Monetary Policy and Corporate Tax Reforms*

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We study how monetary policy responds to permanent corporate tax reforms that are motivated by long-term objectives. Using cross-country data between 1960 and 2020, we construct a set of corporate-tax-reform shocks and show that they are exogenous to current inflation and output. Using these new corporate tax shocks, we document that central banks raise policy rates an average of 2 percentage points following a drop of 10 percentage points in the statutory corporate income tax rate. We setup a New Keynesian model with corporate taxes to assess the observed response and examine optimal monetary policy.

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"Central banks are often accused of being obsessed with inflation. This is untrue. If they are obsessed with anything, it is with fiscal policy."

Mervyn King, Former Governor of the Bank of England.[1]

1 Introduction

The primary objective of most central banks is to ensure price stability, and in many cases, to maintain output close to its potential. To the extent that fiscal policy has an impact on inflation and output dynamics, it matters for monetary policy. Within the various stances of fiscal policy, the corporate tax structure—through its effect on private investment—is relevant in determining the short-run fluctuations and the long-run growth potential of an economy, and in principle, should be of interest to central bankers. Do central banks respond to corporate tax reforms? Should monetary policy explicitly take into account corporate taxation?

The relationship between monetary policy and corporate taxation has surprisingly received little attention in the literature due to several 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.[2] Thus data limitations preclude from establishing a systematic relationship when using information from a single country. The second challenge is methodological: Diff-in-diff methodologies rely on cross-sectional heterogeneity in treatment to estimate firm-level responses to tax reforms, and therefore, by design, are unable to measure the effects on aggregate quantities and prices, which are exactly the objects monetary policy responds to.[3] The third challenge is the identification of fiscal shocks in general, and corporate tax shocks in particular: Both fiscal and monetary policy could be jointly influenced by other aggregate variables, thus a simple series of corporate tax changes is not appropriate to estimate the monetary policy response.

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 dataset with

[1] "Commentary: Monetary Policy Implications of Greater Fiscal Discipline", Budget Deficits and Debt: Issues and Options, Symposium sponsored by the Federal Reserve Bank of Kansas City, 1995, pp.171-183.

[2] See Figure II.

[3] House and Shapiro (2008) exploits differences in the timing and eligibility of the type of capital to study temporary tax incentives in the US; Yagan (2015) exploits differences between C-corporations and S-corporations to estimate the effect of dividend tax cut in the US; Zwick and Mahon (2017) exploit technological differences to estimate the effects of bonus depreciation in the US; Ohrn (2018) exploits industry variations in the eligibility for income deductions to estimate the effects of the corporate income tax rate in the US; Lerche (2019) exploits differences in firm size to estimate the effects of investment tax credits in Germany; Maffini, Xing and Devereux (2019) exploit differences in qualifying thresholds to estimate the effects of accelerated depreciation allowances in the UK; Boissel and Matray (2019) exploit differences in firms' legal statuses to estimate the effects of dividend taxes in France; and Chen, Jiang, Liu, Suárez Serrato and Xu (2019) exploit differences between foreign and domestic firms to estime the effects of VAT deductions in China.

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 or fairness concerns, that remain in place for long periods of time—, which ensures that the drivers of corporate tax reforms are different from the drivers of monetary policy.

Through a variety of tests, we show that our new measure of corporate tax changes is exogenous to monetary policy, and in the case of the US, that our series coincides with series obtained through a narrative approach by Romer and Romer (2010). Third, we use our new shock series to estimate central bank responses to tax reforms using local projections methods (Jordà, 2005). We find that a drop in the statutory corporate tax rate of 10 percentage points generates a 2 percentage point increase in the policy rate.

Empirical results. Our main result is that, on average, central banks raise policy rates after a corporate tax reform. On average, there is a 2 percentage point (pp.) increase in the policy rate after a 10 pp. decrease in the corporate income tax rate. We also observe a lag in the Central Banks responses: On average, monetary policy rates peak two to three years after the tax reform. The shape of the response suggests that the policy response is transitory, as rates go back to their average value after 4 years. Importantly, these results control for country-level outcomes, which by definition, embed general equilibrium and aggregate effects.

With new tax reform instrument to examine the relationship between tax reform, monetary policy, and other aggregates. First, we estimate our baseline specification, using local projection (LP) methods by Jordà (2005). We then employ three other LP estimators to address concerns relating to omitted variable bias, allocation bias, and anticipatory effects. This section concludes with a discussion of the relationship between our tax reform measure and other aggregates.

Quantitative results (In progress). The second part of the paper aims to understand the documented monetary policy response through the lens of a medium-scale New Keynesian model suited to conduct counterfactuals and study optimal policy. In the spirit of Justiniano, Primiceri and Tambalotti (2010), our framework will consist of a DSGE model with capital and labor inputs, wage and price rigidities, investment frictions, corporate taxes, and a Taylor-type rule followed by the monetary authority. The calibrated version of this model replicates the observed response.

Related literature. To be completed.

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 philosophy behind our approach. Second, we implement our methodology.

2.1 Data

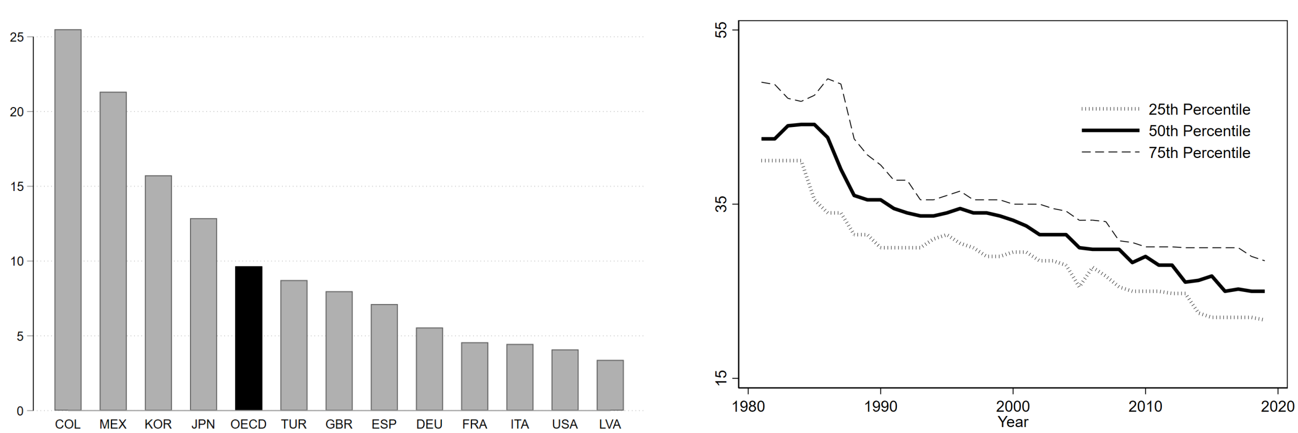
Our data consists of an annual unbalanced cross-country panel for 36 countries for the period 1960–2019. To assemble the cross-country panel on statutory corporate tax reforms, we gather several dispersed sources. Data on statutory corporate tax rates comes from Vegh and Vuletin (2015) for the corporate 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 A 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.

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.

2.2 Endogenous and exogenous corporate tax changes/Conceptual framework

Corporate tax changes always happen for a reason. Nevertheless, to answer our research question, these reasons must be different from the main drivers of monetary policy. Indeed, if corporate tax changes respond to the same economic motives as monetary policy, then we have a "omitted variable bias." In that case, both monetary policy and corporate tax respond to the same shock, and we cannot identify the response of the former to the latter.

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 identifies 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.

The first two categories are considered "endogenous", as the motivations are likely to correlate with developments affecting current output and inflation, critical variables for monetary policy. For instance, tax cuts designed to lift the economy from a recession or to finance transitory increased in expenditure. 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.[4]

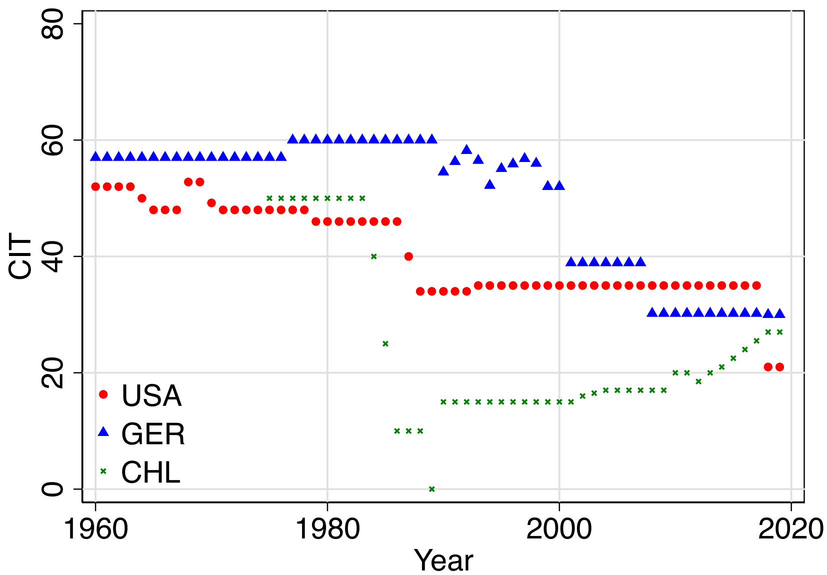
Let us illustrate the type of "exogenous and permanent" tax changes motivated by long-run goals with examples from three countries. Figure II plots the corporate income tax statutory top marginal rate for the US, Chile, and Germany between 1960 and 2020. One common observation across the three time series is 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*.[5] 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,

[4]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.

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

Figure II – Corporate Income Tax Rate



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

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.[6] 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%.[7] 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: tax changes motivated by long-run objectives, such as sustainability of public finances, preferences for redistribution and fairness,

[6]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 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".

[7]See Kinghorn (2016) for details.

and efficiency considerations, from 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, e.g., 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, and thus precludes us from using such filters. 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 (Stevens, 2020; 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.[8] Then, 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).[9] 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. 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

[8] 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.

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

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

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

with initial condition $X_0 \sim_{i.i.d.} \mathcal{N}(\mu_0, \sigma_0)$ and shocks $R_t \sim_{i.i.d.} \mathcal{N}(\mu_P, \sigma_P)$. The distribution of initial conditions reflect level and difference cross-country in corporate income tax rates at the beginning of the sample. The shocks R_t stand for reforms, which have a drift μ_P and dispersion σ_P. The parameter λ_P reflects the frequency at which reforms occur.

The transitory component is described by a discrete state space $S = \{1, 2\}$ with transition probability $Q^S = [q_{11}, q_{12}; q_{21}, q_{22}]$. 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 (μ_0, σ_0) by targeting the mean and dispersion of CIT in 1960. We discipline the probability of transitory and permanent changes together with their size $(\mu_P, \sigma_P, \sigma_T, \lambda_P, q_{11})$ 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

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 ΔCIT_{t+4}	-0.028	-0.033
Std(ΔCIT_{t+1})	0.048	0.031
Std(ΔCIT_{t+4})	0.066	0.044
Frequency(ΔCIT_{t+1})	0.774	0.716
Frequency(ΔCIT_{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	(μ_0, σ_0)	$(0.402, 0.487)$
Average and dispersion of permanent reform	(μ_P, σ_P)	$(0.393, 0.076)$
Arrival rate of permanent reform	λ_P	0.054
Dispersion of transitory tax changes	σ_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.

Break-Test and the Kehoe-Midrigan methods.

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 beggining 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, note that transitory shocks don't allow the method to identify the precise date of the reform but 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

Table II – Moments in the Model and Filtered Moments

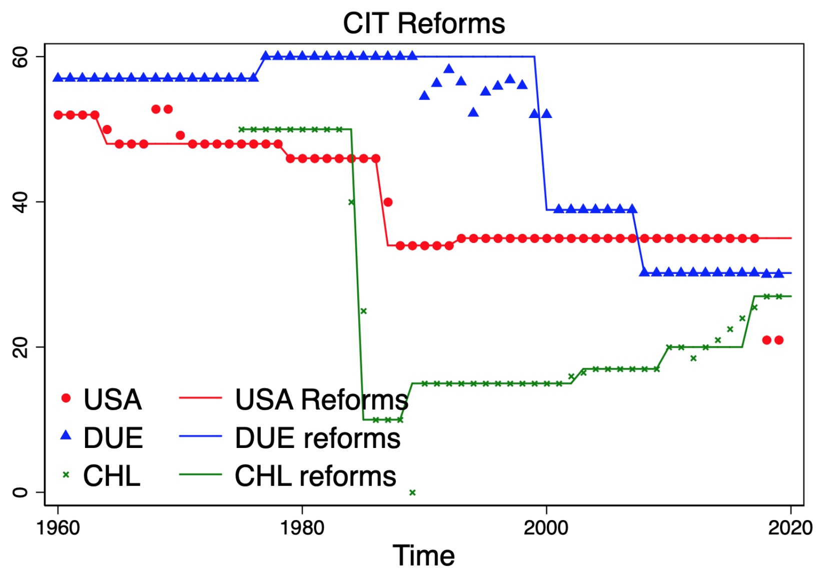
Moment	Model	Break-Test	Kehoe-Midrigan
Frequency reforms	0.052	0.052	0.041
Type I error for no reforms			
Reform t given no reform t		0.009	0.0112
and $T-3>t>3$		0.009	0.0112
Type II error for no reforms			
No reform t given reform t		0.160	0.396
and $T-3>t>3$		0.110	0.338
and no reform in $t-1$, t, $t+1$		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 B 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.

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 the Appendix, Figures XIII 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. Source: Vegh and Vuletin (2015) and authors' calculations. See Data Appendix for details.

2.4 New series of corporate tax reforms

Let us describe the statistical properties of our new series. Before we proceed, we define *radical reforms* as those where the change in the corporate income tax rate is below the 33th percentile in the size distribution, which corresponds to a change in the tax rate of −5.5 pp. or below. In our empirical analysis, we will focus on radical reforms as the filter suffers from identification of permanent small reforms as explained above. Table III shows summary statistics.

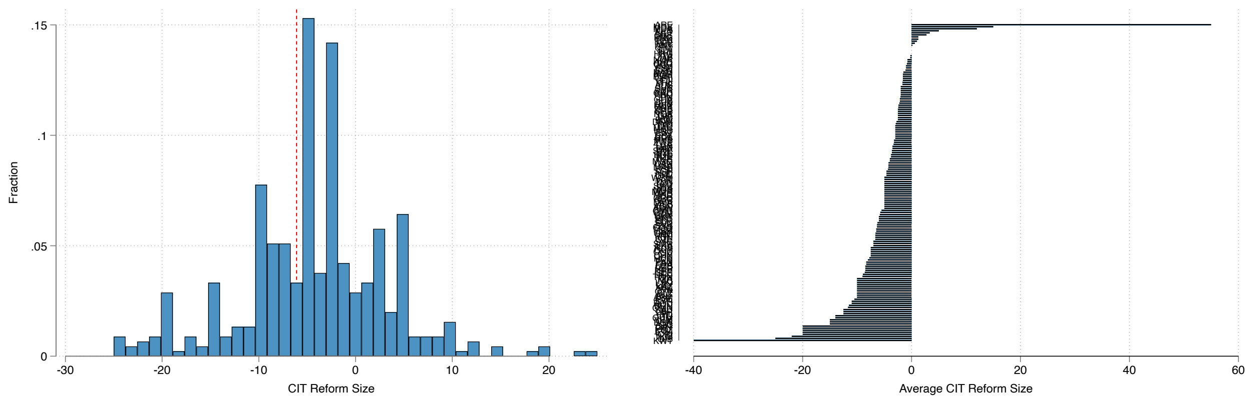
Table III – Statistics of Permanent Corporate Tax Reforms

	All reforms		Radical reforms	
Statistic	Size (%)	Duration (years)	Size (%)	Duration (years)
Mean	−3.3	7.7	−11.7	17.3
Std	7.8	4.7	6.8	6.9
P10	−11.0	4.0	−21.0	8.0
P50	−3.0	6.0	−9.0	16.0
P75	1.3	8.0	−7.0	24.0
P90	5.0	15.0	−6.0	26.0
Obs	132	103	42	16

Notes: Authors' calculations using filtered series of permanent tax reforms. Radical reforms = change in corporate income tax rate is below −5.5 pp.

The average permanent corporate tax reform consists of a rate decrease of 3.3 pp. and occurs almost 8 years after the previous tax change. The average radical reform features a rate decrease of 11.7 pp. and occurs less frequently, once in 17 years on average. On the left, Figure IV plots the histogram of all reforms with a red line indicating the radical reform threshold. On the right, Figure IV plots the average CIT reform size across countries.

Figure IV – Size Distribution of Corporate Income Tax Reforms



Notes: Distribution of filtered corporate tax reforms. Source: Authors' calculations. See Data Appendix for details.

Notice that average reform size varies across countries, which carries two consequences for our project. First, it highlights the importance of a cross-country approach to study the effect of tax policy on monetary policy, since one country's previous tax policies may not span the entire set of possible tax rates. Second, in order to study radical reforms, we will need to exclude a portion of the dataset for which there are no tax reforms in the sample.

Sample selection We exclude observations for countries that joined the European Monetary Union 5 years before they joined. We also exclude countries with zero tax reforms during the sample period.

2.5 Exogeneity of permanent tax reforms

Before we use our measure, we first need to examine whether it is systematically related to other macroeconomic aggregates. The key assumption for identifying the effect of the corporate tax reforms on the monetary policy rate is that corporate tax reforms are exogenous. Otherwise, if the corporate tax reforms were an endogenous response to current economic conditions, then we would measure the monetary policy response to these conditions as well. Our preliminary evaluation proceeds in two steps. First, we examine whether our measure is sufficiently randomized across countries and macroeconomic environments. Second, we examine whether certain aggregates can predict our measure. Lastly, we compare our measure to the narrative measure constructed in Romer and Romer (2010), to benchmark our preliminary analysis.

Are tax reforms randomized? We test our null hypothesis of "balance" across treatment (reform) and control groups by testing the equality of means across the two groups. Table IV shows that we fail to reject the null of equality for contemporaneous variables and their lagged averages. 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, suggesting that 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. We discuss the consequences of this test for our estimates later in the section.

Table IV – Checking for balance across treatment and control groups

	Year of Reform		Previous 5 years	
Variable	Difference	p-value	Difference	p-value
Policy Rate	−.010	0.85	0.10	0.49
Unemployment	−0.92	0.31	−1.02	0.72
Consumer Prices	−1.18	0.66	0.74	0.69
Debt/GDP	−2.17	0.73	−3.07	0.58
Real GDP	−1.04	0.06	0.17	0.73

Notes: The table shows balance between treatment and control groups are balanced. Columns two and three refer to contemporaneous covariates. Columns four and five refer to lagged five year averages of the covariates. Source: Romer and Romer (2010) and authors' calculations. All variables are HP filtered (100).

Are tax reforms unpredictable? Next, we test the null hypothesis that cyclical aggregates does predict our measure. Table V runs a logit regression of permanent negative tax reforms on a set of macroeconomic indicators and country and time fixed effects. The our benchmark regression takes the form

$$\log\left(\frac{P(D_{i,t}=1|Z_{i,t-1})}{P(D_{i,t}=0|Z_{i,t-1})}\right) \;=\; \alpha_i^h \;+\; \delta Z_{i,t-1} \;+\; \varepsilon_{i,t}, \tag{4}$$

where we use a logistic estimator to predict the probability of a tax reform $\hat{p}^D_{i,t} \equiv P(D_{i,t}=1|Z_{i,t-1})$, controlling for country fixed-effects α_i^h and lagged macroeconomic conditions $Z_{i,t-1}$. We fail to reject the null that our measure is not predictable. Even though these coefficients are statistically insignificant, we will address related concerns of allocation and omitted variable bias last in next section.[10] We also report the area under curve (AUC) statistic, alongside their p-values, for each specification following **?**. The AUC statistic tests the classification ability of a specification against a random guess. We use these statistics to test the null hypothesis is that each specification's AUC

[10] Table IX in the Appendix shows the predictability of all reforms.

statistic is not statistically different from the AUC of associated with a random guess (0.5). We fail to reject the null for all specification, suggesting that macroeconomic aggregates are poor predictors of radical tax reforms.

Table V – Predictability of Radical Tax Reforms

VARIABLES	(1) ME	(2) ME	(3) ME	(4) ME	(5) ME
Policy Rate	0.01*	0.01	0.01**	0.01**	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Real Output		1.45		0.78	1.08
		(1.44)		(1.54)	(1.22)
Consumer Prices			-1.08	-0.92	-0.60
			(0.77)	(0.84)	(0.67)
Debt/GDP Ratio					0.00**
					(0.00)
Observations	735	735	735	735	735
AUC	0.632	0.652	0.668	0.676	0.712
AUC p-value	0.00118	1.08e-05	5.79e-06	8.35e-07	4.99e-10

Notes: This table reports the average marginal effects and area under curve (AUC) statistics of fixed-effects logit estimators. Consumer prices are only significant in specifications (3) and (4), and we fail to reject the null hypothesis that the AUC statistic is not different from 0.5. Source: Romer and Romer (2010) and authors' calculations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

2.6 Comparison to US narrative evidence

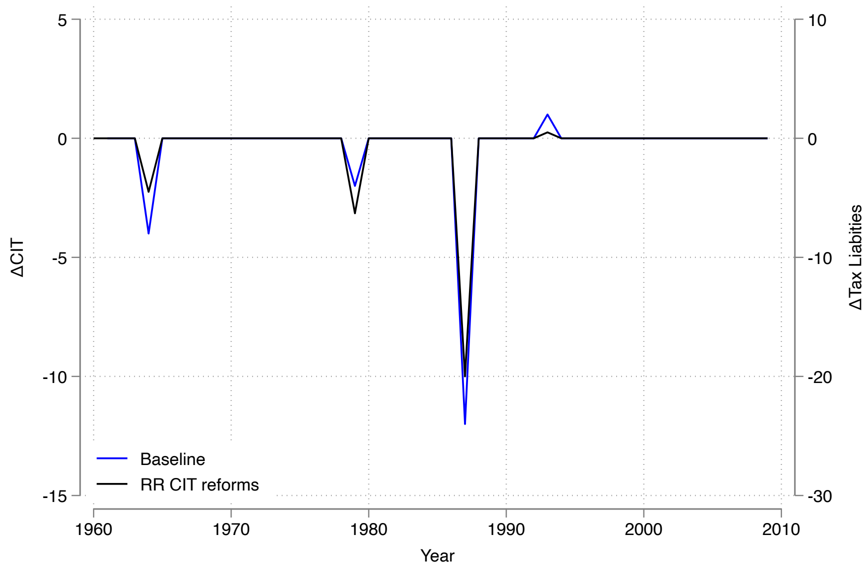
Next, we compare our series of corporate tax reforms in the US with those obtained with the narrative approach in Romer and Romer (2010). This alternative instrument measures the magnitude of tax reforms by changes in aggregate tax liabilities. From their narrative analysis, we isolate those changes related to long-run motivation and only focus on corporate tax reforms. They identify three exogenous changes motivated ostensibly by long-run preferences.[11]

Figure V plots our baseline series against the current value of the change in liabilities of the three exogenous 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

[11] The four bills which affected firm's corporate income tax liabilities are: 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 change in liabilities due to corporate income tax changes come from Romer and Romer (2010) appendix.

Figure V – Comparison with Romer & Romer (2010)



Notes: Source: Romer and Romer (2010) and authors' calculations. Data for 1993 corresponds to Federal Government; Corporate Income Tax Receipts from FRED. Tax liabilities are expressed in current billion USD.

permanent. In addition to this heuristic test, we also check for covariate balance and predictability of negative exogenous changes in US tax liabilities using the Romer and Romer (2010) instrument to benchmark our results. Table VI shows that we again fail to reject the null of equality for contemporaneous variables and their lagged averages. This suggests that covariates are balanced across treatment and control groups.

Table VI – Checking for balance of US Tax Reforms with Romer and Romer (2010) instrument

	Year of Reform		Previous 5 years	
Variable	Difference	p-value	Difference	p-value
Policy Rate	0.31	0.65	−0.10	0.73
Unemployment	−0.50	0.32	−0.50	0.32
Consumer Prices	−0.06	0.90	0.09	0.86
Debt/GDP	1.18	0.21	0.66	0.25
Real GDP	0.10	0.80	−0.10	0.82

Notes: The table shows balance between treatment and control groups are balanced considering negative exogenous US reforms identified by narrative methods. Columns two and three refer to contemporaneous covariates. Columns four and five refer to lagged five year averages of the covariates. All variables are HP filtered (100). Source: Romer and Romer (2010) and authors' calculations. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table VII reports the tax policy regression estimated using the Romer and Romer (2010)

Table VII – Predictability of US Tax Reforms with Romer and Romer (2010) instrument

VARIABLES	(1) ME	(2) ME	(3) ME	(4) ME	(5) ME
Policy Rate	-0.05***	-0.05***	-0.05***	-0.06***	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Real Output		0.79		2.30	2.31
		(3.67)		(3.85)	(4.05)
Consumer Prices			3.74	4.81	3.15
			(2.59)	(3.25)	(2.22)
Debt/GDP Ratio					0.01**
					(0.00)
Observations	55	55	55	55	55
AUC	0.812	0.812	0.826	0.829	0.802
AUC p-value	0.000453	0.000641	0.000149	0.000167	0.00336

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table reports the average marginal effects and area under curve (AUC) statistics of fixed-effects logit estimators. We can neither reject the null that any coefficient is different from zero nor reject the null that the AUC statistic is not different from 0.5. Source: Romer and Romer (2010) and authors' calculations. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

instrument. We again fail to reject the null. Now that we've compared our measure against the baseline narrative measure, the next step is to use it to examine the relationship between tax reform and monetary policy.

3 Monetary Policy Responses to Corporate Tax Reforms

We now use our new tax reform instrument to examine the relationship between tax reform, monetary policy, and other aggregates. First, we estimate our baseline specification, using local projection (LP) methods by Jordà (2005). We then employ three other LP estimators to address concerns relating to omitted variable bias, allocation bias, and anticipatory effects. This section concludes with a discussion of the relationship between our tax reform measure and other aggregates.

3.1 Baseline specification

To investigate the response of the monetary policy rate and other macro variables we use the local projection methods by Jordà (2005) and Angrist, Jordà and Kuersteiner (2018). For each dependent variable y_{it} in country i, year t, and horizon h, we run the following baseline specification:

$$\Delta y_{i,t+h} \;=\; \alpha_i^h \;+\; \gamma t \;+\; \beta^h D_{i,t} \;+\; \varepsilon_{i,t+h}, \tag{5}$$

where $\Delta y_{i,t+h} \equiv y_{i,t+h} - y_{i,t}$, α_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; $D_{i,t}$ is a dummy variable that takes the value 1 if there was a permanent radical tax reform at date t (a reduction of more than 10 pp. in the corporate 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, k). The coefficient of interest is β^h that measures the response of variable of interest h periods ahead.

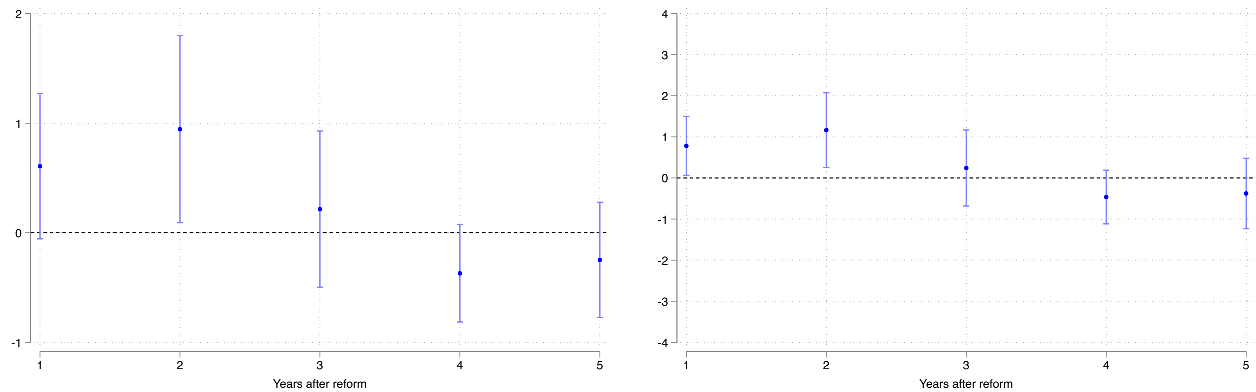
3.2 Main result

Figure VI presents our main result from our simple regression without controls. It plots the coefficient β^h for different horizons h together with 95% confidence intervals. The results suggests that central banks raise policy rates after a radical corporate tax reform. On average, there is a 2% increase in the policy rate after a radical corporate tax reform. We also observe a lag in the Central Banks responses: On average, monetary policy rates peak two to three years after the tax reform. The shape of the response suggests that the policy response is transitory, as rates go back to their average value after 4 years.

We also estimate the effect on prices and output. Figure VII plots the response of output and the price index to the tax reform. While the point estimates suggests that large negative tax reforms increase prices and output, the effects are statistically insignificant. Interestingly, the average GDP and price responses fall coinciding with the monetary policy tightening.

Figure VI – Policy Rate Response to Corporate Tax Cut

(A) All Reforms (B) Radical Reforms (Baseline)



Notes: Policy rate response to corporate income tax reforms. Solid = Coefficient β^h for various horizons h. Dashed = 95% confidence intervals. Panel (A) considers sample of all tax reforms. Panel (B) considers the subsample of radical tax reforms. Both panels show that, on average, policy rates increase two years after a radical corporate income tax cut. Source: authors' calculations, see Appendix A for details.

3.3 Additional Results and Robustness

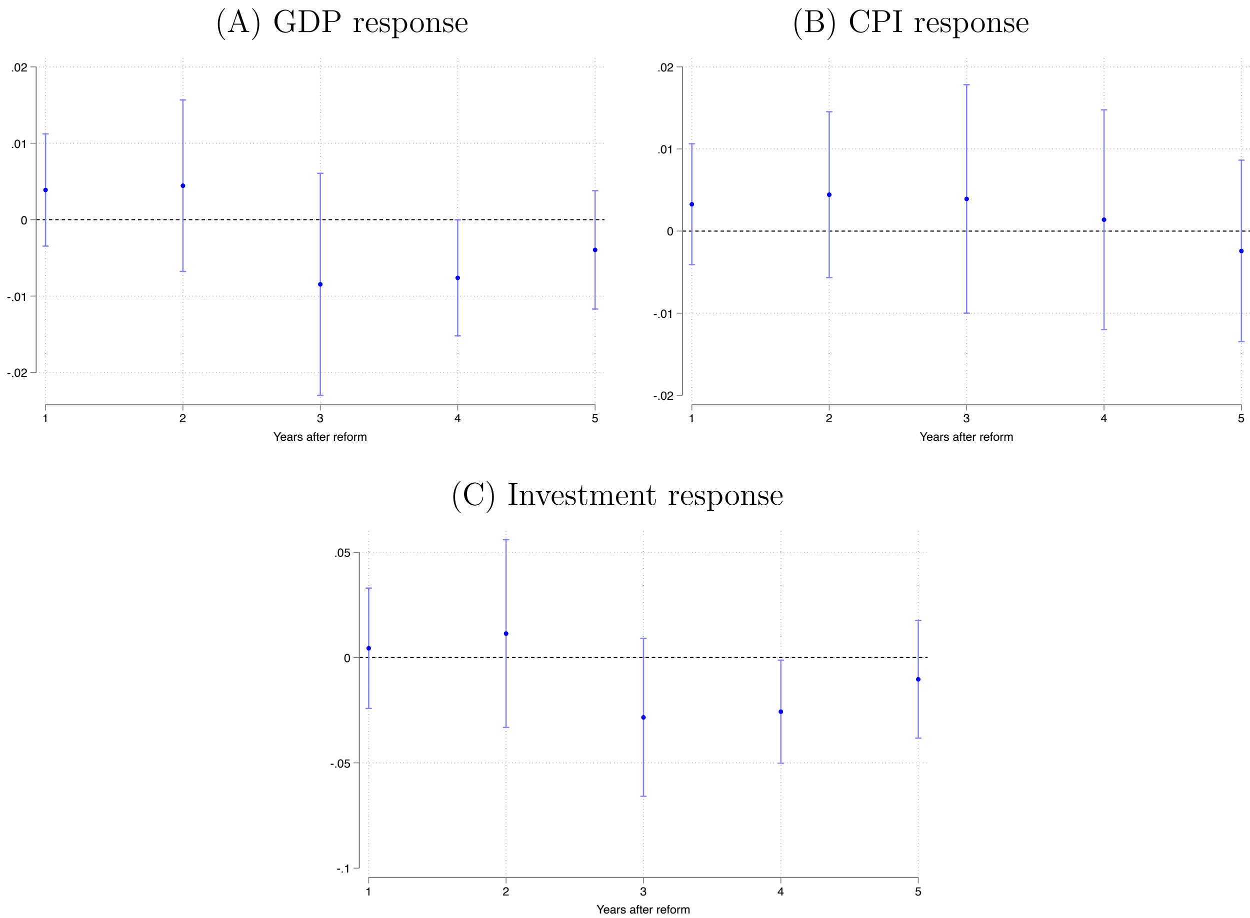
Next, we conduct several robustness checks to evaluate the validity of our main result and to address three vulnerabilities of our baseline specification: omitted variable bias, allocation bias, and anticipatory effects. As preliminary tests indicated that our estimates might suffer omitted variable and allocation bias, we test whether adding controls or inverse propensity weights to our estimator affects its estimates. Then, we test for anticipatory effects (i.e. policy rates move in anticipation of tax reforms) by estimating our local projections backwards.

Omitted Variable Bias. Is the observed policy response explained by macroeconomic variables omitted from our baseline specification? This could occur if our baseline estimator suffers from significant omitted variable bias. To test this possibility, we add to the baseline specification in (5) a set of regressors including macroeconomic controls $Z_{i,t}$, which include the inflation rate, and gross real output.

$$\Delta y_{i,t+h} = \alpha_i^h + \gamma t + \beta^h D_{i,t} + \Phi^h \cdot \Delta Z_{i,t-1} + \varepsilon_{i,t+h}, \tag{6}$$

Figure VIII compares the results with and without the macroeconomic controls. Remarkably, the inclusion of additional macroeconomic controls does not significantly alter the estimated monetary policy response. This result supports our study's assumptions in two key ways. First, it provides suggestive evidence that our baseline specification does not suffer from significant omitted variable bias. Second, it suggests that the monetary policy response to the tax reform goes beyond any

Figure VII – Response of Macro Aggregates to Radical Corporate Income Tax



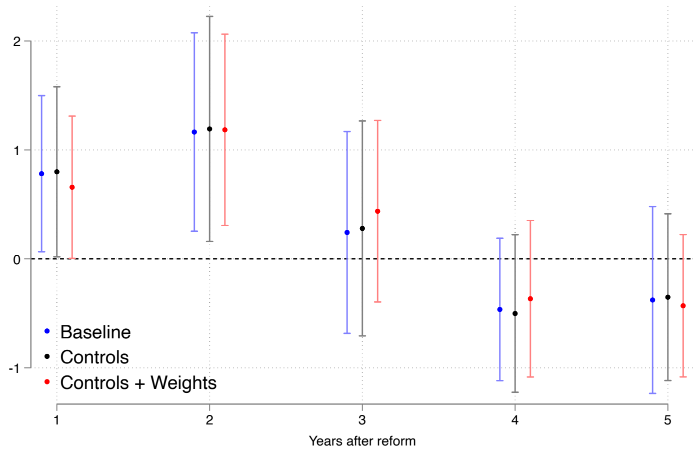
Notes: Output and price responses to radical corporate income tax cut. Solid = Coefficient β^h for various horizons h. Dashed = 95% confidence intervals. Panel (A) considers output response to radical corporate tax reforms. Panel (B) considers price response to radical corporate tax reforms. Panel (A) shows output initially increases following a radical reform, although it falls below its initial level after three years, coinciding with the observed increase in policy rates. Panel (B) shows that prices are stabilized after reforms, on average. Source: authors' calculations, see Appendix A for details.

stabilization of current inflation and output gaps.

Allocation Bias. Are endogenous reforms biasing our estimates? In Section 2.5 we presented evidence that of the reforms in our sample are exogenous. Now, we propose an alternative specification that is robust to bias from selection on observables. This robustness exercise embarks from the null hypothesis that our baseline estimate is not sensitive to selection on observables. We test this hypothesis by comparing our baseline estimator with an inverse propensity weighted regression-adjusted (IPWRA) estimator.

The IPWRA is a two-stage estimator. In the first stage, we use our logit estimator (4) to

Figure VIII – Adding Controls



Notes: Policy rate response to radical corporate income tax cut with and without macroeconomic controls. Solid = Coefficient β^h for various horizons h. Dashed = 95% confidence intervals. The blue estimates depict the policy rate response to radical corporate tax reforms without controls, our baseline measure. The black estimates depict the policy rate response to radical corporate tax reforms with controls. We fail reject the null that these two series do not differ. Source: authors' calculations, see Appendix A for details.

estimate the predicted reform probability $\hat{p}^D_{i,t}$ (policy propensity score, hereafter) for our sample. If our observed sample contains more observations with propensity scores close to 1 than the population average, then the allocation bias resulting from selection-on-observables will be quantitatively relevant. IPWRA estimators are robust to this bias, assuming the estimator is correctly specified, because they reweight the observed sample such that weighted propensity score distribution appropriately conforms to the underlying distribution of scores. To do this, we use the propensity score to construct weights $\hat{w}_{i,t}$:[12] In the second stage, we estimate a local projection with macroeconomic controls and inverse propensity weights.

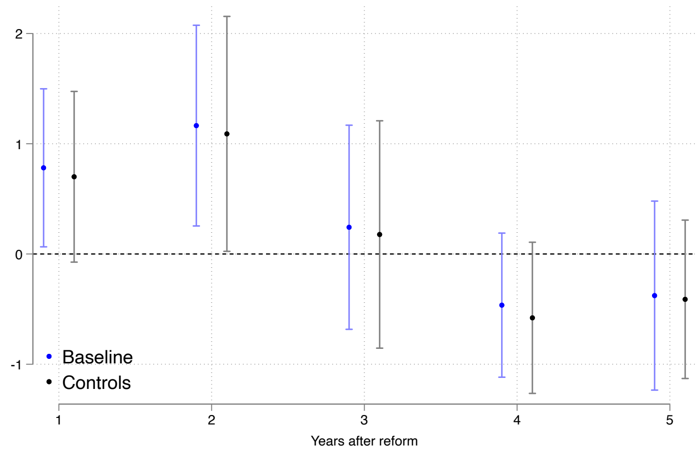
$$\hat{w}_{i,t} \;=\; \frac{D_{i,t}=1}{\hat{P}(D_{i,t}=1)} + \frac{D_{i,t}=0}{\hat{P}(D_{i,t}=0)}. \tag{7}$$

What should we expect graphically? Intuitively, if our baseline sample includes relatively too many observations with propensity scores close to one, then we should expect our baseline estimates to be biased towards the treatment effect of more predictable reforms. Figure IX reports the results of our exercise.

The inclusion of controls and policy weights does not significantly alter our estimates. As a result, we fail to reject the null that our baseline specification introduces allocation bias in our estimates.

[12]Following Richter, Schularick and Shim (2019), we top-code our propensity weights at 10.

Figure IX – Policy weights



Notes: Policy rate response to radical corporate income tax cut with and without controls and rerandomization using weights. Solid = Coefficient β^h for various horizons h. Dashed = 95% confidence intervals. The blue estimates depict the policy rate response to radical corporate tax reforms without controls and rerandomization using weights, our baseline measure. The black estimates depict the policy rate response to radical corporate tax reforms with controls and rerandomization. We fail reject the null that these two series do not differ. Source: authors' calculations, see Appendix A for details.

Anticipatory Effects. To examine whether monetary policy anticipates tax reforms, we estimate the following regression

$$\Delta y_{i,t-h} = \alpha_i^h + \gamma t + \beta^h D_{i,t} + \varepsilon_{i,t-h},, \tag{8}$$

which is our baseline specification but with a lagged regressand instead. For this exercise, our null hypothesis is that monetary policy does not anticipate tax reforms, and as such, β^h should be statistically insignificant. Figure X plots the result of this robustness check. Since the estimated coefficients are statistically insignificant, we fail to reject the null and confirm that our baseline estimator is not biased due to anticipatory effects.

Figure X – Anticipatory Effects

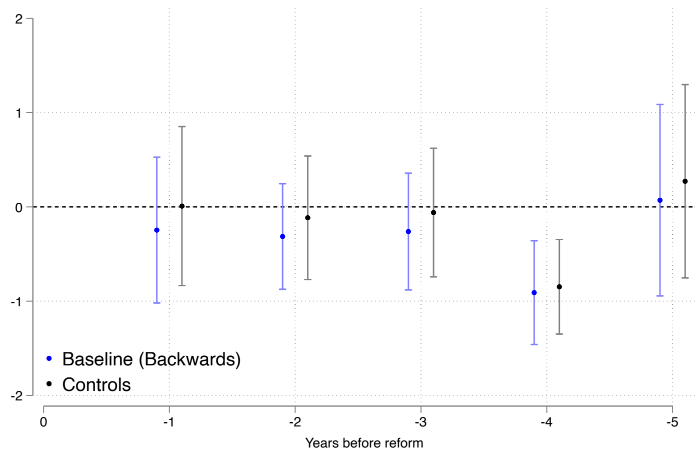


Figure XI – Backward

Notes: Policy rate response to radical corporate income tax cut, backward linear projection with $h < 0$. Solid = Coefficient β^h for various horizons h. Dashed = 95% confidence intervals. The blue estimates depict the policy rate response to radical corporate tax reforms without controls and rerandomization using weights, our baseline measure. The black estimates depict considers the policy rate response to radical corporate tax reforms with controls and rerandomization. We fail reject the null that policy rates within the specified window do not affect future tax reforms at h=0. Source: authors' calculations, see Appendix A for details.

4 A New Keynesian Framework with Corporate Taxes

This section incorporates a New Keynesian framework in Baley and Blanco (2021) following Midrigan (2011), Ottonello and Winberry (2020) and Fang (2021).

Notation. Time is discrete, infinite, and denoted with $s = 0, 1, \ldots$. Financial markets are complete. We denote with Q_s the time–zero Arrow-Debreu price. Let x_s be the discrete set of aggregate shocks at time s in the set X and x^s the history of these shocks in the set $X^s := \prod_s X$. For simplicity, any random variable $Z(x^s)$ measurable with respect to the history of aggregate shocks x^s is denoted by Z_s.

Demography. Seven classes of agents live in the economy: a government, a Central Bank, employment agencies, a final good producer, a capital good producer, and continuum measure one of retailer index with $j \in [0, 1]$, manufacturing firms index with $f \in [0, 1]$, and a measure one of households index with h. Their optimization problems are presented below.

Final good producers. At every point in time s, a perfectly competitive firm produce the final consumption good Y_s combining a continuum of intermediate goods $\hat{y}_s^R(j)$, with $j \in [0,1]$ according to the technology

$$Y_s = \left[\int_0^1 \hat{y}_s^R(j)^{\frac{\gamma_p - 1}{\gamma_p}} \, \mathrm{d}j\right]^{\frac{\gamma_p}{\gamma_p - 1}}. \tag{9}$$

Taking the price of inputs $p_s^R(f)$ and the final good P_s as given, the producer problem entails choosing final-good supply Y_s and input demands $\hat{y}_s^R(f)$ to maximize her profits

$$\max_{\{Y_s, \{\hat{y}_s^R(j)\}_{j=0}^1\}_{s=0}^\infty} \sum_{s=0}^\infty \sum_{x^s \in X^s} Q_s \left[P_s Y_s - \int_0^1 p_s(j) \hat{y}_s^R(j) \, \mathrm{d}j\right] \tag{10}$$

subject to (9).

Retailer. A monopolist retailer produces the good j according to the production function

$$y_s^R(j) = \int_0^1 \left(\frac{\hat{y}_s^M(j,f)}{u_s(f)}\right) \mathrm{d}f, \tag{11}$$

where capital quality $u_s(f)$ decreases the marginal product of the intermediate good f. The monopolistic retailer faces pricing frictions a la Calvo (1983) which we model with a hidden Markov model. Let $n_s(j)$ be a poisson counter with probability $1 - \xi_p$ of unit increment. Then given the firm control variable shadow price $p_s^{R*}(f)$, the retailer price evolve as

$$p_s^R(f) = \begin{cases} p_s^{R*}(f) & \text{if } n_t(j) = n_{t-1}(j) + 1 \\ p_{s-1}^R(f) & \text{if } n_t(j) = n_{t-1}(j) \end{cases} \tag{12}$$

model Calvo (1983) with a hidden Poisson.

Let $\mathbb{E}_0^n[\cdot]$ be the expectation over the stochastic process of $n_t(j)$. Taking the demand schedule $\hat{y}_s^R(j)$, and the price of the price of the input $p_s^M(f)$ as given, together the with stochastic process of possibility of changing the price $n_t(j)$, the firm j chooses price $p_s^{R*}(f)$ and inputs $\hat{y}_s^M(j,f)$ to maximize the the retailer value

$$\max_{\{p_s^{R*}(j), \{\hat{y}_s^R(j,f)\}_{f=0}^1\}_{s=0}^\infty} \mathbb{E}_0^n \left[\sum_{s=0}^\infty \sum_{x^s \in X^s} Q_s \pi_s(j)\right] \tag{13}$$

subject to (11) and (12). Here, $\pi_s(j)$ denotes the retailer's profits given by

$$\pi_s(j) = (1 - t^c)\left[p_s^R(f) y_s^R(j) - \int_0^1 p_s^M(f) \hat{y}_s^M(j,f) \, \mathrm{d}f\right]. \tag{14}$$

Here, t^c denotes the corporate income tax.

Investment good producers. The capital–good firm produces manufacturing firm-specific investment goods $\{i_s(f)\}_{f\in[0,1]}$ in competitive market, according to a linear technology

$$\int_0^1 \left(\frac{\varphi(i_s(j))i_s(f)}{u_s(f)}\right) \mathrm{d}f \;=\; i_s, \tag{15}$$

where

$$\varphi\left(i_s(f)\right) = \begin{cases} \varphi^- & \text{if } i_s(f) > 0 \\ \varphi^+ & \text{if } i_s(f) \leq 0 \end{cases}. \tag{16}$$

The parameters φ^- and φ^+ measure the level of partial irreversibility with $\varphi- > \varphi^+$. i_s is the aggregate investment to produce new capital goods. Taking the prices of firm-specific investment goods $p^k_s(j)$ as given, the capital–good firm problem maximizes her profits

$$\max_{\{i_s(j),i_s\}_{t=0}^{\infty}} (1-t^c)\sum_{s=0}^{\infty}\sum_{x^s\in X^s} Q_s\left(\int_0^1 p^k_{fs} i_{fs}\,\mathrm{d}f - i_s\right), \tag{17}$$

subject to the technology described in (15). Here, t^c denotes the corporate income tax and i_s is the aggregate investment to produce capital. Note that i_{ft} may be positive or negative as there is no technological constraint on its sign.

Employment agency. Firms are owned by a continuum of households. Each household is a monopolistic supplier of specialized labor, $l_s(h)$. A competitive employment agency combine this specialized labor into a homogenous labor input sold to intermediate firms, according to

$$L_s = \left[\int_0^1 \hat{l}^R_s(h)^{\frac{\gamma_w-1}{\gamma_w}}\,\mathrm{d}h\right]^{\frac{\gamma_w}{\gamma_w-1}}. \tag{18}$$

Taking the price of inputs $w_s(h)$ and the final labor supply W_s as given, the producer problem entails choosing final-labor supply L_s and labor input $\hat{l}_s(h)$ to maximize her profits

$$\max_{\{L_s,\{\hat{l}s(h)\}_{h=0}^1\}_{s=0}^{\infty}} (1-t^c)\sum_{s=0}^{\infty}\sum_{x^s\in X^s} Q_s\left[W_sL_s - \int_0^1 w_s(h)\hat{l}_s(h)\,\mathrm{d}j\right] \tag{19}$$

subject to (9).

Households. Financial market are complete. The households supply labor and faces pricing frictions a la Calvo (1983) which we model with a hidden Markov model. Let $n_s(h)$ be a poisson counter with probability $1-\xi_w$ of unit increment. Then given the household control variable shadow wage $w_s^*(h)$, the retailer price evolve as

$$(20) \qquad w_s(s) = \begin{cases} w_s^*(h) & \text{if } n_t(h) = n_{t-1}(h) + 1 \\ w_{s-1}(h) & \text{if } n_t(h) = n_{t-1}(h) \end{cases}$$

The household h chooses the stochastic process of for consumption C_s, one period period state-contingent bonds $B_s(h)$, equity $E_s(h)$, wages $w_s(h)$ subject to the period by period budget constraint

$$\sum_{x_{s+1},\Delta n_{s+1}} B_{s+1}(h) + \int_0^1 P_s(f)\Delta E_{s+1}(f)\,\mathrm{d}f = \mathcal{Y}_s(h) - P_s C_s - t^g \int_0^1 \Delta P_s(f) E_s(f)\,\mathrm{d}f + B_s,$$

$$(21) \qquad \mathcal{Y}_s(h) = R_s + (1-t^p)\left(\int_0^1 D_s(f)E_s(f)\,\mathrm{d}f + i_s B_s + w_s(h) l_s(w_s(h))\right).$$

Here, R_s are lump-sum transfers for the government and the profits of the retailer firms, $D_s(f)$ are firm's f dividend payments, t^p denotes the personal income tax, t^g is the capital gain tax, and $1 + i_s = Q_{s-1}/Q_s$. ΔZ_{s+1} for any random variable Z denotes $Z_{s+1} - Z_s$. Taking prices of asset $\{P_s(f), i_s\}$ and goods P_s as given, and the labor demand schedule $l_s(w_s(h))$, the household problem is to maximize her expected utility (discounted at the rate β)

$$(22) \qquad \mathbb{E}^{n,x^s}\left[\sum_{s=0}^{\infty} \beta^s \left(\log\left(C_s - hC_{s-1}\right) - \varphi \frac{l_s(h)^{1+\nu}}{1+\nu}\right)\right]$$

subject to (20) and (21). Consumption is not indexed by h because the existence of state contingent securities ensures that in equilibrium consumption are the same for all households.

Manufacturing firms. These are the most important agents for the production decision in our economy. Intermediate–good firm $f \in [0,1]$ produces output $y_s(f)$ using capital $k_s(f)$ according to a production function with decreasing returns to scale

$$(23) \qquad y_{fs} = (z_t u_{ft})^{1-\alpha\eta} \left(k_{ft}^{\alpha} \hat{l}_{ft}^{1-\alpha}\right)^{\eta}, \quad \alpha < 1.$$

The firm's total productivity is driven by aggregate z_t and idiosyncratic u_t components. The aggregate productivity grows deterministically at a rate $\mu > 0$,

$$(24) \qquad z_t = (1+\mu) z_{t-1} e^{\sigma_z \epsilon_t^z}.$$

The idiosyncratic productivity follows a geometric Brownian motion with zero drift (w.l.o.g) and volatility σ,

$$u_{ft} \;=\; u_{ft-1}e^{\sigma\epsilon^u_{ft}}, \tag{25}$$

where $\epsilon^u \sim i.i.d.\ N(0, \frac{\sigma_u^2}{2})$. The capital stock, if uncontrolled, depreciates at a constant rate $\zeta^k > 0$.

The firm pays the corporate income tax rate t^c on its cash flow $p_s(f)y_{fs}$ net of deductions $\xi^d k_{fs}$, where ξ^d denotes the deduction rate. Since the physical and the legal depreciation rates differ, we distinguish deductions from the capital stock and denote these with d_{fs}. The state space now includes deductions (k, u, d). The corporate income tax and deductions jointly determine the after–tax profit rate

$$\pi_{fs} \;=\; p_{fs}y_{fs} - t^c(p_{fs}y_{fs} - \xi^d d_{fs}) \;=\; (1-t^c)p_{fs}y_{fs} + t^c\xi^d d_{fs}, \tag{26}$$

and the evolution of deductions

$$\log d_{fs} \;=\; \log d_0 - \xi^d s + \sum_{h:T_{fh}\leq s}\left(1 + \frac{p^k(\hat{i}_{fT_{fh}})\hat{i}_{fT_{fh}} + \theta_{fT_h}}{d_{fT^-_{fh}}}\right). \tag{27}$$

Here, $\theta_s = \theta u_s$ where $\theta > 0$ is constant and $\hat{i}$ is firm's investment.

Taking the prices of the intermediate goods p_{ft}, the marginal investor discount factor Q_t, and firm-specific capital goods $p^k_{ft}(\hat{i})$ as given, together with the adjustment friction θ_{ft}, each firm f chooses a sequence of capital adjustment dates $\{T_{fh}\}_{h=1}^{\infty}$ and investments $\{i_{f,T_{fh}}\}_{h=1}^{\infty}$ to maximize its expected discounted stream of profits

$$\max_{\{T_{fh},\hat{i}_{f,T_{fh}}\}_{h=1}^{\infty}} \mathbb{E}\left[\int_0^{\infty} Q_t\pi_{ft}\ \mathrm{d}s - \sum_{h\,=\,1}^{\infty} Q_{T_{fh}}p^k_{f,T_{fh}}\left(\theta_{fT_{fh}} + p^k_{fT_{fh}}(\hat{i}_{fT_{fh}})\hat{i}_{fT_{fh}}\right)\right], \tag{28}$$

subject to the profits function in (26) and the law of motion for its capital stock

$$\log(k_{ft}) \;=\; \log(k_{f0}) - \zeta t + \sum_{h:T_{fh}\leq t}\log\left(1 + \frac{\hat{i}_{f,T_{fh}}}{k_{T^-_{fh}}}\right). \tag{29}$$

$$y_{ft} = \Gamma^{-1}(z_t u_{ft})^{1-\alpha\eta}\ \left(k^{\alpha}_{ft}\hat{l}^{1-\alpha}_{ft}\right)^{\eta}. \tag{30}$$

The firm's total productivity is driven by aggregate z_t and idiosyncratic u_t components.[13] The

[13] We set $\Gamma = ((1-\alpha)\eta)^{(1-\alpha)\frac{\tilde{\alpha}}{\alpha}}\frac{\eta\alpha}{\tilde{\alpha}}$ where $\tilde{\alpha} \equiv \frac{\eta\alpha}{1-(1-\alpha)\eta}$.

aggregate productivity grows deterministically at a rate $\mu > 0$,

$$z_t = (1+\mu) z_{t-1} e^{\sigma_z \epsilon_t^z}. \tag{31}$$

The idiosyncratic productivity follows a geometric Brownian motion with zero drift (w.l.o.g) and volatility σ,

$$u_{ft} = u_{ft-1} e^{\sigma \epsilon_{ft}^u}, \tag{32}$$

where $\epsilon^u \sim i.i.d.\ N(0, \frac{\sigma_u^2}{2})$. The capital stock, if uncontrolled, depreciates at a constant rate $\zeta^k > 0$.

The firm can control its capital stock through purchasing or selling capital. For every change in its capital stock (investment) $\hat{i}_{ft} \equiv \Delta k_{ft}$, the firm must pay an adjustment cost θ_{ft} that is proportional to aggregate productivity. The value of investment is given by $p_{ft}^k(\hat{i})\hat{i}$, where the price of capital $p_{ft}^k(\hat{i})$ depends on the firms' investment level.[14] The adjustment cost are random and they are different for positive and negative investments. Concretely, the adjustment cost takes the form

$$\theta_{ft} \equiv \Theta\left(\hat{i}_{ft}, \vartheta_{ft}\right) \mathcal{I}\left(\hat{i}_{fT_{ft}}\right) z_t, \tag{33}$$

where $\vartheta_{ft} = (\vartheta_{ft}^+, \vartheta_{ft}^-)$ are i.i.d. random variables with cumulated distribution $F^{\pm}(\vartheta)$ and support $[0, \bar{\theta}^{\pm}]$. The function $\Theta\left(i_{ft}, \vartheta_{ft}\right)$ takes the following values:

$$\Theta\left(\hat{i}_{ft},\ \vartheta_{ft}\right) \equiv \begin{cases} \vartheta_{ft}^- & \text{if } \mathcal{I}^-\left(\hat{i}_{fT_{ft}}\right) = 1, \\ \vartheta_{ft}^+ & \text{if } \mathcal{I}^+\left(\hat{i}_{fT_{ft}}\right) = 1. \end{cases} \tag{34}$$

Central Bank.

Government. Since Ricardian equivalence holds, we assume the government follows a period by period balance budget without loss of generality. The period expenditures, given by the lump-sum

[14] For any stochastic process q_t, we use the notation $\Delta q_t = q_t - q_{t^-}$ to denote the limit from the left, where $q_{t^-} \equiv \lim_{s \uparrow t} q_s$. We use the notation $\mathcal{I}\left(i_{fT_{ft}}\right)$ (resp. $\mathcal{I}^-\left(i_{fT_{ft}}\right)$ and $\mathcal{I}^+\left(i_{fT_{ft}}\right)$) for the indicator function of non-zero (resp. positive and negative) investments.

transfers R_s, has to be equal to the revenue from the firms $\int_0^1 T_{js}\,\mathrm{d}f$ and the household T_s^h

$$\begin{aligned} R_s &= \int_0^1 T_{fs}\,\mathrm{d}f + T_s^h, \\ T_s(f) &= t^c\left(p_s(f)y_s(f) - \xi^d d_s(f)\right), \\ T_s(j) &= t^c\left(p_s(f)y_s(f) - \xi^d d_s(j)\right), \\ T_s^h &= t^p\left(\int_0^1 D_s(f)E_s(f)\,\mathrm{d}f + i_s B_s\right) + t^g \int_0^1 P_s E_s(f)\,\mathrm{d}f. \end{aligned} \tag{35}$$

4.1 Quantitative analysis

In progress.

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A Data Appendix

A.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.

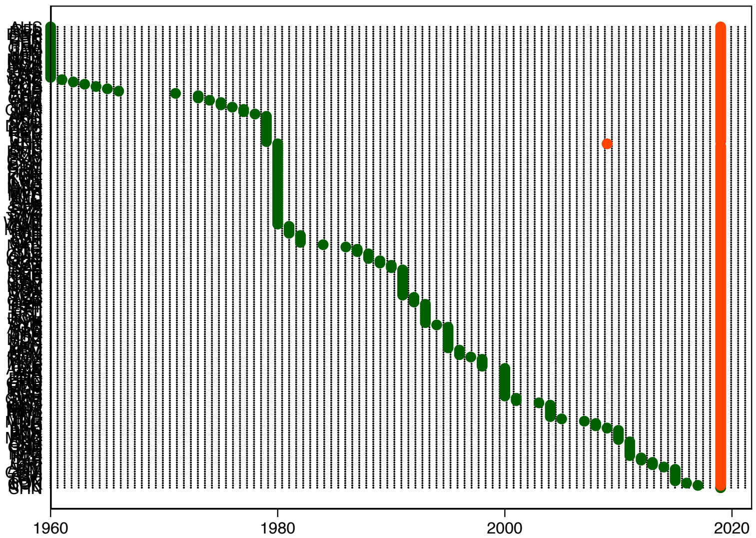
Data Cleaning Data cleaning is intentionally as naive as possible. First, we interpolate on one variable, the debt to GDP ratio variable, because there are a number of breaks in otherwise well-behaved series. We then drop certain outlier periods prior to applying a time-series filter, so that the filter does not create a systematic relationship between periods with extremely different economic regimes and cyclical dynamics. At the moment, we drop observations from: Brazil prior to 1995 (hyperinflation), Russia prior to 2000 (liberalization), Turkey prior to 2004 (no price stabilization), and CHL prior to 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.

We finally construct variables of interest (e.g. log real output) and then apply a Hodrick-Prescott filter to each variable using a smoothing parameter of 100. We know 6.25 is more scientific, while 100 is used in certain parts of the literature, so we can try out various options. We then merge it with the LumpyTaxes 1 dataset. The resulting dataset corresponds to the map that is mostly blue. From here, we refine the sample so that the included country-year observations reflect a common set of monetary policy priorities currently shared by a subset of countries.

Sample Selection Currently sample selection features two steps. First, we exclude countries with insufficient coverage of certain main variables. At the moment, we use four coverage threshold rules. First, we exclude any country for which we observe corporate income tax policy for fewer than 10 years. Second, we exclude any country with less than one corporate tax reform. Third, we exclude any country for which we observe monetary policy data for fewer than 8 years. Lastly, we exclude any reform with a sufficient number of leads (currently 5) and lags (currently 1).

After excluding countries that do not meet these coverage thresholds, our remaining sample features countries which hold a variety of different monetary and fiscal policy perspectives. Our research design implicitly assumes common fiscal and monetary policy link functions across countries. As such, our baseline specification should include countries that exhibit similar fiscal and monetary priorities and behaviors. At the moment, we are excluding non-OECD countries and Iceland at this point. Iceland has admitted to being too small to engage in independent monetary policy. Now there are plenty of more scientific approaches here. We am working on compiling a list of different measures of fiscal and monetary priorities that we could use in this step.

Figure XII – Sample Period and Reform Size



Notes:

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.

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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

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

A.2 Additional results

Table IX – Predictability of All Permanent Negative Tax Reforms

VARIABLES	(1) ME	(2) ME	(3) ME	(4) ME	(5) ME
Policy Rate	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Real Output		1.13	1.13	1.13	1.13
		(1.00)	(1.00)	(1.00)	(1.00)
Observations	932	932	932	932	932
AUC	0.577	0.575	0.626	0.623	0.638
AUC p-value	0.0284	0.0280	5.11e-05	6.18e-05	1.49e-05

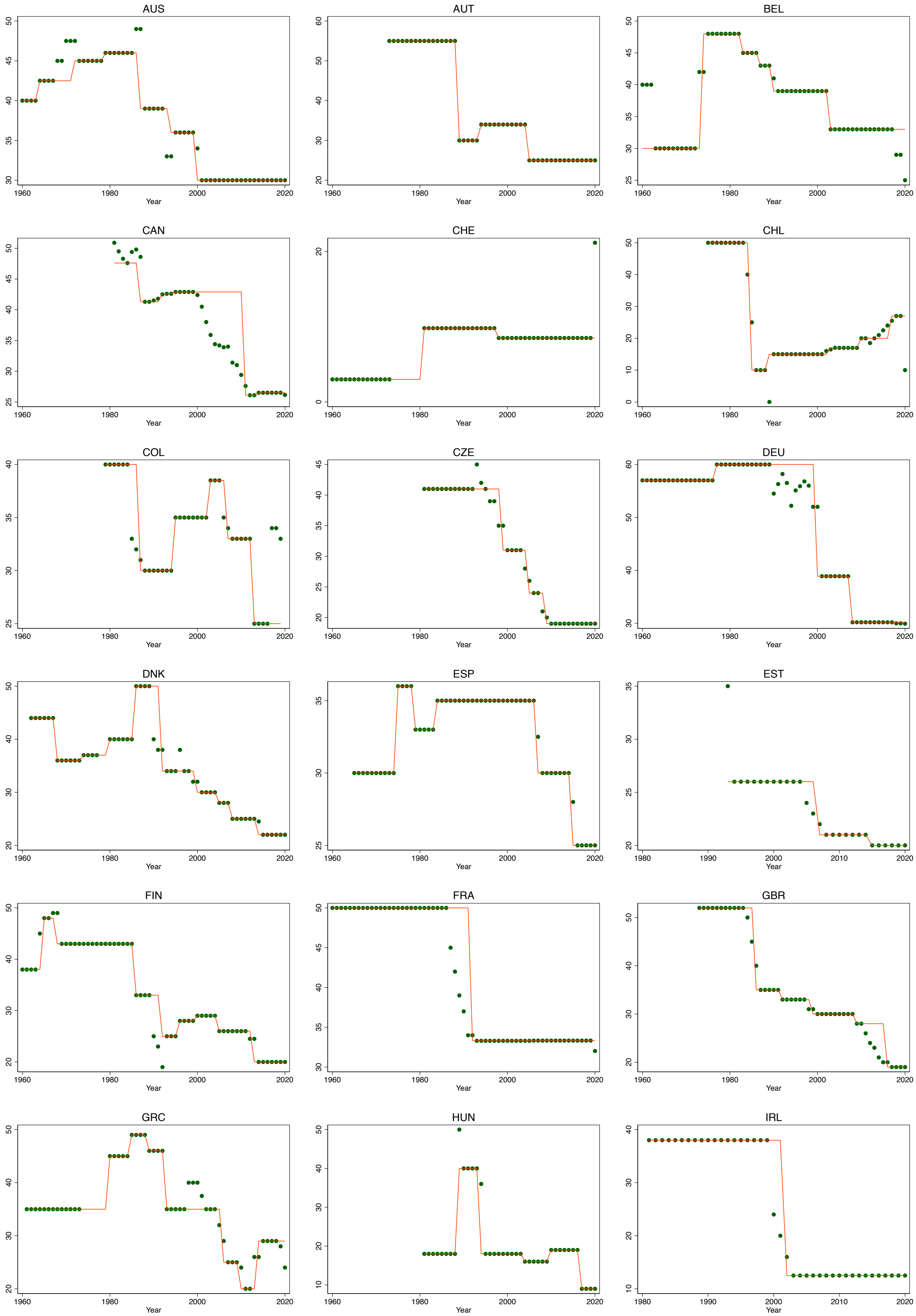
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

B Filters

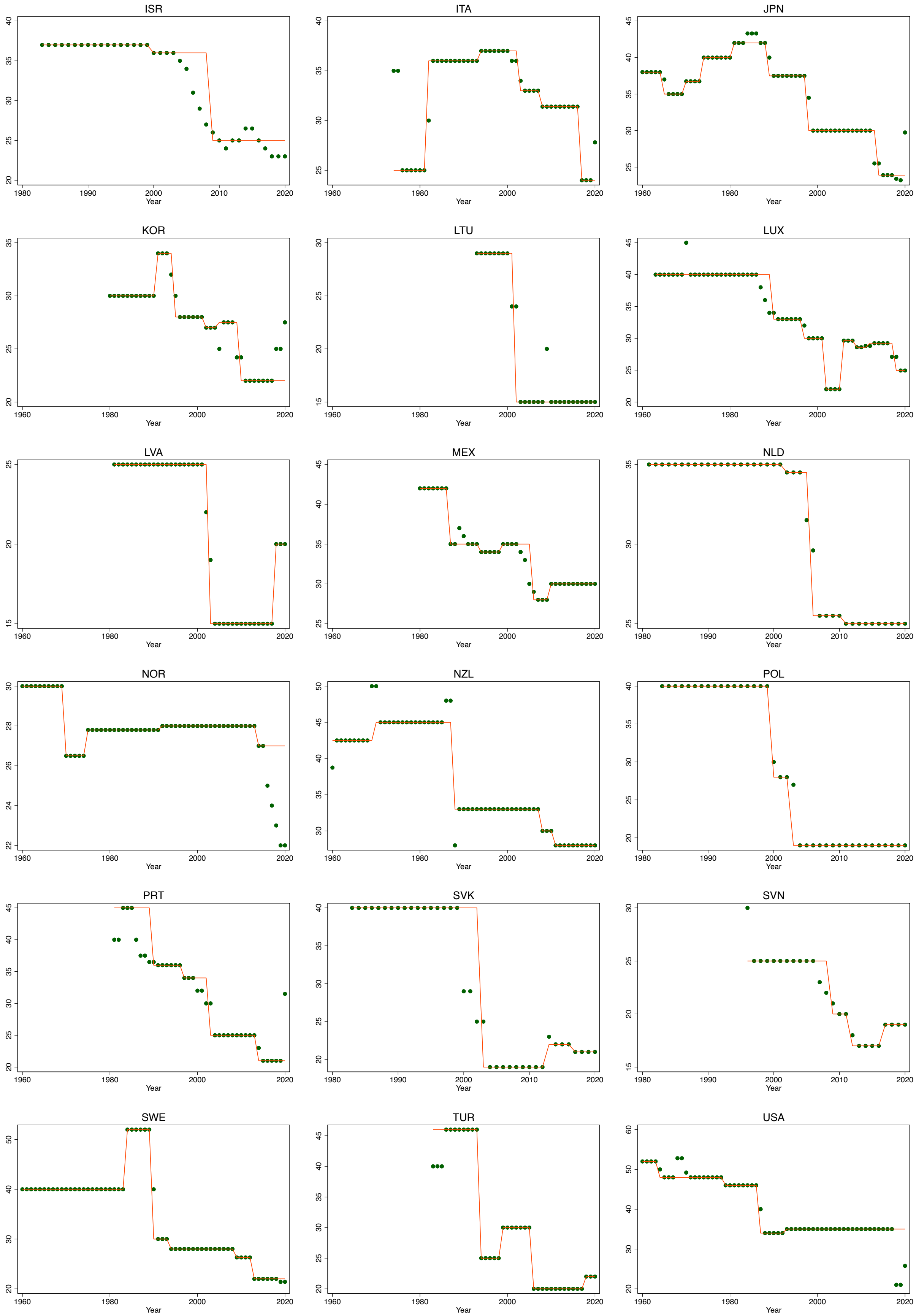
B.1 Permanent tax reforms by country

Figure XIII – Permanent Tax Reforms by Country



Notes: Raw and filtered series.

Figure XIV – Permanent Tax Reforms by Country (cont...)



Notes: Raw and filtered series.