, the largest in Latin America. Following Romer and Romer (2010) classification, the main objective was to achieve fairness to higher expenditure in social programs.

## 2.3 Strategy

Our objective is to decompose tax changes into two categories: 1) tax changes motivated by long-run objectives, such as sustainability of public finances, preferences for redistribution and fairness, and efficiency considerations; 2) tax changes motivated by short-run objectives. The premise is that tax changes with long-run objectives are uncorrelated to the economic drivers of monetary policy, namely, output and price stabilization. The main identifying assumption is that corporate tax changes with long-run objectives generate permanent changes in the statutory corporate tax rate. Therefore, the problem reduces to disentangle permanent from transitory shocks to the time series of corporate tax rates at the country level.

Standard filtering techniques similar to the Kalman filter heavily rely on normality assumptions for the stochastic process that is being filtered. The lumpy nature of adjustments to the statutory corporate tax rates, as illustrated in Figure II, is far from normal. While the Kalman filter provides the

and 1977 to more normal levels;" and for the *Tax Reform Act of 1986*, they say "Because the act was motivated by a desire to make the tax system fairer, simpler, and more conducive to long-run growth, and not by a desire to return growth to normal, we classify it as an exogenous, long-run action".

[5] See Kinghorn (2016) for details.

optimal linear projection by minimizing mean squared error, we employ a non-parametric approach to better capture the higher-order moments of the two distributions of interest: the size and duration of statutory corporate tax rate adjustments. As the most immediate and appropriate alternative, we resort to non-parametric methods that identify multiple structural breaks in time-series (see, for instance, Carlstein, 1988; Bai and Perron, 1998). In a nutshell, the idea is to split the time series into two contiguous subsamples and, using an appropriate measure of distance, test whether those subsamples were drawn from different distributions. A drawback from using these methods is that they require specifying a threshold $\mathcal{K}$ to determine whether differences in the subsamples are large enough to reject the null hypothesis of no break in the series.

Following recent applications of non-parametric filters to study pricing and wage setting literature (see, for instance, Stevens, 2019; Blanco *et al.*, 2021), we develop and implement a "break test" that determines the value of the parameter $\mathcal{K}$ via a cross-validation exercise based on the estimation and simulation of a statistical model of the underlying time series. In our case, we write and estimate a model of permanent and transitory corporate tax changes that replicates salient features of the average dynamics of corporate taxes worldwide.[6] Using the estimated model, we calibrate the threshold $\mathcal{K}$ to match the frequency of permanent tax reforms in the model and the filtered series.

Besides determining the value of the distance thresholds, this method allows us to validate the structural breaks by applying the filter to model-simulated data and to assess the magnitude of type I errors (no reforms when there is a reform) and type II errors (reforms when there is no reform).[7] Next, we describe each step in more detail.

**Step 1. A model of permanent and transitory corporate tax reforms.** There are three empirical properties of statutory corporate income tax rates: (i) permanent changes are infrequent, and conditional on changing, their growth rate tends to be negative and highly dispersed; (ii) transitory deviations from the mode within a rolling windows tend to be persistent, (iii) some reforms tend to be gradual.[8] We set up a statical model to capture these properties.

Let $\tau_{t,i}$ be the tax rate in year $t$ and country $i$. The tax rate is jointly determined by the sum of a permanent component $X^P_{t,i}$ and a transitory component $X^T_{t,i}$ in the following way:

$$\tau_{t,i} \;=\; \frac{1}{1+e^{X^P_{t,i}+X^T_{t,i}}} \;\in\; [0,1]. \tag{1}$$

The permanent component follows a markov process

$$X^P_{t,i} \;=\; \begin{cases} X^P_{t-1,i} & \text{with probability } 1-\lambda_R \\ X^P_{t-1,i} \;+\; R_{t,i} & \text{with probability } \lambda_R \end{cases}, \tag{2}$$

with initial condition $X_0 \sim_{i.i.d.} \mathcal{N}(\mu_0, \sigma_0)$ and shocks $R_t \sim_{i.i.d.} \mathcal{N}(\mu_R, \sigma_R)$. The distribution of initial conditions reflect cross-country differences in the level of the corporate income tax rate at the beginning of the sample. The shocks $R_t$ represent tax reforms, which have a drift $\mu_R$ and dispersion $\sigma_R$. The parameter $\lambda_R$ reflects the frequency at which reforms occur.

The transitory component is described by a discrete state space $S = \{1, 2\}$ with transition proba-

[6] Our framework builds on Galí (1999), who writes a model of permanent and transitory shocks to real output to disentangle productivity shocks from demand shocks.

[7] See Online Appendix Section B.5 in Blanco *et al.* (2021) for further details on the design of the break test.

[8] This framework is similar to regime switching models (see, for instance, Carlstein, 1988; Bai and Perron, 1998)

**Table I** – A Model of Corporate Tax Reforms: Targets and Estimation

| **Target moments** | **Data** | **Model** |
|---|---|---|
| Average CIT in 1960 | 0.388 | 0.416 |
| Dispersion of CIT in 1960 | 0.141 | 0.149 |
| Average $\Delta CIT_{t+4}$ | -0.028 | -0.033 |
| Std($\Delta CIT_{t+1}$) | 0.048 | 0.031 |
| Std($\Delta CIT_{t+4}$) | 0.066 | 0.044 |
| Frequency($\Delta CIT_{t+1}$) | 0.774 | 0.716 |
| Frequency($\Delta CIT_{t+4}$) | 0.456 | 0.556 |
| Prob. change in $t+1$\|change in $t$ | 0.833 | 0.880 |
| **Parameter** | **Symbol** | **Estimate** |
| Average and dispersion of initial CIT distribution | $(\mu_0, \sigma_0)$ | $(0.402, 0.487)$ |
| Average and dispersion of permanent reform | $(\mu_R, \sigma_R)$ | $(0.393, 0.076)$ |
| Arrival rate of permanent reform | $\lambda_R$ | 0.054 |
| Dispersion of transitory tax changes | $\sigma_T$ | 0.049 |
| Persistence of transitory state | $(q_{11}, q_{22})$ | $(0.898, 0.485)$ |

Notes: The table presents moments used in and parameter estimates from the SMM estimation. $\Delta\tau_{t+h} \equiv \log(\tau^{CIT}_{t+h,i}/\tau^{CIT}_{ti,i})$ denotes statutory corporate income tax changes. The first block of rows (i.e., rows 1 to 8) describes the corporate tax moments in the data and in the model. The second block of rows (i.e., rows 9 to 13) describes the estimated parameters.
Source: Vegh and Vuletin (2015) and simulations.

bility $Q^S = [q_{11}, q_{12}; q_{21}, q_{22}]$. Given the realization of the state, the transitory component is

$$X^T_t = \begin{cases} 0 & \text{if } S = 1 \\ \eta_t & \text{if } S = 2 \end{cases}. \tag{3}$$

If $S = 1$, then there are no transitory shocks. If $S = 2$, then there are Gaussian transitory shocks $\eta_t \sim_{i.i.d.} \mathcal{N}(0, \sigma_T)$. Observe that, if $q_{22}$ is sufficiently high, then a transitory deviation beget another transitory deviation. Together, the specifications for the permanent and the transitory components imply that a markovian stochastic process in the state $(S, X^P)$.

We estimate the parameters for the model. We target a set of empirical moments and estimate the parameters using an SMM procedure. We aim to capture relevant moments in corporate taxes worldwide that identify each parameter in our model. Concretely, we discipline the mean and dispersion of the permanent component initial condition $(\mu_0, \sigma_0)$ by targeting the mean and dispersion of CIT in 1960. We discipline the probability of transitory and permanent changes together with their size $(\mu_R, \sigma_R, \sigma_T, \lambda_R, q_{11})$ by targeting the frequency and size of CIT changes at the different horizons. Finally, we discipline the persistence of having transitory changes $(q_{22})$ by targeting the probability of a CIT change in the next period given a change in the current period. Table I shows the targeted moments in the data and the estimated parameters. As we can see in the table, the model is able to capture the empirical properties (i)-(iii) in corporate taxes.

**Step 2. Application of non-parametric method to identify breaks to model and data.** We calibrate $\mathcal{K} = 0.5$ to match the frequency of reforms in the simulated data of our model and their filtered permanent component. We also compare our results with the Kehoe and Midrigan

(2015) filter that computes the mode of the series in a rolling window. Table II shows selected moments of the simulated permanent component of the model and their filtered version with the Break-Test and the Kehoe-Midrigan methods.

**Table II** – Moments in the Model and Filtered Moments

| **Moment** | **Model** | **Break-Test** | **Kehoe-Midrigan** |
|---|---|---|---|
| Frequency reforms | 0.052 | 0.052 | 0.041 |
| Type I error for no reforms | | | |
| Reform $t$ given no reform $t$ | | 0.009 | 0.0112 |
| and $T-3>t>3$ | | 0.009 | 0.0112 |
| Type II error for no reforms | | | |
| No reform $t$ given reform $t$ | | 0.160 | 0.396 |
| and $T-3>t>3$ | | 0.110 | 0.338 |
| and no reform in $t-1$, $t$, $t+1$ | | 0.023 | 0.119 |
| Reforms % change | | | |
| Mean | -30.797 | -27.724 | -33.029 |
| Std | 10.819 | 14.720 | 14.961 |
| P10 | -42.123 | -42.328 | -49.799 |
| P50 | -29.381 | -28.1362 | -29.978 |
| P75 | -23.893 | -21.114 | -24.198 |
| P90 | -19.430 | -4.857 | -19.501 |

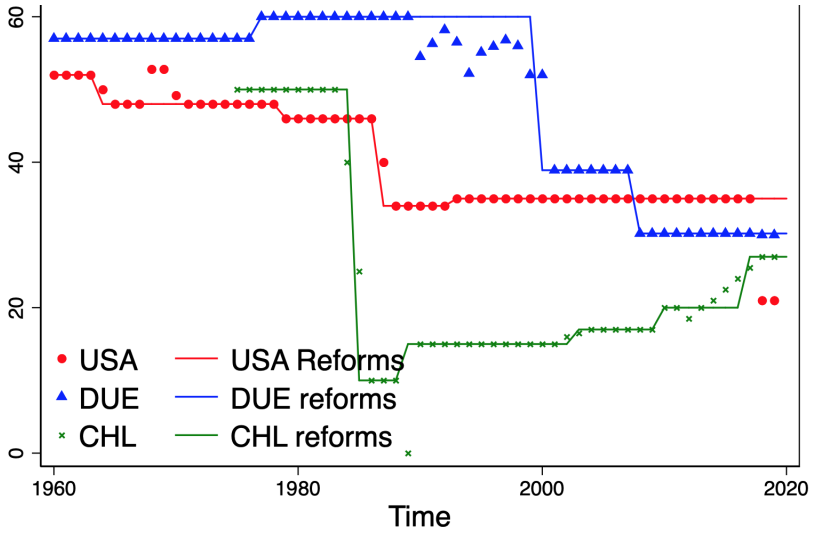
Notes: The table presents moments in the simulated permanent component and its filtered version. The first, second and third columns describe the moments in simulated data of the model, in the filtered series in the mdoel with the break test, and the filtered series in the model the Kehoe-Midrigan method. We use the $\mathcal{K}=0.50$ for the Break Test method and $\mathcal{L}_{KM}=3$, $\mathcal{C}_{KM}=0.3$, and $\mathcal{A}_{KM}=0.5$ for the Kehoe and Midrigan (2015) (see Online Appendix 6 for a description of the method). The first row describes the frequency of reforms, the second block (i.e., rows 2 to 6) describes the type I and type II errors of the null hypothesis of no break, and the last block (i.e., rows 7 to 11) describes growth rate moments of reforms.
Source: Model's simulation.

By construction, the filtered series with the break test matches the frequency of reforms. It is a result that the errors in the estimated reforms are with low probability. There could be two errors in the method. First, the method could identify a reform given that there wasn't a reform. The probability of this event is 0.009. This result is not surprising since the probability of reform is low, i.e., 0.05. Second, the method could identify not identify reform given that there was a reform. The probability of this event is 0.16. Since the method uses past and future information to identify a reform, most of these errors are at the end and beginning of the sample. For that reason, the probability drops to 0.11 once we compute the probability of no reform in $t$ given a reform in $t$. Finally, while transitory shocks preclude the method from identifying the precise date of the reform, the method is capable of identifying reforms in a rolling window of one year. To see this property, observe that the probability of identifying no reform in $t-1$, $t$, and $t+1$ given a reform in $t$ is 0.023.

The method is not perfect, and therefore, there would be measurement errors in the estimated changes. As Table II shows, the break test method tends to identify small reforms when there are not. Nevertheless, the method provides a good estimation of the reforms below the 75th percentile of growth rates in corporate taxes.

Figure III shows the same corporate tax raw series (dots) as Figure II and the filtered permanent reforms series (solid lines). In Appendix 5, Figures XVIII and show these series for each country in the sample.

**Figure III** – Filtered Corporate Income Tax Reforms




Notes: Raw and filtered top statutory marginal corporate income tax rate in US, Germany and Chile between 1960 and 2020. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

## 2.4 New Series of Corporate Tax Reforms

Let us describe the statistical properties of our new series. Table III presents the summary statistics of our new measure against the unfiltered corporate income tax changes in our sample.

**Table III** – Statistics of Permanent Corporate Tax Reforms

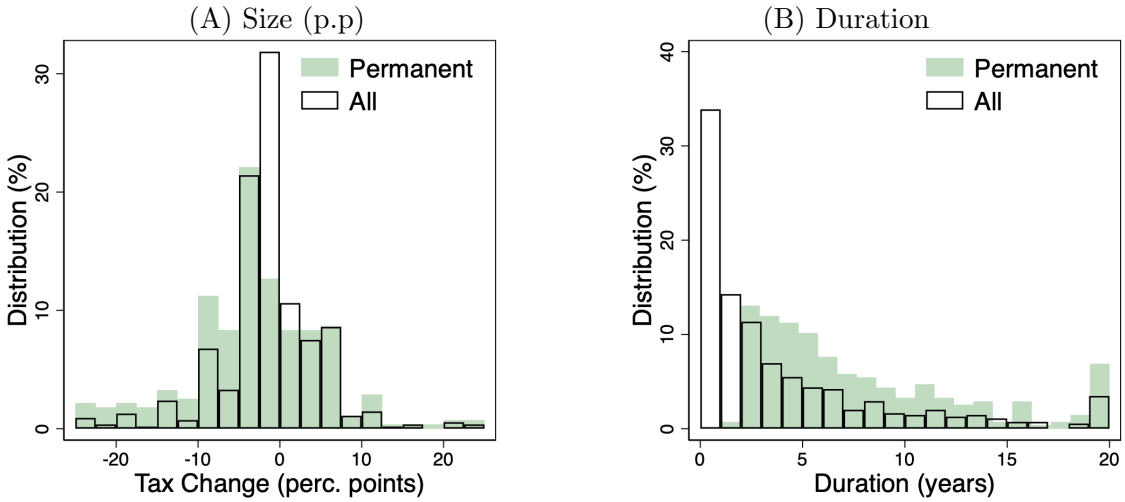
| | All Tax Changes | Permanent |
|---|---|---|
| Size (p.p.) | −1.95 | −3.51 |
| | (6.41) | (8.81) |
| Duration (years) | 3.80 | 7.93 |
| | (5.46) | (6.10) |
| *N* | 546 | 276 |

Notes: Authors' calculations using filtered series of permanent tax reforms. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

From our sample of 546 tax changes, we recover 276 permanent tax reforms, which vary in size and duration. As expected, the average permanent tax reform and the average tax change in our sample are both negative. The average permanent tax reform is of greater magnitude than the average tax change. The average permanent corporate tax reform consists of a rate decrease of 3.51 pp. and occurs almost 8 years after the previous tax reform. This duration spans beyond most expansionary and contractionary periods experienced in wealthier economies. To learn more about which tax changes our filter labels as permanent, Figure IV plots the size and duration distributions of our sample. The distribution of all tax changes is in white, while the distribution of permanent tax reforms is in green.

Panel A plots the histogram of the size of permanent tax reforms against all tax changes. Remarkably, the two size histograms overlap significantly, with the permanent reform distribution being towards the left of the distribution. Moreover, some permanent reforms are close to zero in magnitude,

**Figure IV** – Size Distribution of Corporate Income Tax Reforms




Notes: Distribution of filtered corporate tax reforms. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

highlighting an interesting lesson from our filtering approach. Some permanent tax changes are small and positive, but most are negative. This reflects the descriptive evidence on the long-run behavior of the corporate tax rate presented earlier in this section. Panel B plots the histogram of the duration of permanent tax reforms against all tax changes. The duration histogram of permanent tax changes falls to the right of the duration histogram of all tax changes in the sample. Our filter identifies vanishingly few permanent reforms that last fewer than three years. This result will help us interpret our empirical findings presented later. That said, the size and duration of permanent reforms has not remained constant over time. Table IV reports how the number, size, and duration of permanent tax reforms have evolved over the preceding decades.

**Table IV** – Duration of Corporate Income Tax Reforms

| | 1960 | 1970 | 1980 | 1990 | 2000 | 2010 | All |
|---|---|---|---|---|---|---|---|
| Size (p.p.) | 2.38 | 3.79 | −4.34 | −4.68 | −5.93 | −3.2 | −3.51 |
| Duration (years) | 7.47 | 8.82 | 6.95 | 10.11 | 7.64 | 4.84 | 7.93 |
| $N$ | 15 | 28 | 44 | 76 | 74 | 39 | 276 |

Notes: Distribution of filtered corporate tax reforms. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

Notice that average permanent reform size and magnitude varies non-monotonically across decades. From 1960 to 1970, the average reform size is positive: 2.38pp to 379pp, respectively. In the following three decades, the average size is negative and decreasing, from -4.34 in the 1980s to -5.93 in the 2000s. The 2010s saw a moderation of the size of tax cuts, but the reforms remained negative. The 2010s are also notable for the relative length of their tax reforms, although this is partially due to the sample size being limited to the period prior to the COVID-19 pandemic. The two decades with the most reforms are the 1990s and 2000s, and the durations of these reforms are high. As the size and duration of these reforms are not randomly distributed across time, we now evaluate whether they are randomly distributed across economic states.

## 2.5 Exogeneity of Permanent Tax Reforms

Before we use our measure, we document the relationship between permanent tax reforms are systematically related to other macroeconomic aggregates. To do this, we examine whether permanent tax reforms occur more frequently in certain macroeconomic environments. Then, we evaluate whether permanent tax reforms identified by our measure are consistent with "exogenous" tax changes identified by narrative approaches in the literature.

**Are permanent tax reforms random?** To test whether permanent tax reforms occur more frequently in certain economic states, we test the null hypothesis of "balance" across treatment (reform) and control groups. Figure V shows that we fail to reject the null of equality for contemporaneous variables and their lagged averages in the years preceding positive and negative permanent tax reforms. This suggests that covariates are balanced across treatment and control groups. In a randomized control trial, this result implies that when we compare outcomes across the treated and control groups, the estimates are not biased by differences in observable characteristics of the participants. In the context of this project, failing to reject the null suggests that the radical tax reforms identified by our measure are not systematically related with cyclical aggregates.

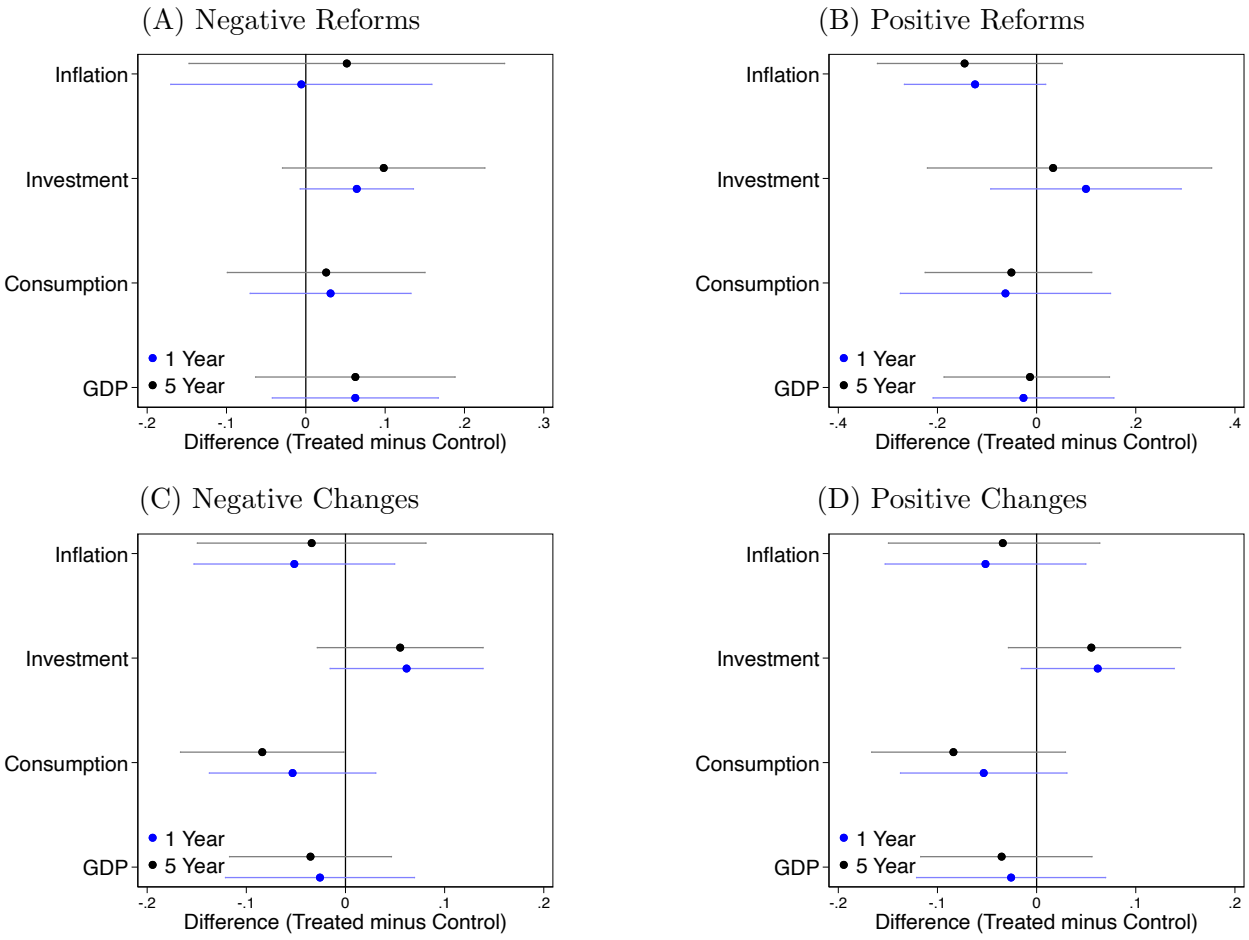
Figure V shows that we again fail to reject the null of equality for the contemporaneous and five-year lagged average variables across the permanent tax reforms in our sample. This suggests that covariates are balanced across treatment and control groups, such that our nonparametric instrument captures tax reforms across an unbiased distribution of economic climates. In general, corporate tax changes occur more frequently in periods of persistently low consumption growth

## 2.6 Comparison with Narrative Measures

We begin comparing our series of corporate tax reforms in the US with those obtained with the narrative approach in Romer and Romer (2010). This alternative instrument measures the magnitude of tax reforms by changes in aggregate tax liabilities. From their narrative analysis, we isolate those changes related to long-run motivation and only focus on corporate tax reforms. They identify three exogenous corporate income tax changes motivated ostensibly by long-run preferences: Revenue Act of 1964, Revenue Act of 1978, Tax Reform Act of 1986. Our measure also includes the Omnibus Budget Reconciliation Act of 1993, which previous narrative analysis categorized as exogenous and deficit motivated. This result is not concerning, since policymakers largely intended for the bill's tax changes to be permanent and drew motivation from outside of the business cycle. The figure plots the change in tax liabilities in billions. The variation in liabilities due to corporate income tax changes comes from Romer and Romer (2010) appendix.

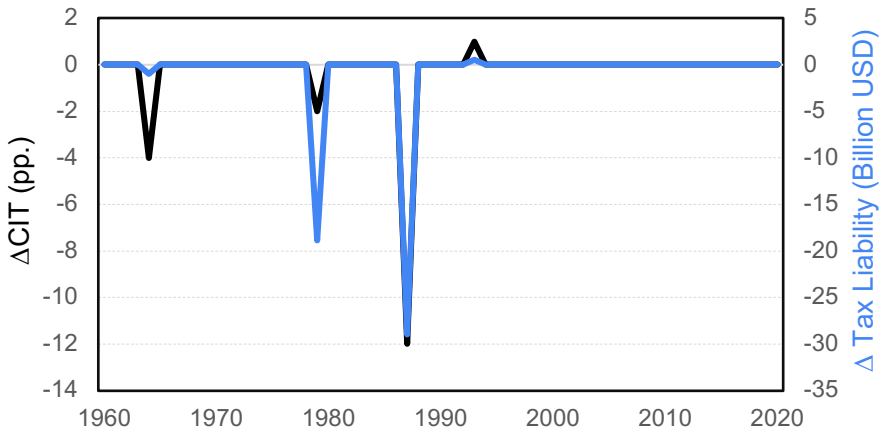
Figure VI plots our baseline series against the current value of the change in liabilities of the three permanent negative changes. Our baseline tax reform measure includes all four exogenous reforms found in the narrative measure. Moreover, records show that policymakers intended for all of these tax changes to be permanent. In addition to this heuristic test, we also check for covariate balance of negative exogenous changes in US tax liabilities using the Romer and Romer (2010) instrument to benchmark our results. Table VII shows that we again fail to reject the null of equality for the contemporaneous and five-year lagged average variables. This suggests that covariates are balanced across treatment and control groups, such that the passage of permanent corporate tax reforms is not correlated with certain economic states. Appendix 6 presents the balance table plots of narrative and nonparametric, negative corporate tax reforms for Portugal (Pereira and Wemans, 2015), Spain (Gil *et al.*, 2019), Canada (Hussain and Liu, 2019), and the UK (Cloyne, 2013).

**Figure V** – Covariate Balance across Treatment and Control Groups



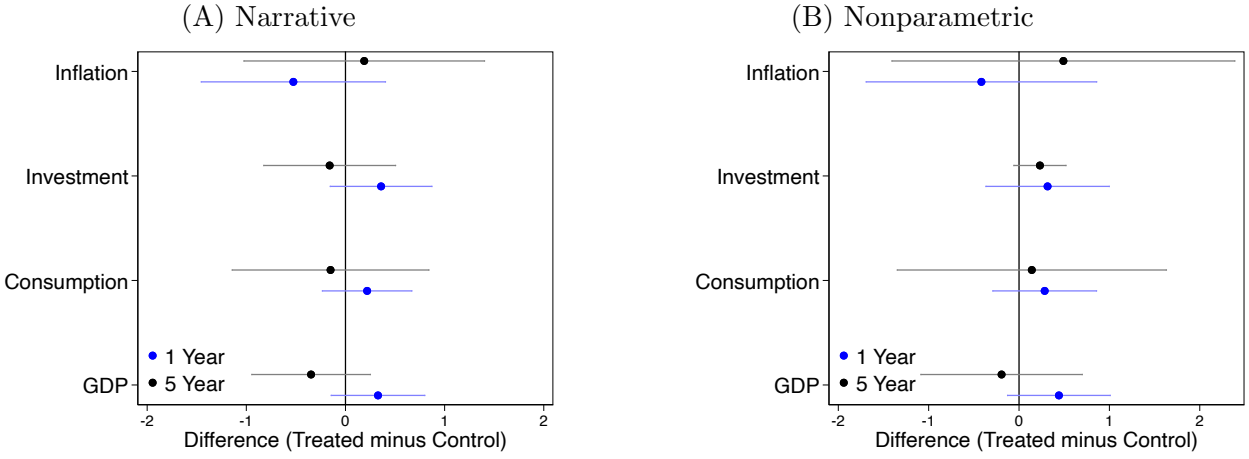

Notes: The table shows balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

**Figure VI** – U.S.A. Tax Reforms: Nonparametric and Narrative Approaches




Notes: See Appendix 5 for details.
Source: Romer and Romer (2010) and authors' calculations.

**Figure VII** – Covariate Balance Across tax Reforms, USA




Notes: The table shows balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. As narrative and nonparametric methods identified only one positive permanent corporate tax reform for the USA, we only report the balance plots for negative reforms Source: Authors' calculations. See Appendix 5 for details.
Source: Romer and Romer (2010) and authors' calculations.

# 3 Macroeconomics Effects of Corporate Tax Reforms

We now use our new tax reform instrument to examine the relationship between permanent tax reforms and macroeconomic aggregates. First, we estimate our baseline specification using local projection (LP) specification employed by Romer and Romer (2010). Second, we introduce controls into our regression to study the whether the our initial estimates suffer from omitted variable bias. Third, we decrease the number of lags specified in our local projection framework to evaluate the anticipatory economic response to tax reforms. Throughout this section, we compare the effect of permanent corporate tax reforms to the effect of an average tax reform.

## 3.1 Baseline Specification

To investigate the response of the monetary policy rate and other macro variables, we use the local projection specification employed by Romer and Romer (2010).For each dependent variable $y_{it}$ in country $i$, year $t$, and horizon $h$, we run the following baseline specification:

$$\Delta y_{i,t+h} \;=\; \alpha_i^h \;+\; \gamma t \;+\; \sum_{j=0}^{M} \beta_j^h \Delta T_{i,t-j} \;+\; \sum_{j=0}^{N} \gamma_j^h \Delta y_{i,t-j} \;+\; \varepsilon_{i,t+h}, \tag{4}$$

where $\Delta y_{i,t+h} \equiv y_{i,t+h} - y_{i,t}$, $\alpha_i^h$ is a set of country fixed-effects that controls for idiosyncratic trends; $\gamma_t^h = \gamma t$ is a time fixed effect that control for global trends; $\Delta T_{i,t-j}$ is a change in the permanent tax rate, and $\varepsilon_{i,t+h}$ is a mean-zero error term with $\mathbb{E}[\varepsilon_{i,t+h}, \varepsilon_{j,t+k}] = 0$ for all $(i, j, h)$. The coefficient of interest is $\beta^h$ that measures the response of variable of interest $h$ periods ahead. Standard errors are clustered at the country level.

We estimate the effect of permanent corporate tax reforms on output, consumption, investment, and inflation. Figure VIII plots the percentage point responses of the outcome variables. As the majority of permanent tax reforms are negative, we report $-\beta_1^h$, which is the coefficient multiplied by negative one.

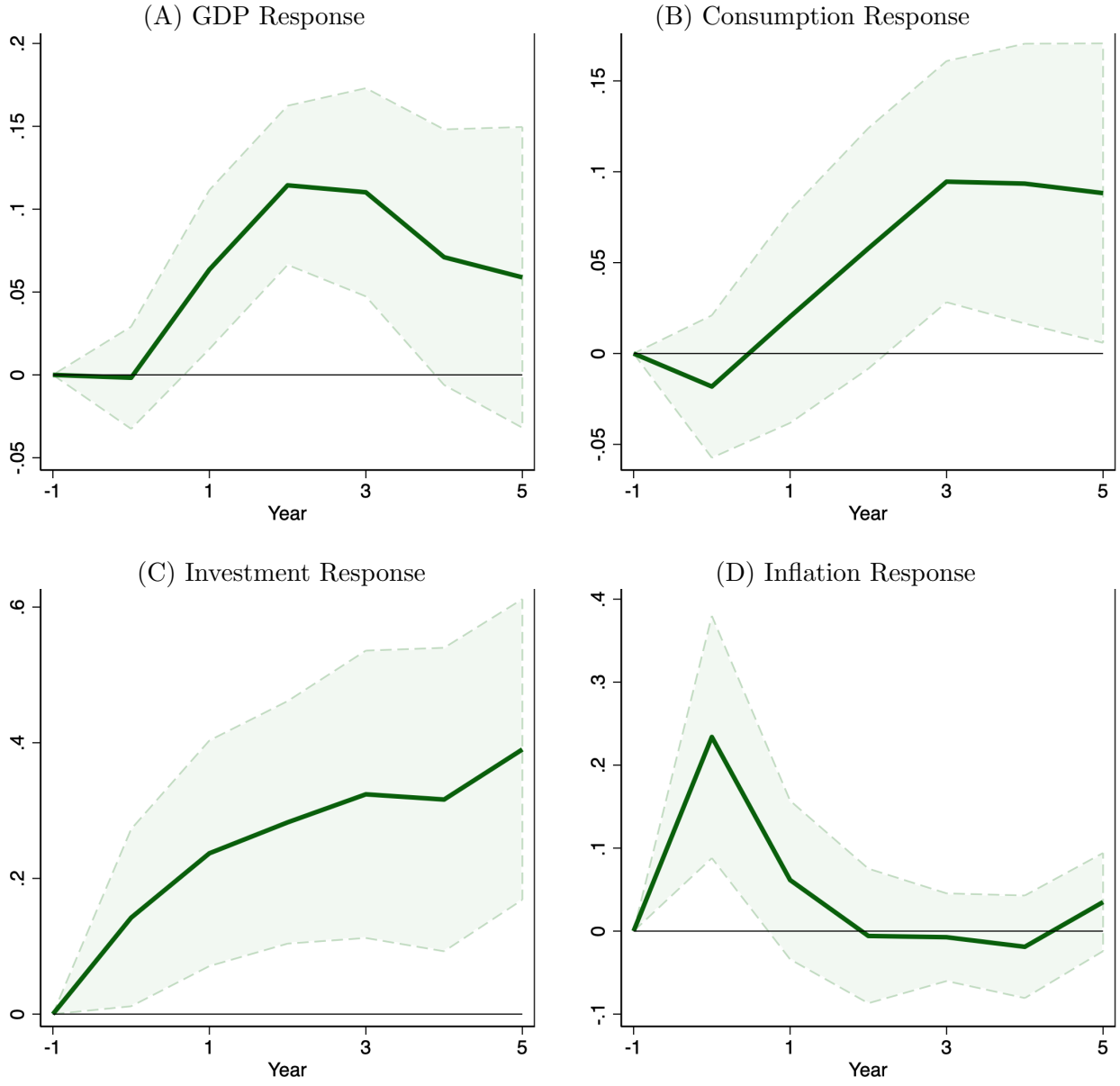
Figure VIII presents our main result from our simple regression without controls. It plots the coefficient $\beta_1^h$ for different horizons $h$ together with 90% confidence intervals. Panel (A) shows output responding to a permanent tax reform with a lag, rising above baseline one year after the reform, and by 10 bp following a 1pp. decrease in the permanent corporate tax rate. Panel (B) shows consumption responding with a longer lag, rising above baseline after three years, cresting at a 10 bp increase following a 1pp before returning to baseline five years after the reform. decrease in the permanent rate.

Panel (C) shows investment responding on impact, achieving a 40 bp increase following a 1pp. decrease in the permanent rate. This persistent rise suggests that the economy's capital stock is adjusting to its new steady state level in accordance with the new user cost of capital implemented by the tax reform. Panel (D) shows inflation responding on impact, achieving a 20 bp increase when a 1pp. decrease in the permanent rate hits the economy, before swiftly returning to baseline.

Figure IX presents estimates from our simple regressions without controls for all corporate tax changes, and Figure X presents estimates of the effect of temporary tax changes without controls.

As temporary tax changes are more likely to be correlated with macroeconomic aggregates and expectations, we should expect the effect of an arbitrary corporate tax to be attenuated relative to permanent corporate tax reforms. Relative to the effect of permanent corporate tax reforms, an arbitrary corporate tax change has an attenuated effect on output, consumption, investment, and inflation. Temporary corporate tax reforms do not have a significant effect on output or inflation.

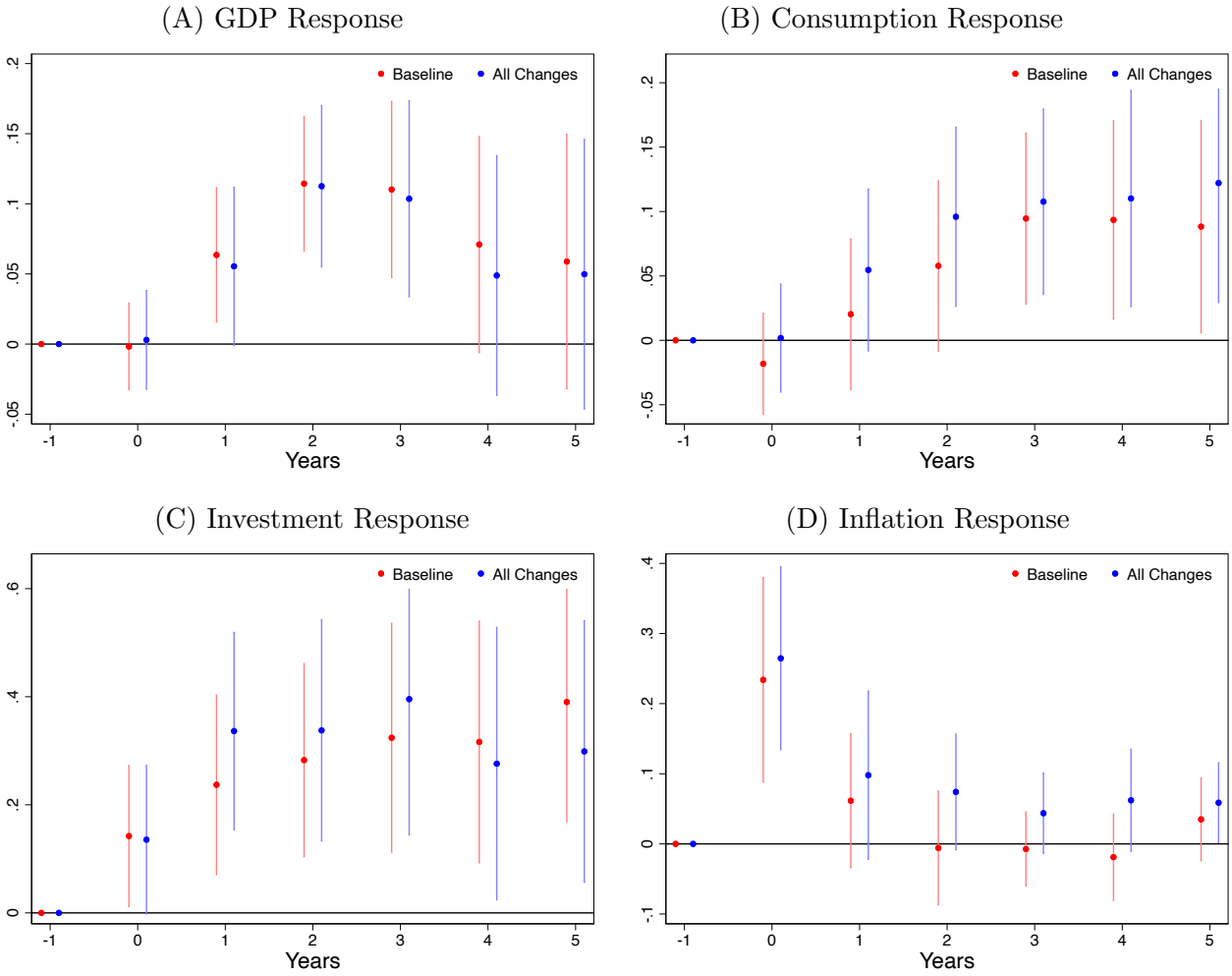
**Figure VIII** – Response of Macro Aggregates to Corporate Income Tax Reform




Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cut. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms. Panel (B) considers consumption response to permanent corporate tax reforms. Panel (C) considers investment response to permanent corporate tax reforms. Panel (D) considers the inflation response to permanent corporate tax reforms. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

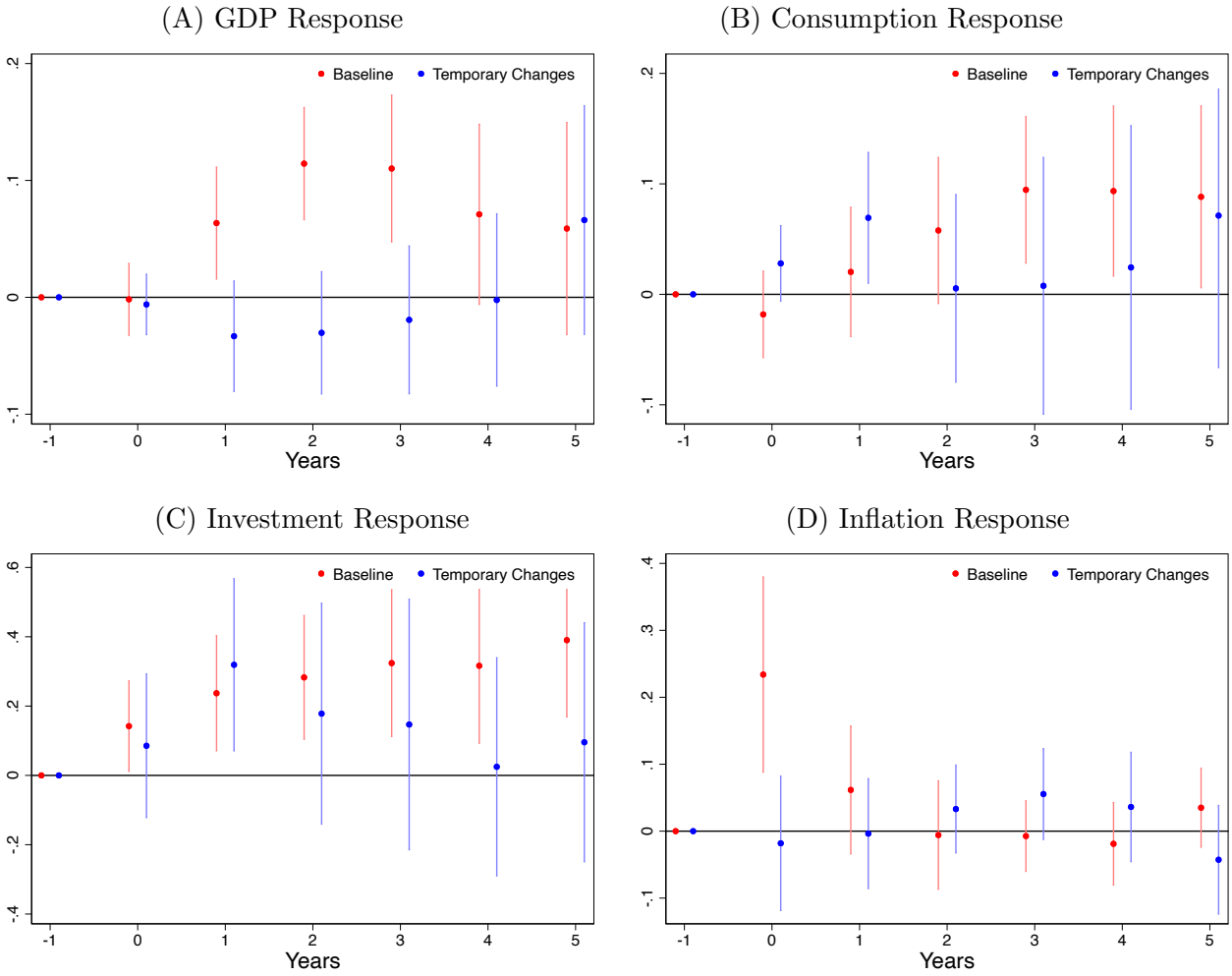
They effect on consumption and investment are transitory with both affects returning to baseline after two years. These results are consistent with out hypothesis that temporary corporate income tax changes exhibit limited effects on macroeconomic aggregates. As inflation increases following permanent tax reforms, our specification should account for how the nominal dynamics affect the real economy. As such, we pursue this question next, as we consider two robustness exercises.

**Figure IX** – Response of Macro Aggregates to All Corporate Income Tax Changes




Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cut. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms and all corporate tax changes. Panel (B) considers consumption response to permanent corporate tax reforms and all corporate tax changes. Panel (C) considers investment response to permanent corporate tax reforms and all corporate tax changes. Panel (D) considers the inflation response to permanent corporate tax reforms and all corporate tax changes. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

**Figure X** – Response of Macro Aggregates to Temporary Corporate Income Tax Changes




Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cut. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms and temporary corporate tax changes. Panel (B) considers consumption response to permanent corporate tax reforms and temporary corporate tax changes. Panel (C) considers investment response to permanent corporate tax reforms and temporary corporate tax changes. Panel (D) considers the inflation response to permanent corporate tax reforms and temporary corporate tax changes. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

### 3.2 Robustness Exercises

We conduct several robustness checks to evaluate the validity of our main result and to address two vulnerabilities of our baseline specification: omitted variable bias and anticipatory effects. As the previous exercise indicated that our baseline estimates might suffer omitted variable, we first test whether adding controls. We then test for anticipatory effects (i.e. outcome variables move in anticipation of tax reforms).

Is the observed policy response explained by a macroeconomic variable omitted from our baseline specification? This could occur if our baseline estimator suffers from significant omitted variable bias. As temporary tax changes are correlated with consumption dynamics, the addition of controls is more likely to affect our estimates of the effect of temporary tax changes. To test this possibility, we add a set of regressors including macroeconomic controls $Z_{i,t}$ to the baseline specification in (4). For the real variables, we include the inflation rate, to account for different inflation regimes. For inflation, we include output growth, to account for different growth regimes.

$$\Delta y_{i,t} \;=\; \alpha_i^h \;+\; \gamma t \;+\; \sum_{j=0}^{M} \beta_j \Delta T_{i,t-j} \;+\; \sum_{j=0}^{N} \gamma_j \Delta y_{i,t-j} \;+\; \sum_{j=0}^{N} \nu_j x_{i,t-j} \;+\; \varepsilon_{i,t}, \tag{5}$$

Figure XI compares the results with and without the macroeconomic controls. The inclusion of controls affects our estimates of the output and consumption response following permanent corporate tax reforms.

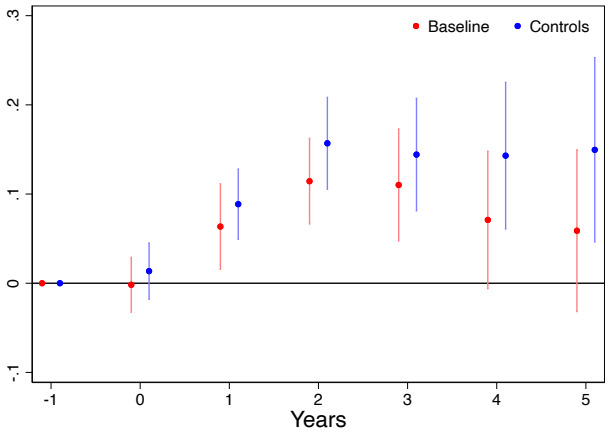
Panel (A) shows output responding to a permanent tax reform with a lag, rising above baseline one year after the reform, and achieving a persistent 15 bp increase after two years. This response is more persistent and larger than our baseline estimate, while it still exhibiting a lag. Panel (B) shows consumption responding with a longer lag, rising above baseline after three years, achieving a persistent 10 bp increase. This suggests the role of inflation in mediating the effect of output and consumption to permanent tax reforms. To benchmark our analysis, Figure XIII plots local projections for all tax changes, and Figure XII plots local projections for temporary tax changes.

The inclusion of nominal controls does not affect our inference. We find that arbitrary corporate tax changes have an attenuated effect on output, consumption, investment and inflation. The average corporate tax change has a transitory effect on consumption and investment that dissipates after two years. The effect of temporary corporate tax cuts is transitory for consumption. For GDP, investment, and inflation, the addition of controls results in insignificant and slightly negative estimates.

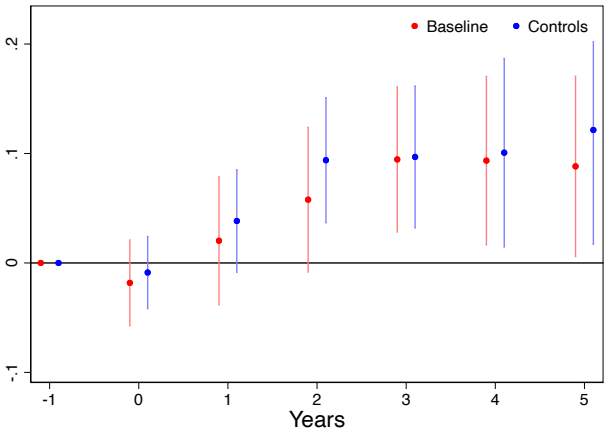
These estimates are not statistically significantly different from our baseline estimates and provide two key conclusions. First, they suggest that our baseline estimates do not suffer from omitted variable bias. Second, they suggest that permanent corporate tax changes have a greater effect on real and nominal variables than temporary tax changes. This conclusion is in line with prior empirical and theoretical work on the macroeconomic effect of tax changes. Temporary cuts in corporate income taxes below their long-run level may encourage consumption in households who cannot smooth consumption, but are unlikely to cause shifts in output or investment by forward-looking firms. Persistent shifts in the corporate tax increase the long-run profitability of capital, spurring investment. As the economy adjusts to its new steady state, consumption, output, and prices all increase. Given the large effects associated with permanent tax reforms, we consider whether economies anticipate permanent tax reforms.

**Figure XI** – Response of Macro Aggregates to Corporate Income Tax Reform with Controls

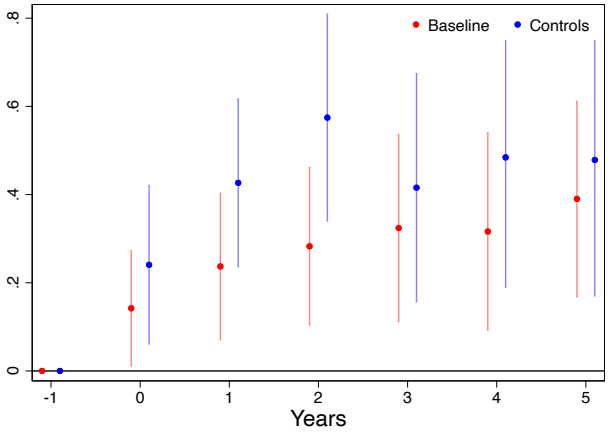
(A) GDP Response



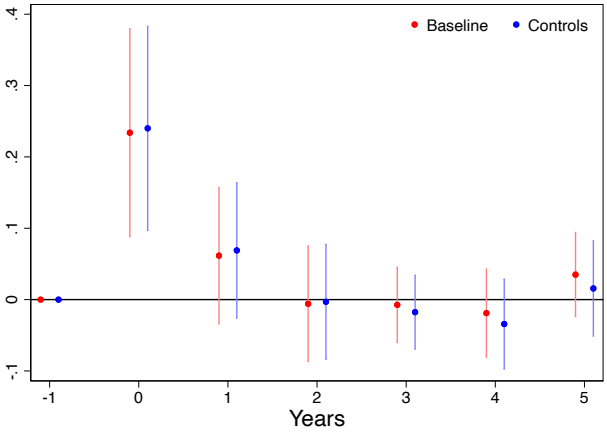

(B) Consumption Response




(C) Investment Response

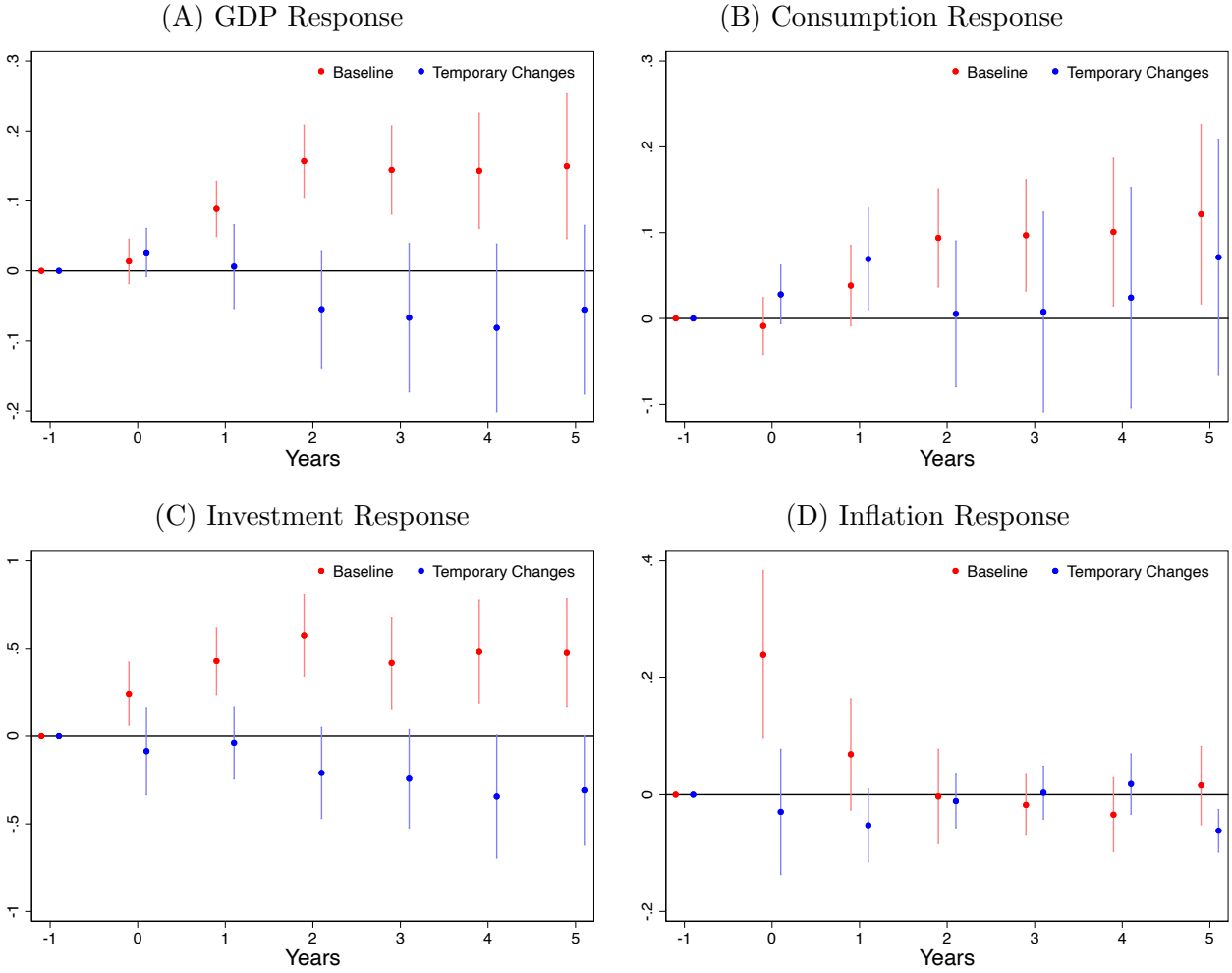



(D) Inflation Response



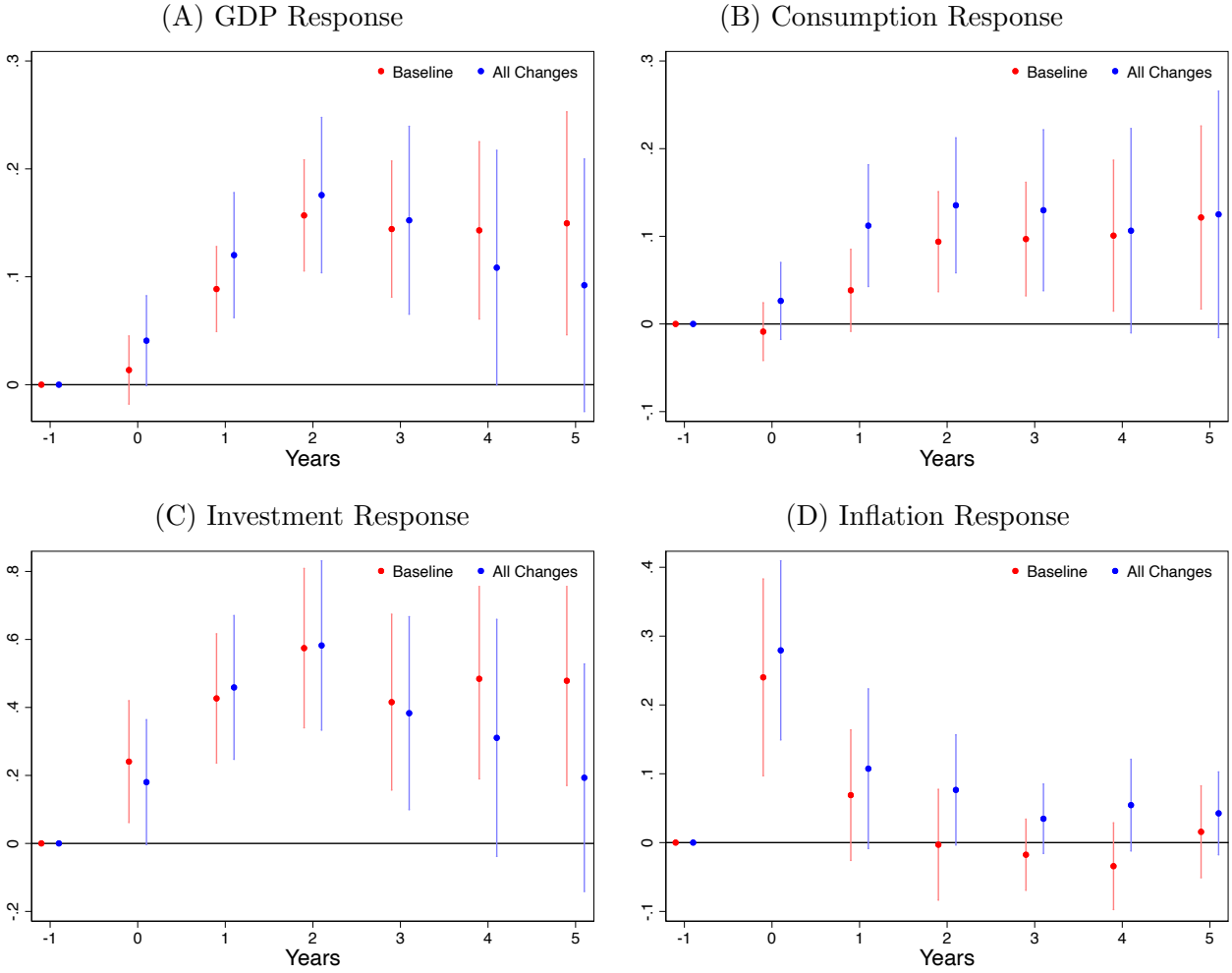

Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cuts, including controls for either output growth or inflation for real and nominal variables, respectively. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms estimated with and without controls. Panel (B) considers consumption response to permanent corporate tax reforms with and without controls. Panel (C) considers investment response to permanent corporate tax reforms with and without controls. Panel (D) considers the inflation response to permanent corporate tax reforms with and without controls. See Appendix 5 for details. Source: Vegh and Vuletin (2015) and authors' calculations.

**Figure XII** – Permanent v. Temporary Corporate Income Tax Changes with Controls




Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cuts, including controls for either output growth or inflation for real and nominal variables, respectively. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms and temporary corporate tax changes. Panel (B) considers consumption response to permanent corporate tax reforms and temporary corporate tax changes. Panel (C) considers investment response to permanent corporate tax reforms and temporary corporate tax changes. Panel (D) considers the inflation response to permanent corporate tax reforms and temporary corporate tax changes. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

**Figure XIII** – Permanent v. All Corporate Income Tax Changes with Controls

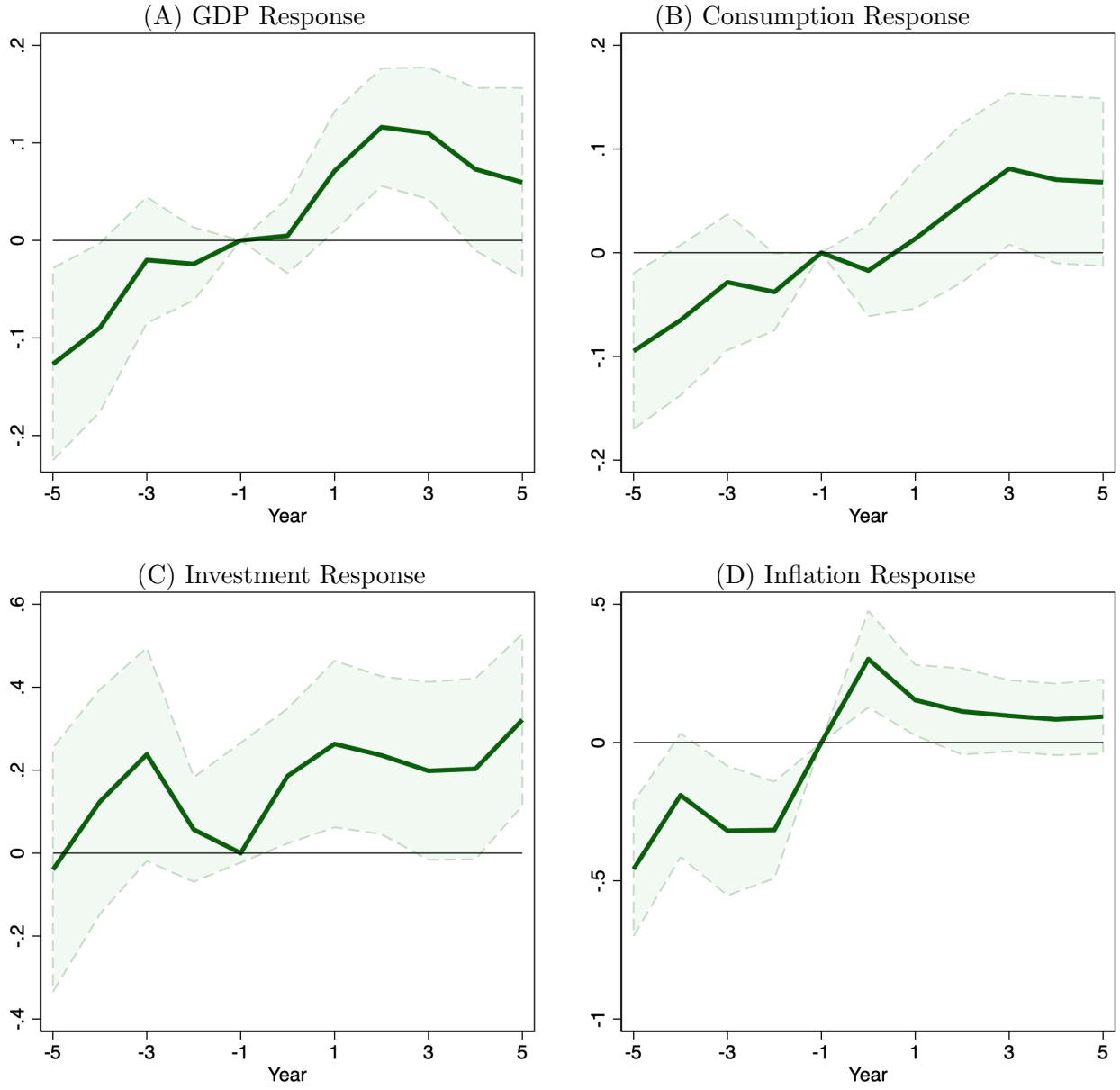



Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cuts, including controls for either output growth or inflation for real and nominal variables, respectively. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms and all corporate tax changes. Panel (B) considers consumption response to permanent corporate tax reforms and all corporate tax changes. Panel (C) considers investment response to permanent corporate tax reforms and all corporate tax changes. Panel (D) considers the inflation response to permanent corporate tax reforms and all corporate tax changes. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

### 3.3 Anticipatory Effects of Permanent Corporate Tax Reforms

Now, we study how economies anticipate permanent corporate tax reforms, by setting the lag orders of our our outcome variable and policy variable, $M$ and $N$, to one. Figure XIV plots the result of this specification.

**Figure XIV** – Anticipatory Effects of Permanent Corporate Tax Reforms

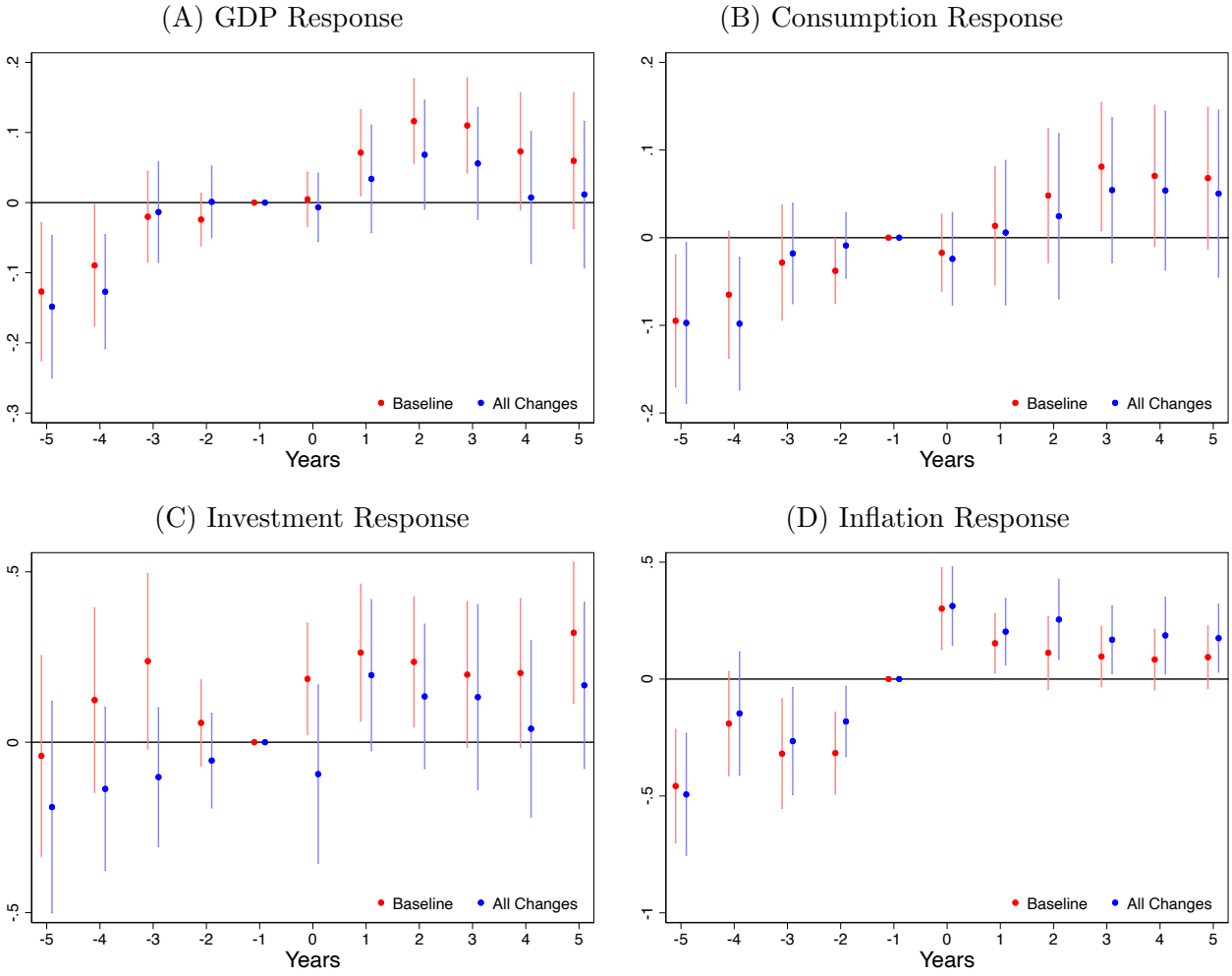



Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cuts, including controls for either output growth or inflation for real and nominal variables, respectively. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms. Panel (B) considers consumption response to permanent corporate tax reforms. Panel (C) considers investment response to permanent corporate tax reforms. Panel (D) considers the inflation response to permanent corporate tax reforms. See Appendix 5 for details. Source: Vegh and Vuletin (2015) and authors' calculations.

We find evidence of anticipation in output, consumption, investment, and inflation. Turning our

attention to panels (A) and (B), we observe a positive pre-trend in output and consumption, whereby the variables are below 10 bp baseline five years before the permanent tax reform. Panel (C) reports an investment response that rises above baseline a year before the tax reform impacts the economy. Panel (D) reports that inflation is .5bp below baseline in the years leading up to the reform. Figure XV plots local projection for all tax changes, and Figure XVI plots local projection for temporary tax changes.

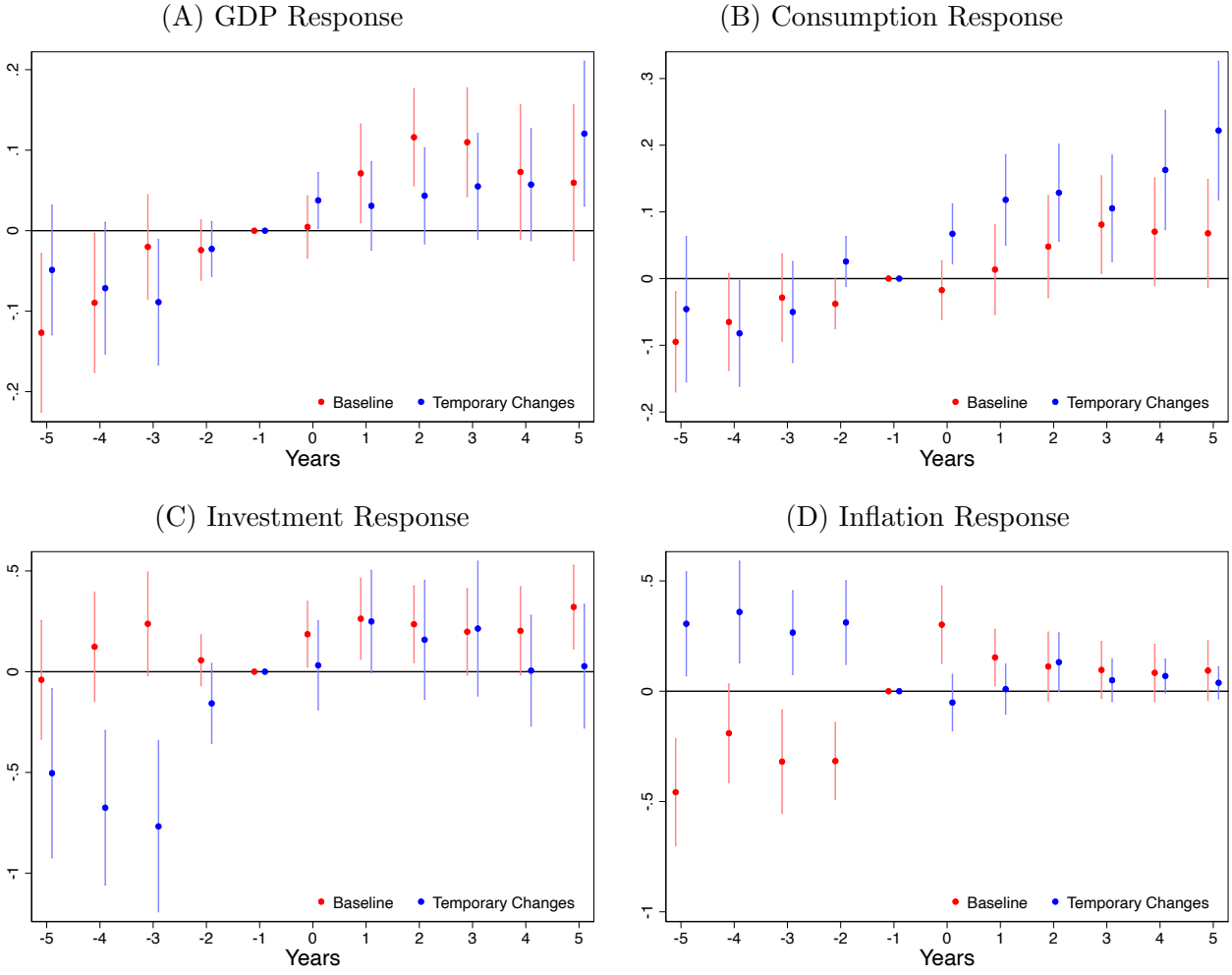
**Figure XV** – Anticipatory Effects of All Corporate Tax Changes




Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to all corporate income tax changes, allowing for anticipatory affects. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms and all corporate tax changes. Panel (B) considers consumption response to permanent corporate tax reforms and all corporate tax changes. Panel (C) considers investment response to permanent corporate tax reforms and all corporate tax changes. Panel (D) considers the inflation response to permanent corporate tax reforms and all corporate tax changes.. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

We observe greater anticipatory response of GDP and investment preceding arbitrary and temporary corporate tax changes, while the anticipatory response of consumption is smaller. While there is anticipation in prices for permanent and temporary tax changes, this may be due to the limited lags of the treatment and outcome variables. Given that reforms in our sample occur at various frequencies, these anticipatory effects could be capturing the economy responding to previous tax reforms. While

**Figure XVI** – Anticipatory Effects of Temporary Corporate Tax Changes




Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to all corporate income tax changes, allowing for anticipatory effects. Solid = Coefficient $\beta^h$ for various horizons $h$. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms and temporary corporate tax changes. Panel (B) considers consumption response to permanent corporate tax reforms and temporary corporate tax changes. Panel (C) considers investment response to permanent corporate tax reforms and temporary corporate tax changes. Panel (D) considers the inflation response to permanent corporate tax reforms and temporary corporate tax changes. See Appendix 5 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

this may be the case, these results suggest that economies expect permanent tax reforms less than temporary tax changes. This result does not contradict our pre-analysis though, as the pre-analysis simply examines whether permanent tax reforms are randomly allocated across different economic states, instead of observing time variation within countries experiencing reforms.

# 4 Conclusion

In this paper, we propose a novel methodology to identify permanent tax reforms motivated by long-term objectives. The tax reforms identified by our non-parametric filter are persistent and exogenous to current economic conditions. This methodology provides a standardized way to catalogue and study

these corporate tax reforms, without the need for time-intensive narrative approaches. Permanent corporate tax reforms identified by this filter are not systematically related to certain macroeconomic states. We also show that our measure of permanent corporate tax reforms coincides with the reforms identified by narrative methods. We find that permanent corporate tax reforms have persistent effects on real variables and transitory effects on inflation. While corporate tax reforms have a sizable effect on aggregates, the duration of the tax reform matters for its effect. In line with prior empirical and theoretical research, temporary corporate tax changes have an attenuated effect on aggregates. This suggests that corporate tax reforms have limited use as a stimulus policy to address cyclical variation in the economy.

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# 5 Data Appendix

## 5.1 Sample construction

First, we import and clean the monetary policy rate data from the BIS. The BIS data is monthly, so we take the average of the policy rates within that period. Although we recognize that one approach would be to take the end of year values, the resulting annual series did not reproduce noticeable features of the monthly series. These policy rate variables have undergone the most administrative scrutiny, since the BIS produced the dataset in collaboration with the participating central banks. Since the BIS dataset does not include all members of the OECD, we download additional interest rate data compiled by FRED and the IMF.

We import interbank rates from FRED. For now, we download immediate interest rates when available. If not, we download 3-month interbank rates. Now, there are more scientific ways of making this choice. A more rigorous option would be to read central bank annual reports following tax reforms to check which series they mention in relation to the policy. we import immediate interbank rates from Portugal and Italy. We also import 3-month interbank rates for Germany, Spain, Greece, France, and Japan.

We import consumer price indexes, industrial production indexes, nominal gross domestic production series, gross domestic production deflators, interest rates, and fiscal policy data from various IMF surveys. The IMF provides a version of the WORLD fiscal database in Stata format. We downloaded series-specific spreadsheets for variables included in the HPDD, IFS, and CPI surveys.

We import unemployment rate data hosted by the World Bank and constructed by the International Labour Organization, as part of their ILOSTAT database. We also import additional unemployment rate data hosted by the OECD.

The last dataset we import is the Penn World Table 10.0. Currently, we import the entire data set, but only use the data on real gross output constructed using national product accounts and investment.

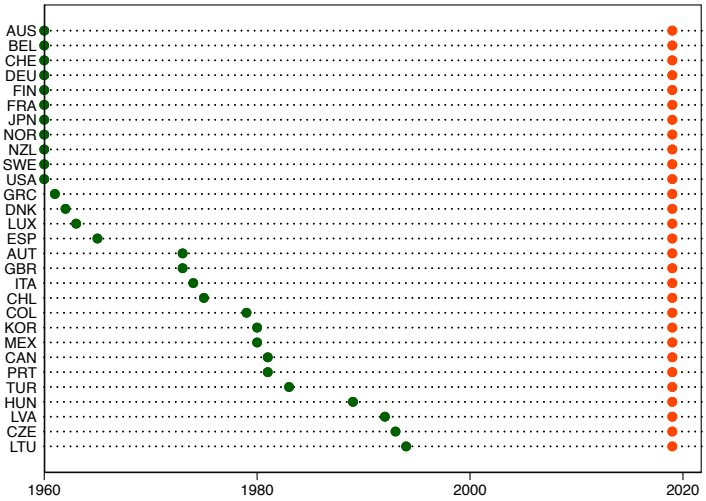
**Source Selection** There exists more than one candidate series for some countries and variables. At this point, we need to select our sources at the country-level for the following series: real output, policy rate, and unemployment. The source selection process occurs in three steps. First, we assign all countries to a base source. This base source differs across variables: IMF for policy rates, BIS for policy rates, and OECD for unemployment. Second, we assign countries to the source that maximizes the size of the country's sample. Third, we manually assign countries to sources that do not maximize the size of their sample, if one source appears to have less noise.

A more scientific approach would be to develop more stringent criteria for selection, but most cases are well behaved and the few that aren't are very apparent. For policy rates, we manually reassign Hungary, Norway, and Austria to the BIS and Japan to the IMF because those series are more well-behaved than other, longer series. For policy rates, we manually reassign Germany and the UK to the PWT real output data for similar reasons. We do not manually reassign any unemployment series.

**Data Cleaning** Data cleaning is intentionally as naive as possible. First, we interpolate on one variable, the debt to GDP ratio variable, because there are several breaks in otherwise well-behaved series. We then drop certain outlier periods before applying a time-series filter, so that the filter does not create a systematic relationship between periods with extremely different economic regimes and cyclical dynamics. At the moment, we drop observations from: Brazil before 1995 (hyperinflation),

Russia prior to 2000 (liberalization), Turkey before 2004 (no price stabilization), and CHL before 1980 (inflation). Again, we based these decisions on the series and brief research, so we can discuss how to make this more scientific, although it seems like the approach might need to be somewhat heuristic and narrative.

**Figure XVII** – Sample Countries and Period




Notes: This table reports the sample size of each country included in the data set.

### Tax data.

**Macroeconomic data.** We searched for aggregate data relevant to monetary and fiscal policy starting in 1960. For our corporate income tax series, we augmented and revised the data presented in Vegh and Vuleting (2015). For aggregate variables, we collected data on nominal output, price deflators, unemployment at annual frequencies from datasets maintained by the IMF. For government debt to output ratios, we include data from the Penn World Table version 10.0. Compiling data from these sources, we constructed a new dataset to study corporate income tax reform. Specifically, we use the following series, in addition to our tax dataset, for our empirical analysis.

- Nominal Gross Domestic Product, Domestic Currency, International Financial Statistics, IMF, https://data.imf.org/?sk=4c514d48-b6ba-49ed-8ab9-52b0c1a0179b, accessed: February 24, 2022
- Gross Domestic Product Deflator, Index, International Financial Statistics, IMF, https://data.imf.org/?sk=4c514d48-b6ba-49ed-8ab9-52b0c1a0179b, accessed: February 24, 2022
- Monetary Policy-Related Interest Rate, Percent per annum, International Financial Statistics, IMF, https://data.imf.org/?sk=4c514d48-b6ba-49ed-8ab9-52b0c1a01\79b, accessed: February 7, 2022
- Discount rate, Percent per annum, International Financial Statistics, IMF, https://data.imf.org/?sk=4c514d48-b6ba-49ed-8ab9-52b0c1a0179b, accessed: February 7, 2022
- Industrial Production, Index, International Financial Statistics, IMF, https://data.imf.org/?sk=4c514d48-b6ba-49ed-8ab9-52b0c1a0179b, accessed: February 24, 2022

- Debt-to-GDP ratio, Historical Public Debt Database, IMF, https://data.imf.org/?sk=806ED027-520D-497F-905 accessed: February 24, 2022
- All Indexes, Consumer Price Index, IMF, https://data.imf.org/?sk=4FFB52B2-365\3-409A-B471-D47B46D904B5 accessed: February 24, 2022
- All items, World Revenue Longitudinal Data Set, IMF, https://data.imf.org/?sk=77413F1D-1525-450A-A23A-47 accessed: February 24, 2022
- Real GDP at constant 2017 national prices ,index, Penn World Table, https://www.rug.nl/ggdc/productivity/pwt/?lang=en, accessed: December 15, 2021
- Unemployment rate, All Series, OECD (2022), https://data.oecd.org/unemp/unem\ployment-rate.htm, accessed: February 24, 2022
- Unemployment rate (modeled ILO estimate), ILOSTAT database, https://data.worldbank.org/indicator/SL.UEM.TOTL.ZS, accessed: February 24, 2022
- Central bank policy rates, BIS database, www.bis.org/statistics/cbpol.htm, accessed: October 17, 2021
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for the Euro Area, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IRSTCI01EZM1\56N, accessed February 24, 2022.
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for Portugal, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IR3TIB01PTM1\56N, accessed February 24, 2022.
- 3-Month or 90-day Rates and Yields: Interbank Rates for Italy, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IR3TIB01ITM156N, accessed February 24, 2022.
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for Germany, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IRSTCI01DEM1\56N, accessed February 24, 2022.
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for Spain, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IRSTCI01ESA156N, accessed February 24, 2022.
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for Greece, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IRSTCI01GRM156N, accessed February 24, 2022.
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for France, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IRSTCI01FRM156N, accessed February 24, 2022.
- Immediate Rates: Less than 24 Hours: Call Money/Interbank Rate for Japan, OECD, retrieved from FRED, https://fred.stlouisfed.org/series/IRSTCI01JPM156N, accessed February 24, 2022.

When aggregating corporate tax rates within a country, we always keep the top marginal rate to ensure consistency. Figure XVII depicts the coverage of our sample. Table A.I reports our data sources and when they were accessed.

**Table A.I** – Macroeconomic Time Series: Description and Sources

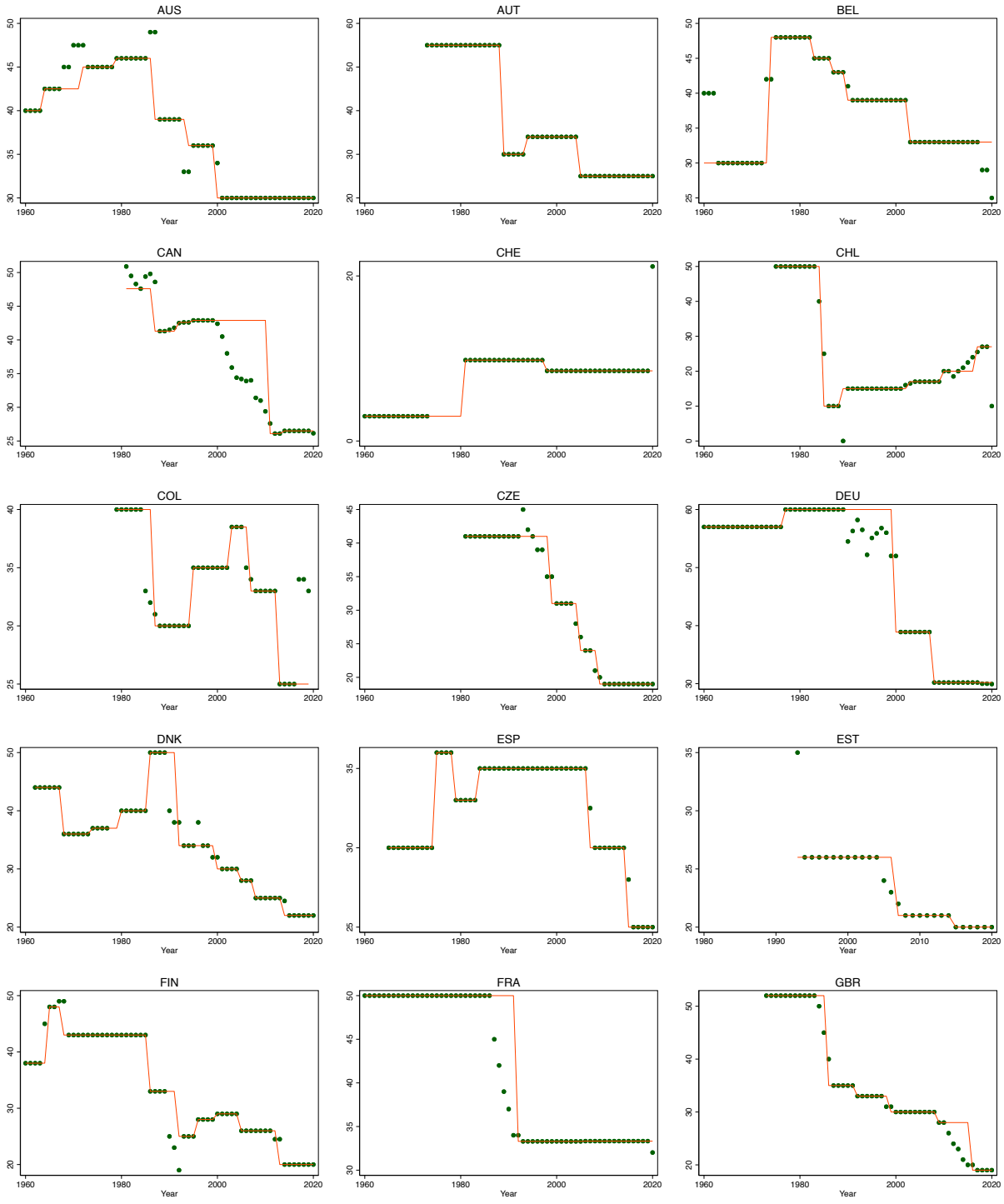
| Label | Short description | Source | Frequency |
|---|---|---|---|
| GDP | Nominal GDP, Billions of Dollars | IMF | Annual |
| GDPD | GDP Implicit Price Deflator | IMF | Annual |
| CPWe | Consumer Price Index | IMF | Annual |
| We | Central Bank Policy Rate, Percent | BIS | Monthly |
| DEBT | Debt to GDP ratio, Percent | PWT | Annual |

Notes: Access Dates: BIS: October 17, 2021, IMF:October 17, 2021, PWT: Dec 15, 2021

# 6 Permanent Tax Reforms by Country

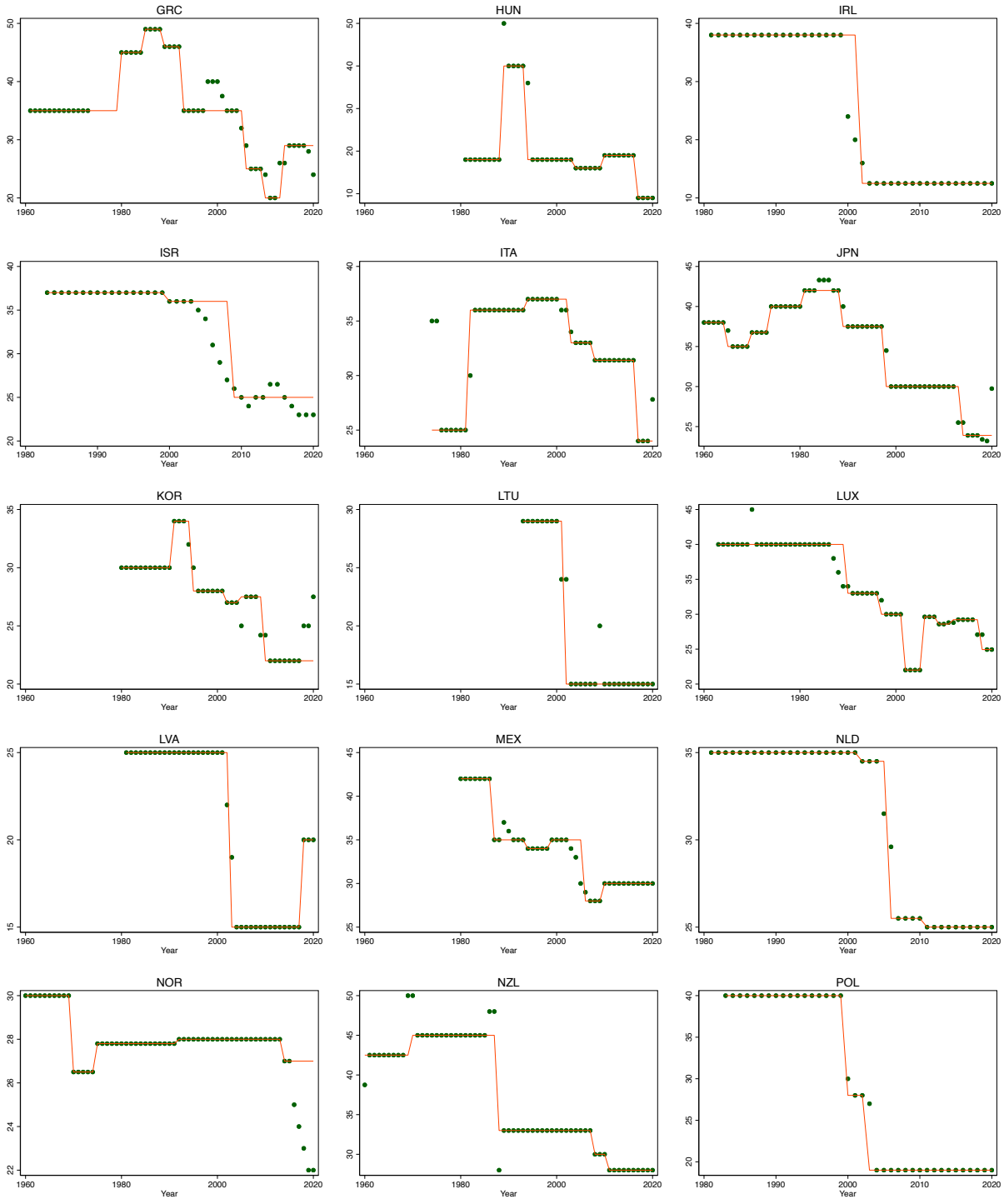
In this section, we present the result of the filtering procedure by country. Each figure plots the permanent corporate income tax series, along with the raw, unfiltered corporate income tax data.

**Figure XVIII** – Permanent Tax Reforms by Country (A)



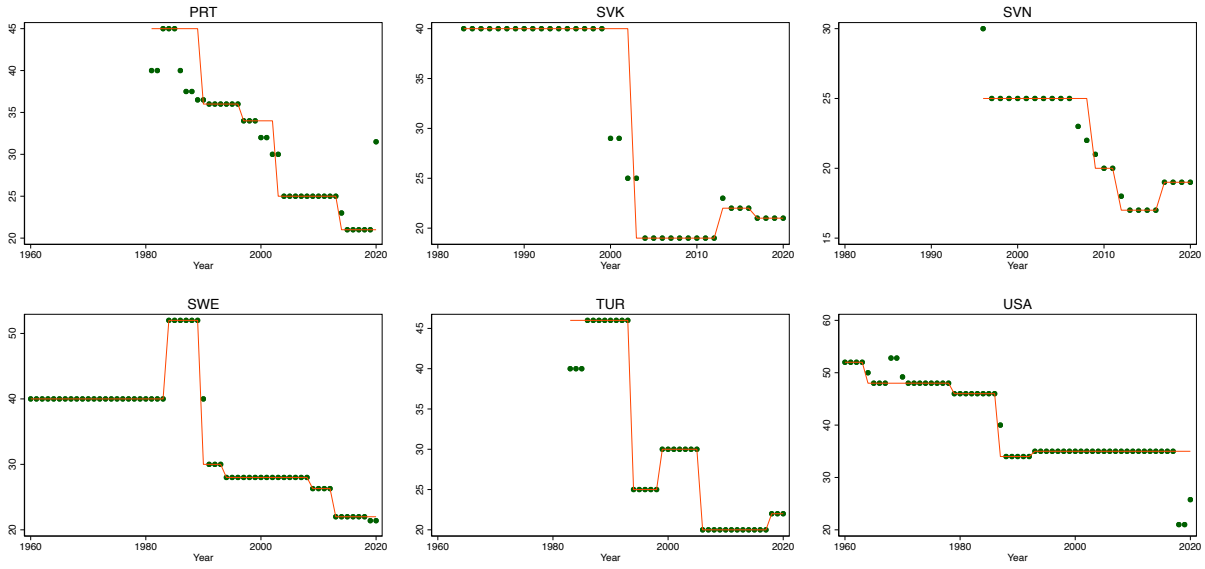

Notes: Raw and filtered series.

**Figure XIX** – Permanent Tax Reforms by Country (B)




Notes: Raw and filtered series.

**Figure XX** – Permanent Tax Reforms by Country (C)

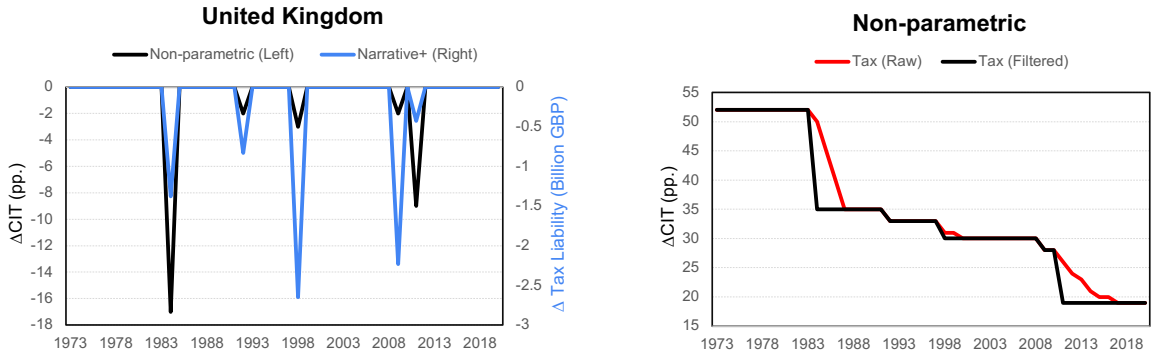



Notes: Raw and filtered series.

# 7 Comparison with Narrative Measures

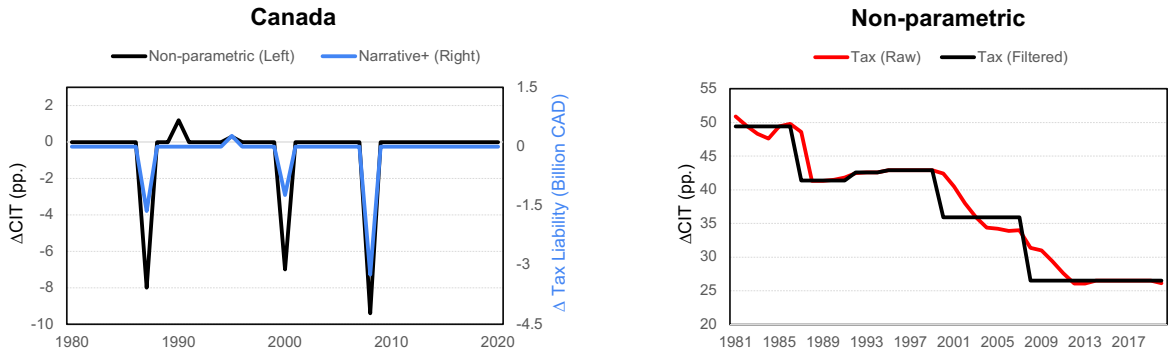
In this section, we present the comparison of the nonparametric filter approach with narrative approaches for specific countries. We find that our nonparametric measure largely coincides with the narrative approach and is less correlated with cyclical variation in aggregates relative to the narrative measures.

**Figure XXI** – UK Tax Reforms: Nonparametric and Narrative Approaches



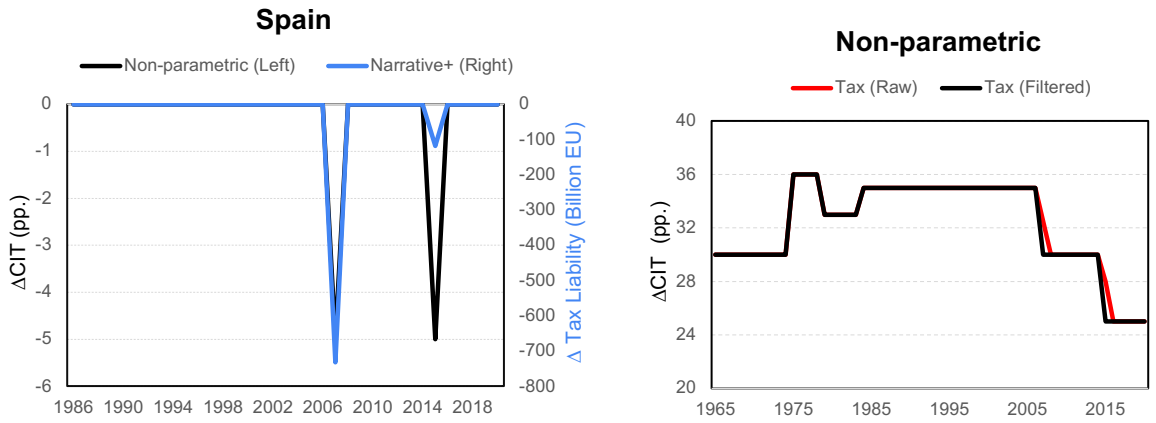

Notes: Source: Authors' calculations and Cloyne (2013). see Appendix 5 for details.

**Figure XXII** – Canada Tax Reforms: Nonparametric and Narrative Approaches



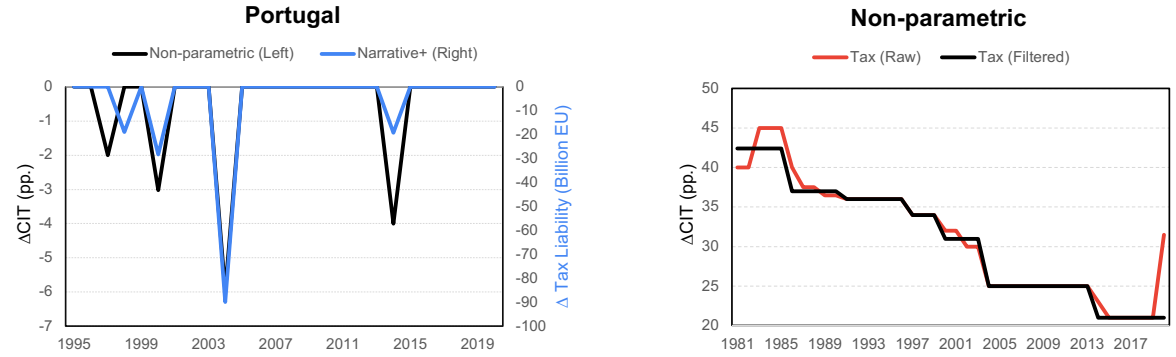

Notes: Source: Authors' calculations and Hussain and Liu (2019). see Appendix 5 for details.

**Figure XXIII** – Spain Tax Reforms: Nonparametric and Narrative Approaches



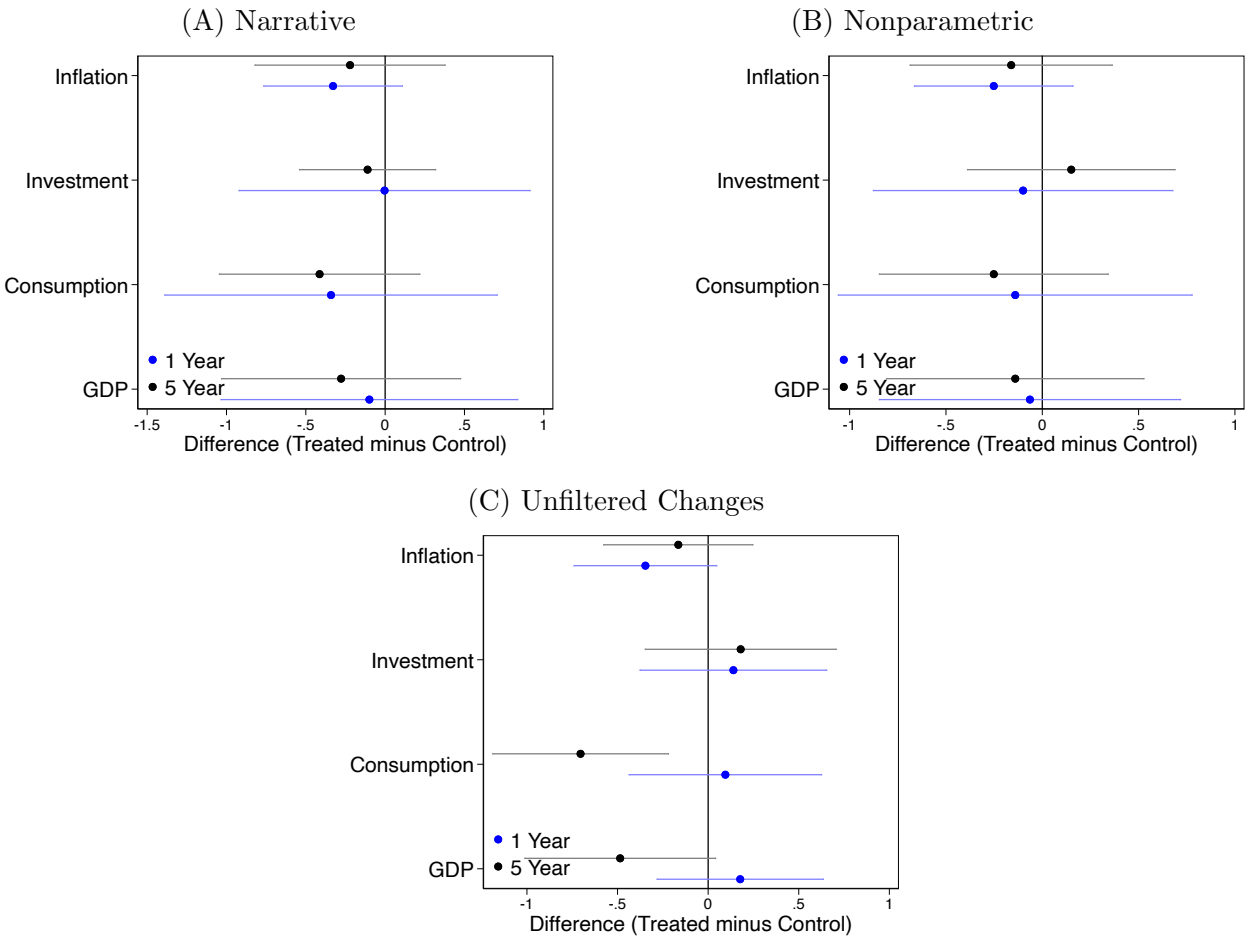

Notes: Source: Authors' calculations and Gil *et al.* (2019), see Appendix 5 for details.

**Figure XXIV** – Portugal Tax Reforms: Nonparametric and Narrative Approaches



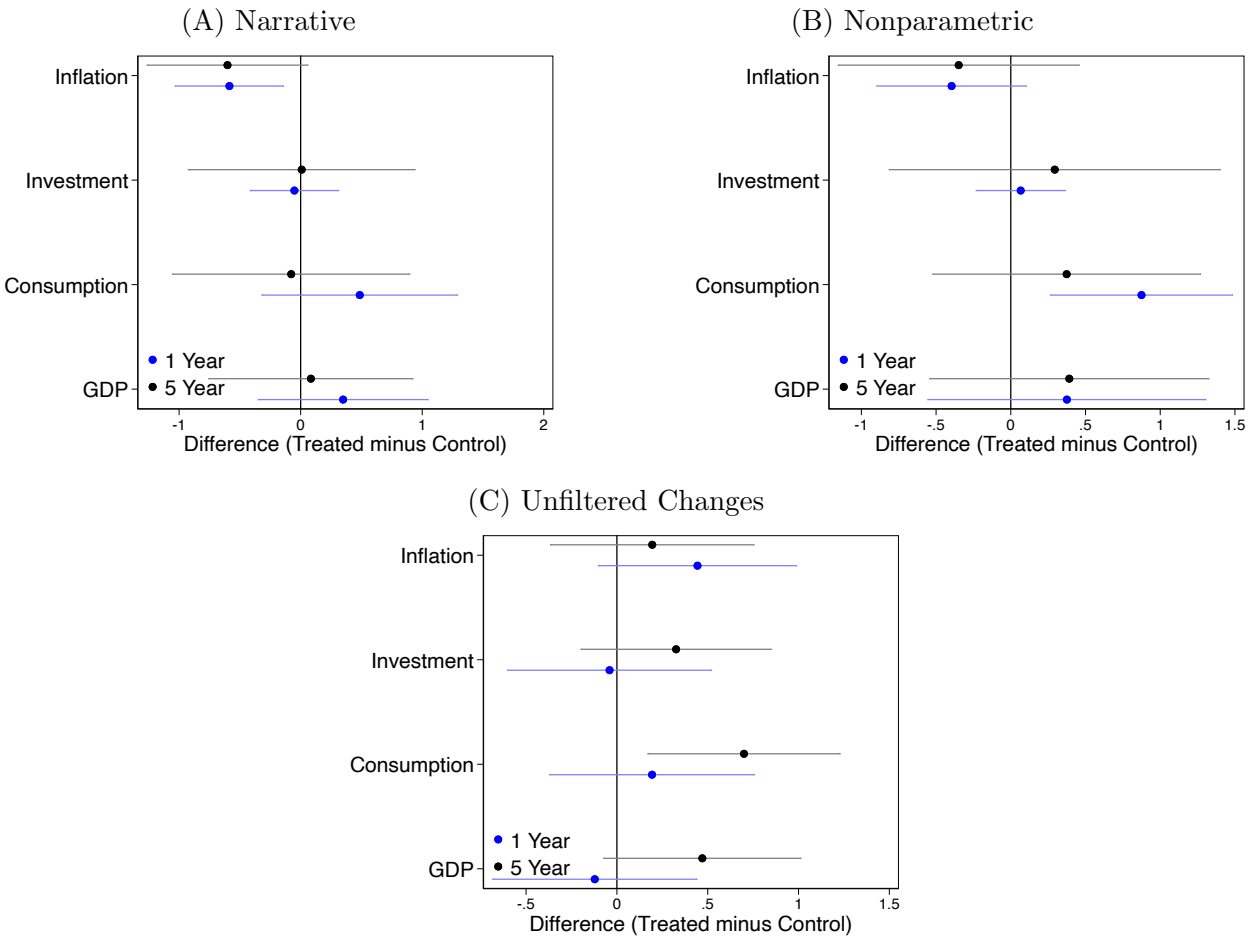

Notes: Source: Authors' calculations and Pereira and Wemans (2015). see Appendix 5 for details.

**Figure XXV** – Covariate Balance across Negative Tax Reforms, UK



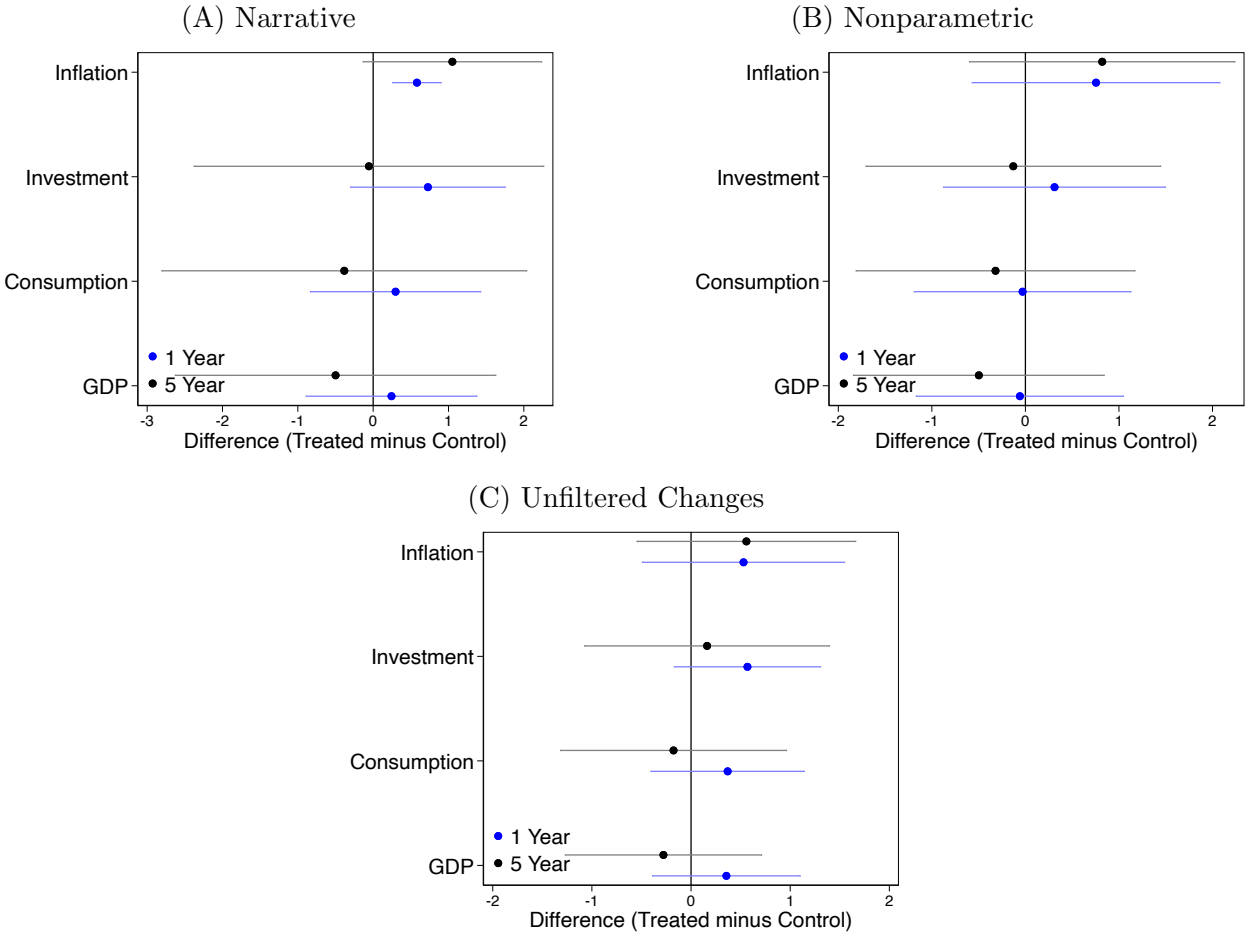

Notes: These tables show balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. As narrative and nonparametric methods identified only vanishingly few permanent corporate tax reform for the United Kingdom, we only report the balance plots for negative reforms. Source: Authors' calculations and Cloyne (2013). See Data Appendix for details.

**Figure XXVI** – Covariate Balance across Negative Tax Reforms, Canada



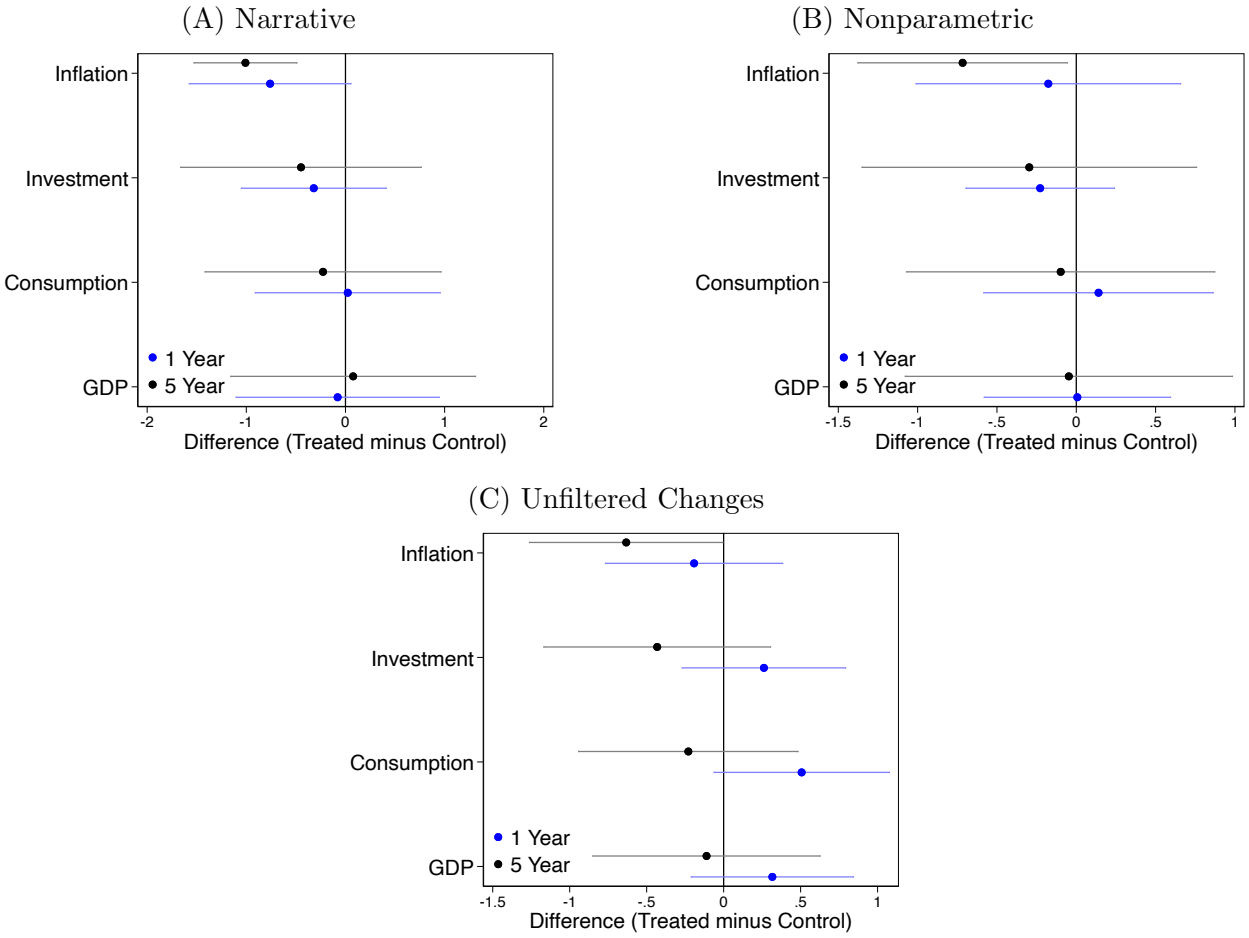

Notes: These tables show balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. As narrative and nonparametric methods identified only vanishingly few permanent corporate tax reform for the Canada, we only report the balance plots for negative reforms. Source: Authors' calculations and Hussain and Liu (2019). See Data Appendix for details.

**Figure XXVII** – Covariate Balance across Negative Tax Reforms, Spain




Notes: These tables show balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. As narrative and nonparametric methods identified only vanishingly few permanent corporate tax reform for the Spain, we only report the balance plots for negative reforms Source: Authors' calculations and Gil *et al.* (2019). See Data Appendix for details.

**Figure XXVIII** – Covariate Balance across Negative Tax Reforms, Portugal




Notes: These tables show balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. As narrative and nonparametric methods identified only vanishingly few permanent corporate tax reform for the Portugal, we only report the balance plots for negative reforms. Source: Authors' calculations and Pereira and Wemans (2015). See Data Appendix for details.