Essays on Investment Dynamics, Corporate Taxation, and Industrial Relations

by

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For my parents and brothers

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TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

Table

LIST OF APPENDICES

Appendix

ABSTRACT

This dissertation contains three essays on investment dynamics, corporate taxation, and industrial relations. Chapter I studies the effect of Right-To-Work policies on local unions and firms. Chapter II studies the effect of permanent corporate tax reforms using a new cross-country panel of corporate taxation. Chapter III examines the implications of specification choice for measures of the conditional volatility of aggregate investment.

In Chapter I "Labor Organizations and Corporate Leverage: The Effect of Right-To-Work Laws" I use the staggered rollout of Right-to-Work (RTW) policies, which prohibit the collection of mandatory agency fees, to examine the relationship between labor union's financial conditions and firms' capital structure. I use a novel data set that links large public corporations' financial reports to unions' balance sheet data. I find that in the years following RTW, unions that previously relied on agency fees experience a significant loss of revenue before they report a significant loss of membership. Unions exposed to RTW policies, through their reliance on agency fee income, respond to the loss of income by decreasing their spending on operations and increasing the income share of membership contributions, which increase the relative cost of union services on members. I show that large public firms have greater leverage when they are exposed to unions with strong financial balance sheets.

In Chapter II "A New Measure Global Series of Corporate Tax Shocks " Isaac Baley, Andrés Blanco, and I propose a methodology to identify tax reforms that are 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-run aggregate effects.

In Chapter III "Cyclicality of Investment Volatility: Implications of Specification Choice" I estimate the conditional volatility of aggregate investment and its components as an autoregressive-moving-average process (ARMA) using standard generalized autoregressive conditional heteroskedasticity (GARCH) estimators. I show that the aggregate volatility of total and equipment investment are acyclical and exhibit low persistence, while the volatility of structure investment is countercyclical and exhibits high persistence. I benchmark my results to prior estimates of the cyclicality of the volatility of aggregate investment in US data and simulated environments. This comparative analysis shows that other specifications in the literature are sensitive to sample selection and outliers.

CHAPTER I

Labor Organizations and Corporate Leverage: The Effect of Right-To-Work Laws

1.1 Introduction

Do labor regulations affect firms' capital structure? While a large literature shows that labor regulations determine firms' financing decisions, these studies rely on the assumption that labor regulations affect workers or firms directly. While this assumption is made to address data limitations, it does not always reflect the intended target of some policies. Certain labor regulations target labor unions specifically. For example, the Taft-Hartley Act of 1947 permitted the inclusion of agency shop clauses in collective bargaining agreements. Agency shop clauses allow unions to collect mandatory fees from workers who are not members of a union (known as agency fee payers). Since a union's ability to finance operations determines their workers' bargaining power, labor unions' financial conditions should mediate the effect of labor regulations on firms' capital structure.

In this paper, I use the staggered rollout of Right-to-Work (RTW) policies, which prohibit the collection of agency fees, to examine the relationship between labor union's financial conditions and firms' capital structure. I use a novel data set that links large public corporations' financial reports to unions' balance sheet data. This research design uses a differential exposure design to measure effect of RTW on unions' revenue and a panel regression to estimate the relationship between unions' revenue and firms' leverage. Together, these estimates allow a subsequent counterfactual analysis of the effect of RTW on firms' capital structure.

First, I use a differential exposure design to estimate the direct effect of RTW on unions' balance sheets. As the Taft-Hartley Act of 1947 only permitted the inclusion of agency shop clauses in collective bargaining agreements, there is intra-state heterogeneity in the pre-treatment take-up of agency shop clauses by unions and firms. The take-up of agency shop clauses is not systematically correlated with firms' capital structure or unions' membership or financial conditions. As RTW prohibits the mandatory collection of agency fees through agency shop clauses, unions' exposure to RTW depends on whether they benefited from an agency shop clause prior to RTW. As such, I measure the differential effect of RTW on unions with agency shop agreements prior to RTW. The identifying assumption of this design is that, in the absence of RTW laws, trends in agency shop agreements would be the same across states.

Second, I use a panel regression to estimate the cross-sectional relationship between unions' balance sheets and firms' leverage. Public firms often distribute economic activity across different jurisdictions in response to regional variation in economic regulations, input prices, and output demand. This behavior makes it difficult to identify the causal effect of RTW on firms' leverage. RTW affects local unions' ability to organize workers at the establishment level, and a firm's leverage reflects strategic decisions made at the firm level. As such, I use a panel regression to recover the semi-elasticity of a firm's leverage to unions' revenue. This panel regression and subsequent counterfactual analysis assumes that firms do not change their allocation of their operations in the short run. I then use estimates from the panel regression and event study estimates to construct the counterfactual effect of RTW on a unionized firms with business activity limited to one state. This counterfactual analysis applies to a counterfactual, average firm with operations limited to one state that unexpectedly enacts RTW. This analysis offers a useful benchmark of the effect of RTW on firms' capital structure.

The effect of RTW on unions' balance sheets is large and persistent. Unions with agency fee payers lose 15 percent of their total receipts over five years relative to unions without agency fee payers. This persistent decline in receipts accompanies a proportional fall in total disbursements. As liabilities and assets decrease, the unions' net worth remain constant. To make up for lost revenue, unions who lost agency fee payers relied on members and their affiliates including intermediate and national labor organizations for financial support. Interestingly, affiliate contributions to unions with unexposed balance sheets falls by 5% during the same period. This asymmetric response suggests that labor federations use intra-state transfers to help insure local unions against income shocks. While affiliates were able to partially insure against the income shock associated with RTW, unions with agency fee payers lose 16% of their membership over the five year sample period, while unions without agency fee payers maintain their pre-reform membership level.

For the average firm in the sample, the decrease in exposed union income following with RTW is associated with a 0.5 standard deviation decrease in firm leverage, assuming that the firms' activity is limited to a state affected by the reforms included in the sample. The counterfactual effect of RTW on firms' leverage is consistent with prior estimates found by Matsa (2010) and Chava et al. (2020). This relationship between unions' revenue and firms' leverage provides novel insights on the determinants of workers' bargaining power. Additionally, firms' leverage decisions are not responsive to union's net worth. As unions have limited ability to finance operations due to their limited collaterizable capital, a decrease in a union's income decreases their ability to invest in bargaining campaigns and membership drives, which further decreases worker bargaining power within firms. More broadly, this result shows how the financial constraints of firm stakeholders shape the distribution of surplus within a firm.

These results also respond to persistent disagreement surrounding the effect of RTW on local unions. As RTW outlaws the collection dues from nonmembers, the reform increases the relative cost of union services for workers employed in positions covered by a collective bargaining agreement. When workers are no longer required to pay for a union's services, they may decide to relinquish their membership. As they benefit from a collective bargaining contract without paying dues (known as "free-riding"), relinquishing their membership is a more financially attractive option in the short-run. The economics literature argues that workers' relative preference for union services, the union-wage premium, non-pecuniary benefits of unions, and workers' income also determine the effect of RTW across states Moore and Newman (1985). Contrary to the economics literature, national labor representatives argue that the effect of RTW on unions depends primarily on local organizers' ability to increase the salience of the long-term costs of free-riding. Specifically, labor representatives highlight the need to push back against "drop your membership" campaigns (see Will (2018)). These campaigns seek to increase the salience of the short-term benefits of free-riding in states that have recently enacted RTW. National labor representatives have also dismissed concerns that losing mandatory agency fee payers could have a significant effect on their federations' income, which would increase the relative cost of representational services for members (see Weingarten et al. (2018)). This paper responds to this disagreement and finds an answer somewhere in the middle. In the years following RTW, unions with agency fee payers experience a significant loss of revenue before they report a significant loss of membership. Unions exposed to RTW respond by decreasing their spending on operations and increasing the income share of membership contributions, which increases the relative cost of union services for members.

This project contributes to three strands of the literature. First, it contributes to a broad literature on the relationship between labor regulations and corporate capital structure. This literature has persisted at the intersection of corporate finance and industrial relations. Bronars and Deere (1993) measure a correlation between unionization rates and leverage across industries. They emphasized that firms increase leverage in response to unions' bargaining power, thus diminishing workers' ability to negotiate for a larger share of firm revenue. Matsa (2010), Chava et al. (2020), and Simintzi et al. (2015) consider the relationship between labor protection policies and firms financing decisions. Matsa (2010) considers the link between workers' bargaining power and firms' use of debt financing. Chava et al. (2020) shows that firms increase leverage following changes in the minimum wage, expanding on Matsa (2010). Simintzi et al. (2015) show how labor regulations increase firms' leverage, constraining their investment decisions. I contribute to these results by showing the effect of labor organizations' financial health on firms' outcomes.

Second, this project contributes to the corporate finance literature linking input and product markets to firms' capital structure. In her study of supermarkets, Chevalier (1995) shows that supermarkets are more likely to enter into markets wherein their potential competitors have higher leverage. In their study of LBOs, Brown et al. (2009) observed that increasing leverage does not lead to a significant decrease in the price of inputs charged by suppliers. As unions have limited ability to secure external finance, this project contributes to these results by measuring the effect of input market constraints on firms' capital structure.

Third, my paper contributes to the literature on the economic effects of unionization and RTW policies. Moore and Newman (1985) summarizes the early literature on the economic effects of RTW, emphasizing the challenge of addressing the correlation between the passage of RTW policies and unionization rates. Specifically, RTW and unionization rates are correlated with anti-union sentiments. Carroll (1983) and Ellwood and Fine (1987) use measures of gender diversity, democratic control of the legislature, and public education bargaining policies to control for the effect of anti-union sentiments with mixed results. To control for state-level variation in anti-union sentiments, Fortin et al. (2022) uses a similar differential exposure design to the one employed in this paper. They measure the effect of RTW by measuring the response of wages in industries with high unionization rate relative to wages in industries with low unionization rates following the reform, as industry-level unionization rates do not vary systematically across their treatment and control groups. Ellwood and Fine (1987) find that RTW decreases the flow into membership through certified elections. Zullo (2020) shows that RTW decreases the number of decertifying elections, where members of the bargaining unit vote to remove a union from the workplace, as non-members are no longer required to support the union financially. This project contributes to this literature by highlighting the importance of union revenue as the primary mechanism through which RTW affects the provision of union services and unionization rates.

1.2 Data Description

In this section, I discuss the data that I use to estimate the effect RTW policies on unions and large private firms. The analysis relies on a sample of five RTW reforms in Wisconsin, Michigan, Indiana, Kentucky, and West Virginia. The reforms in my sample occurred between 2012 and 2017 and resulted from legislative decisions. This project uses a novel data set that links large public corporations' financial reports from Compustat to administrative unions' balance sheet data from LM-2 filings. Administrative data on local unions' membership composition allow me to measure each union's differential exposure to RTW given their pre-treatment take-up of agency fees. These data also include financial information on unions' income sources and spending patterns, which allow for research designs that study the effect of RTW on unions' financial health. Data on large public corporations' financial reports, once linked with local union data, allow me to estimate each firms' exposure to local unions.

Local unions, intermediate bodies, and national labor organizations must file an annual report to the United States Department of Labor (DOL), in accordance with the Labor Management Reporting and Disclosure Act (LMRDA) of 1959. This act covers labor organizations in the private sector, as well as federal agencies and the US postal service. Labor organizations' reporting requirements vary by the level of their annual total receipts and governance structure.

A labor organization with annual gross receipts greater than $250,000 must file an LM-2, unless they are in trusteeship (i.e. have relinquished governance to an intermediate or national organization). On the LM-2, unions must report detailed information on their income sources and their outlays, including the number of agency fee payers in their bargaining unit and their spending on representational services, personnel, and overhead. Organizations with less than $250,000 in receipts must file an LM-3, and organizations with less than $10,000 in receipts must file an LM-4. These annual report includes detailed data on spending on representational services, personnel, and overhead but does not include data on the amount of agency fee payers. As such, I restrict my baseline sample to include only local unions with greater than $250,000 in annual reports. This threshold is low enough to include local unions relevant to the study of firm dynamics.

To match labor organizations to relevant firms, I use separate administrative collective bargaining data collected by the DOL and the Federal Mediation and Conciliation Service (FMCS). Congress formed the FCMS in 1947 to expedite the resolution of labor disputes, as a neutral body to support unions and employers engaged in bargaining, arbitration, and mediation. When a union or employer wants to negotiate, modify, or terminate a collective bargaining agreement, they must file a notice of bargaining (an F-7) with the Federal Mediation and Conciliation Service (FMCS). These forms include data on the union membership, bargaining unit size, and location of operation. Unions and firms are also encouraged, but not required, to submit CBAs to the DOL. The DOL collects CBA's from public and private firms, in accordance with Section 211(a) of the Taft-Hartley Act "for the guidance and information of interested representatives of employers, employees, and the general public." As the National Mediation Board (NMB) is responsible for collecting the CBAs for firms in the railroad or airline industries, DOL does not collect CBAs from these firms. These CBA reports include data on the local unions affiliation and operation location, although data on the specific local unions is sometimes missing or incomplete.

The firm and union data included in F7 and CBA reports allow me to construct a crosswalk between union's annual reports and firms financial reports. I use a fuzzy matching algorithm to match employer names in CBA and F7 filings to financial reports in Compustat. I use a records-linking algorithm to match local unions' affiliations and addresses in the CBA and F7 filings to local unions' annual reports. I then verify the validity of the crosswalk by hand. As a result, the crosswalk also includes data on the distribution of firm operations across state lines. Since RTW policies only affect local unions within the state, the location of a firm's chief executives does not determine their exposure to RTW. As such, this crosswalk improves upon prior estimates of the effects of RTW using Compustat data. A firm's location reported in Compustat is the address of their principal executive offices. The location of a firm's principal executive offices does not necessarily signal a firm's exposure to collective bargaining reform and other labor regulations. Appendix A.1 discusses the construction of the matched panel in detail.

This project relies on two separate samples. First, I estimate the effect of RTW on union balance sheets using LM-2 data from 2007-2019. I restrict my sample to 2007-2019, as the DOL began collecting more detailed data on agency fee payers in 2007. I limit the end of the sample to 2019 to avoid variation due to the coronavirus pandemic. Second, I estimate the cross-sectional relationship between unions' balance sheets and firms' leverage using a matched LM-2 and Compustat panel. I use an extended sample from 2000-2019 to take advantage of the entire span of LM-2 data on unions' balance sheets.

Prior to the construction of the second panel, I clean the Compustat data using the following steps. I remove financial and utility firms from my sample (i.e. SIC 6000-6999, 4900-4999), in accordance with the data cleaning practices employed Chava et al. (2020), which is a combination of those used in Vuolteenaho (2002) and Whited and Wu (2006). I also winsorize each variable by 0.5% to diminish the effect of outliers on the estimates. I also drop firms with equity valuations below \$10 million dollars in 2009 dollars. I also drop firms with negative total assets, total liabilities, long-term debt, employees, cash, and dividends. In accordance with the sample selection of Chava et al. (2020), I filter out firm observations that are likely the result of mergers or acquisitions by dropping observations in which capital expenditures are greater than 50% of total property, plant, and equipment owned by the firm. Table 1.1 reports the summary statistics of all firms in the sample.

Table 1.1 – Firms in Matched Panel

	Mean	Std. Dev.	10^{th}	25^{th}	Median	75^{th}	90^{th}
Leverage	0.407	1.191	0.000	0.021	0.208	0.408	0.693
Tobin's Q	4.167	15.325	0.895	1.111	1.583	2.719	5.430
Tangibility	0.245	0.252	0.017	0.051	0.143	0.368	0.684
Log Sales	5.173	2.756	1.560	3.503	5.481	7.124	8.424
Profitability	-0.211	1.426	-0.529	-0.060	0.080	0.140	0.205
Observations	77786						

Notes: This table reports summary statistics for firms in the matched LM-2 and Compustat sample. Observations reported are firm-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports union financial and membership data at the union level.
Sources: DOL, FCMS, Compustat and author's calculations.

Table 1.2 reports the summary statistics of the baseline sample constructed using LM-2 Data. The two analysis samples provide insight into the role of agency shop agreements in unions' financial agreements and firms' leverage decisions. Most unions in the sample do not report agency fee payers. Around 26% of the unions in the sample ever report agency fee payers, and make up around 20% of the total union by year observations.

Table 1.3 reports the summary statistics of unions with and without agency fee payers in this sample. Unions with agency fee payers are more financially stable relative to unions without agency fee payers on average, although these differences are not statistically significant. Unions with agency fee payers report annual receipts of 7.74 million annually, while unions without agency fee payers report 2.39 million in receipts. Unions with agency fee payers also tend to be larger. The average union with agency fee payers reports an average membership of 12472 workers, while unions without agency fee payers report 3686 members. This difference in receipts and membership does not necessarily correlated with greater receipts per members. On average, unions without agency fee payers report $2600 more income per member than unions with agency fee payers. That said, these differences in income and membership across these two groups translates to differences in financial health and organizing capacity. Unions with agency fee payers have greater assets and net worth, relative to

Table 1.2 – Unions in Baseline Sample

	Mean	Std. Dev.	10^{th}	25^{th}	Median	75^{th}	90^{th}
Receipts	329.06	843.05	29.20	44.27	90.90	236.67	623.22
Disbursements	321.07	831.33	27.87	42.86	88.20	228.38	603.58
Assets	356.07	902.07	13.85	34.94	89.72	257.87	751.73
Rec / Members	0.31	3.29	0.03	0.05	0.08	0.17	0.31
Liabilities	39.95	165.32	0.00	0.04	1.27	9.95	56.83
Net Worth	292.08	673.36	10.73	30.79	80.93	231.27	660.57
Members	5160.51	16477.75	255.00	532.00	1120.00	2696.00	8116.00
AFP Rate	0.50	2.26	0.00	0.00	0.00	0.00	0.42
Representation	71.66	179.07	1.55	7.66	19.80	53.53	147.94
Political	6.60	25.45	0.00	0.00	0.34	2.35	10.28
Overhead	40.01	109.97	0.94	4.21	11.31	28.66	75.21
Observations	74506						

Notes: This table reports summary statistics for local unions in the baseline LM-2 sample. Variables are winsorized at the 0.5% level. Observations reported are union-year observations. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2007-2019. Table reports union financial and membership data at the union level.
Sources: DOL, FCMS, and author's calculations.

unions without agency fee payers. Unions with agency fee payers also tend to be larger and spend more on general overhead, representational services, and political activities. While these differences suggest that unions with agency fee payers tend to be larger unions with greater organizing capacity, these differences are insignificant. I discuss the implications of these summary statistics for the research design in the next section.

In the matched Compustat and LM-2 panel, these differences persist. The sample selection decision to use large public corporations to measure leverage selects unions that are larger and more likely to have agency fee payers. I am able to match around 13 percent of the first sample to Compustat firms. This share is reasonable, given that larger firms are more likely to be unionized. The matching procedure does not affect the average union included in the sample. Appendix A.1 presents the summary statistics for unions in the matched panel, as well additional discussion of the matching procedure and distribution of unions across industries.

The matched sample of Compustat and LM-2 data is consistent with prior work on the relationship between unionized firms and non-unionized firms. Table 1.4 reports the summary statistics of unionized and nonunionized firms in this sample, and Table 1.5 reports the summary statistics of unionized firms without and without agency fee payers.

Unionized and nonunionized firms in the matched patterns exhibit differences in invest-

Table 1.3 – Unions with and without Agency Fee Payers in Baseline Sample

	Reported Agency Fee Payers			No Agency Fee Payers		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Receipts	774.20	1466.71	215.00	239.31	610.19	80.34
Disbursements	757.73	1444.75	209.22	233.04	602.88	77.83
Assets	725.82	1511.07	145.72	281.53	695.95	82.56
Liabilities	118.33	305.62	5.79	24.15	111.91	0.95
Rec / Members	0.10	0.27	0.07	0.36	3.60	0.09
Net Worth	511.40	1030.61	118.20	247.86	564.85	75.67
Members	12472.40	27149.02	3046.00	3686.34	12834.05	966.00
Representation	180.81	310.85	57.73	49.66	127.15	16.59
Political	18.23	45.81	1.34	4.25	17.95	0.23
Overhead	96.10	197.13	22.10	28.70	77.03	10.06
Observations:	12501			62005		

Notes: This table reports summary statistics for local unions without agency fee payers in the baseline LM-2 sample. Observations reported are union-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2007-2019. Table reports union financial and membership data at the union level.
Sources: DOL, FCMS, and author's calculations.

ment opportunities, profitability, leverage, size, and tangibility although these differences are not statistically significant. The median non-unionized firm in the matched panel reports book leverage of 0.35, while the median unionized firm reports a leverage of 0.41. Unionized firms also tend to be larger. The average unionized firm reports annual log sales of 8.43. The average nonunionized firm reports annual log sales of 5.09. Unionized firms also tend to have a lower Tobin's q, defined here as a firm's market to book ratio. The average unionized firm reports a Tobin's q of 1.48, while the average nonunionized firm reports a Tobin's q of 4.22. This difference is consistent with patterns of unionization across firms. As newer firms are less likely to be organized than older larger firms, one should expect that unionized firms report a lower Tobin's q. Unionized firms also hold a greater share of tangible capital. The median non-unionized firm in the matched panel reports a ratio of tangible capital to total assets of 0.405, while the median unionized firm reports a ratio of 0.139. As unionization varies across industries, these differences reflect variation in investment opportunities, profitability, input market, and output demand, which vary across industries, in addition to industrial relations. As such, I use these variables, in addition to fixed effects, to identify the cross-sectional relationship between unions' balance sheets and firms' capital structure. Notably, I find that unionized firms with agency fee payers are remarkably similar to firms without agency fee payers. In the next section, I discuss how these findings provide support

Table 1.4 – Unionized and Nonunionized Firms in Matched Panel

	Unionized			Nonunionized		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Leverage	0.35	0.18	0.32	0.41	1.20	0.20
Tobin's Q	1.48	0.70	1.28	4.22	15.46	1.59
Tangibility	0.43	0.24	0.41	0.24	0.25	0.14
Log Sales	8.43	1.38	8.34	5.09	2.73	5.41
Profitability	0.12	0.09	0.11	-0.22	1.44	0.08
Observations:	1799			75987		

Notes: This table reports summary statistics for unionized and nonunionized firms in the matched LM-2 and Compustat sample. Observations reported are firm-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. *Sources:* DOL, FCMS, Compustat and author's calculations.

for the identifying assumptions of my research design.

1.3 Effect of RTW on Unions

In this section, I estimate the effect of RTW on local union's balance sheets. First, I explain how institutional details of union governance inform my research design. Second, I explain how these features of labor law and union governance shape unions' differential exposure to RTW policies. Third, I present the effect of RTW of union balance sheets and discuss how local unions and their affiliates responded to RTW.

1.3.1 Institutional Arrangements

While state legislatures enacted the RTW policies studied here, a union's past collective bargaining outcomes determine how RTW affects that union's membership and balance sheet. RTW prohibits agency shop agreements between unions and employers. These contract clauses stipulate that all employees covered by the contract must contribute a portion of their wages to support the union's bargaining and representative services. Employees who contribute to the union that represents them under such agreement are known as agency fee payers. While the National Labor Relations Act (NLRA) established the legality of agency shop agreements in 1935, it did not mandate the inclusion of agency shop agreements in all collective bargaining agreements. Unions must bargain with their employer for the ability to enforce mandatory collection of agency fees, as well as the size of those fees. Employers do not need to grant unions the ability to collect agency fees. Assuming that denying agency

Table 1.5 – Unionized Firms with and without Agency Fee Payers in Matched Panel

	Reported Agency Fee Payers			No Agency Fee Payers		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Leverage	0.36	0.20	0.32	0.34	0.17	0.32
Tobin's Q	1.38	0.45	1.25	1.51	0.76	1.29
Tangibility	0.33	0.22	0.27	0.46	0.24	0.44
Log Sales	8.54	1.48	8.43	8.40	1.35	8.33
Profitability	0.12	0.06	0.11	0.12	0.10	0.11
Observations:	387			1412		

Notes: This table reports summary statistics for unionized firms with and without agency fee payers in the matched LM-2 and Compustat sample. Observations reported are firm-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. *Sources:* DOL, FCMS, Compustat and author's calculations.

fees weakens workers' bargaining position, employers often seek to exclude these clauses from collective bargaining agreements to limit workers' bargaining power. This practice is widespread in states without RTW policies. In 2017, 80% of construction projects in New York City were open-shop (see La Guerre (2017)).

Subsequently, this variation in agency shop agreements across unions shapes unions' exposure to RTW polices. Some unions do not receive income from agency fee payers, while others rely on agency fee payers to fund their operations. Following the passage of a RTW law, nonmembers are no longer required to pay agency fees to the union that represents them, once the contract containing the agency shop agreement expires. Under RTW, unions with agency shop agreements lose income from agency fee payers and face the organizing challenge of convincing members to pay a higher relative cost of membership. As free riding becomes more attractive, these unions may also lose income from members leaving the organization. While the passage of RTW laws sometimes occur alongside other rollbacks in labor regulations and redistributive policies that disadvantage workers, RTW only affects the income of unions with agency fee payers. Unions without agency fee payers are not immediately affected by the passage of RTW, as RTW does not initially affect these unions' incomes or their relative cost of membership. As such, unions with agency shop agreements are more exposed to RTW policies than unions without such agreements.

The presence of agency fee payers is the binary exposure measure in this research design. The use of this measure relies on the assumption that unions that report agency fee payers are more likely to have an agency shop agreement than unions that do not report agency fee payers. Local unions do not report whether they have an agency shop agreement in their contract to the DOL. Rather they report agency fee payers in the LM-2 filings. Some collective bargaining contracts in RTW and non-RTW states allow for voluntary agency fee payers. This allows unions to collect fees from workers who are not willing to become a member. For example, Wayne State University AAUP-AFT Local 6075 offers employees in their bargaining unit the option of paying voluntary agency fees. Organizers affiliated with AFT Michigan claim that organizing new employees to chose to become voluntary agency fee payers is easier than organizing them into full union members. As such, there are unions in RTW states that report agency fee payers. Interestingly, agency fee payers does not decrease to zero after RTW.

Voluntary agency fee payers are rare in RTW states. Table 1.6 reports the share of local unions with agency fee payers in states that passed RTW prior to the start of the sample period and states that have never passed RTW legislation. In states without RTW, approximately 20% of unions report agency fee payers. In states where RTW laws passed prior to 2007, 5.5% of unions report agency fee payers. The latter statistic represents either unions with voluntary agency fee payers or ones with misreported data. The latter is unlikely as unions are not required to report agency fee payers and may simply leave the entry blank. In states without RTW, this ratio is above 12 agency fee payers per 100 members for most of the sample. In states with RTW, this ratio is less than 10 agency fee payers per 100 members for most periods. In states without RTW, the ratio of agency fee payers varied insignificantly throughout the sample period. In states with RTW before 2007, the ratio of agency fee payers to members increased slightly throughout this period. As such, these data suggest that voluntary agency fee payment is rare in RTW states.

Following RTW, the share of unions reporting agency fee payers and the ratio of agency fee payers to members decreases. As unions allow for voluntary agency fee payers and agency shop clauses persist until a collective bargaining agreement ends, RTW does not completely eliminate agency fee payers in the short run. Instead, RTW removes mandatory agency fee payers, causing a significant decrease in the share of unions reporting agency fee payers and the ratio of agency fee payers to members. Table 1.7 reports the share of local unions with agency fee payers and the ratio of agency fee payers to members for unions with agency fee payers in the states that enacted RTW within the analysis sample. Across all treatment groups, the share of unions reporting agency fee payers fell from 14% in the year RTW passed to 8% five years after the reform. Over the five years following the passage of RTW, this ratio decreases almost 50% from approximately 8 agency fee payers per 1000 members to 6 agency fee payers per 1000 members.

The dynamics of agency fee payment are heterogenous across states. A year after RTW passed in West Virginia, no unions reported any agency fee payers. Two years after RTW passed in Kentucky, unions reported 1 agency fee payer to 10,000 members. Michigan and Wisconsin are the two states in the sample with the largest share of unions with agency fee payers prior to RTW. As such, the share of unions reporting agency fee payers remains higher than that of the other states in the treatment group, but the ratio of agency payers to members fell significantly. Following the passage of RTW in Wisconsin, the share of unions reported agency fee payers decreased from 24% to 17%, while the average ratio of agency fee payers to members decreased from 0.017 to 0.001 after four years. In Michigan, the share of unions reported agency fee payers decreased from 24% to 17%, while the average ratio of agency fee payers to members decreased from 0.009 to 0.0007 after five years. While the effect of RTW led to gradual, heterogenous decreases in agency fee payers across states, the decreases were significant and persistent. Appendix A.2 reports additional information on membership trends across these cross-sections.

Table 1.6 – Trends in Agency Fee Payers Outside of Treatment Group

	Share of Unions with AFP			AFP/Members		
Year	Total	RTW Never	RTW Pre-2007	Total	RTW Never	RTW Pre-2007
2007	0.1617	0.1959	0.0566	0.0113	0.0131	0.0058
	(0.0063)	(0.0079)	(0.0080)	(0.0011)	(0.0013)	(0.0019)
2008	0.1521	0.1855	0.0543	0.0105	0.0114	0.0079
	(0.0062)	(0.0077)	(0.0077)	(0.0011)	(0.0012)	(0.0023)
2009	0.1548	0.1903	0.0501	0.0106	0.0122	0.0058
	(0.0063)	(0.0079)	(0.0075)	(0.0011)	(0.0013)	(0.0019)
2010	0.1629	0.1998	0.0466	0.0128	0.0140	0.0090
	(0.0069)	(0.0086)	(0.0080)	(0.0013)	(0.0015)	(0.0028)
2011	0.1760	0.2172	0.0532	0.0143	0.0152	0.0116
	(0.0067)	(0.0084)	(0.0079)	(0.0014)	(0.0015)	(0.0030)
2012	0.1769	0.2197	0.0486	0.0140	0.0155	0.0096
	(0.0067)	(0.0084)	(0.0076)	(0.0013)	(0.0015)	(0.0028)
2013	0.1829	0.2270	0.0531	0.0147	0.0162	0.0101
	(0.0068)	(0.0086)	(0.0079)	(0.0014)	(0.0016)	(0.0028)
2014	0.1857	0.2304	0.0524	0.0144	0.0164	0.0084
	(0.0069)	(0.0086)	(0.0079)	(0.0013)	(0.0016)	(0.0025)
2015	0.1856	0.2315	0.0513	0.0141	0.0167	0.0064
	(0.0069)	(0.0086)	(0.0077)	(0.0013)	(0.0016)	(0.0019)
2016	0.1841	0.2272	0.0583	0.0145	0.0163	0.0094
	(0.0068)	(0.0085)	(0.0082)	(0.0014)	(0.0016)	(0.0025)
2017	0.1857	0.2303	0.0569	0.0139	0.0152	0.0101
	(0.0069)	(0.0086)	(0.0081)	(0.0013)	(0.0015)	(0.0027)
2018	0.1719	0.2129	0.0579	0.0098	0.0102	0.0084
	(0.0067)	(0.0084)	(0.0080)	(0.0011)	(0.0013)	(0.0022)
2019	0.1627	0.1971	0.0646	0.0105	0.0096	0.0132
	(0.0065)	(0.0081)	(0.0085)	(0.0013)	(0.0013)	(0.0031)

Notes: This table reports means and their standard errors in agency fee payers in local unions located in states which were unaffected by RTW reforms from 2007-2019.
Sources: DOL

Table 1.7 – Trends in Agency Fee Payers Relative to the Enactment of RTW

	Share of Unions with AFP						AFP/Members					
h	Total	MI	IN	WI	WV	KY	Total	MI	IN	WI	WV	KY
-5	0.1130	0.1230	0.0714	0.1791	0.0833	0.1132	0.0047	0.0015	0.0079	0.0017	0.0064	0.0108
	(0.0146)	(0.0241)	(0.0230)	(0.0472)	(0.0467)	(0.0439)	(0.0019)	(0.0008)	(0.0063)	(0.0009)	(0.0062)	(0.0066)
-4	0.1173	0.1016	0.0840	0.2192	0.1053	0.1154	0.0045	0.0013	0.0085	0.0047	0.0053	0.0062
	(0.0149)	(0.0222)	(0.0255)	(0.0488)	(0.0505)	(0.0447)	(0.0018)	(0.0007)	(0.0066)	(0.0021)	(0.0052)	(0.0041)
-3	0.1093	0.1091	0.0877	0.1892	0.0606	0.0755	0.0058	0.0018	0.0084	0.0026	0.0031	0.0184
	(0.0149)	(0.0243)	(0.0266)	(0.0458)	(0.0422)	(0.0366)	(0.0025)	(0.0008)	(0.0068)	(0.0011)	(0.0030)	(0.0147)
-2	0.1185	0.1341	0.0816	0.2000	0.0541	0.0600	0.0036	0.0019	0.0094	0.0031	0.0010	0.0014
	(0.0154)	(0.0255)	(0.0278)	(0.0465)	(0.0377)	(0.0339)	(0.0018)	(0.0007)	(0.0079)	(0.0013)	(0.0010)	(0.0011)
-1	0.1326	0.1412	0.0901	0.2464	0.0526	0.1000	0.0046	0.0029	0.0104	0.0021	0.0007	0.0040
	(0.0161)	(0.0263)	(0.0273)	(0.0523)	(0.0367)	(0.0429)	(0.0019)	(0.0014)	(0.0073)	(0.0008)	(0.0006)	(0.0031)
0	0.1490	0.1686	0.0973	0.2394	0.0513	0.1458	0.0084	0.0079	0.0102	0.0146	0.0007	0.0027
	(0.0169)	(0.0286)	(0.0280)	(0.0510)	(0.0358)	(0.0515)	(0.0032)	(0.0048)	(0.0072)	(0.0110)	(0.0006)	(0.0018)
1	0.1403	0.1744	0.0714	0.2329	0.0000	0.1458	0.0055	0.0016	0.0101	0.0139	0.0000	0.0004
	(0.0165)	(0.0290)	(0.0244)	(0.0498)	(0.0000)	(0.0515)	(0.0026)	(0.0007)	(0.0073)	(0.0107)	(0.0000)	(0.0002)
2	0.1024	0.1124	0.0811	0.2059	0.0000	0.0556	0.0045	0.0011	0.0150	0.0024	0.0000	0.0001
	(0.0143)	(0.0237)	(0.0260)	(0.0494)	(0.0000)	(0.0315)	(0.0024)	(0.0005)	(0.0098)	(0.0012)	(0.0000)	(0.0001)
3	0.1145	0.1322	0.0541	0.2192	0.0000		0.0051	0.0057	0.0078	0.0020	0.0000	
	(0.0161)	(0.0258)	(0.0216)	(0.0488)	(0.0000)		(0.0028)	(0.0045)	(0.0070)	(0.0009)	(0.0000)	
4	0.1114	0.1294	0.0439	0.1733			0.0052	0.0056	0.0072	0.0012		
	(0.0166)	(0.0258)	(0.0193)	(0.0440)			(0.0030)	(0.0046)	(0.0068)	(0.0006)		
5	0.0836	0.1034	0.0531				0.0061	0.0007	0.0144			
	(0.0164)	(0.0232)	(0.0212)				(0.0038)	(0.0004)	(0.0096)			

Notes: This table reports means and their standard errors in agency fee payers in states affected by RTW reforms from 2007-2019. *Sources:* DOL

1.3.2 Identification

This research design exploits this differential exposure to measure the effect of RTW on unions. In this setting, treatment occurs when a state's legislature passes RTW. I measure a union's exposure to RTW as a binary indicator that takes the value of one if they report agency fee payers prior to the passage of RTW and zero otherwise. Differential exposure designs rely on two identifying assumptions. First, differential exposure designs rely on the assumption that the gap between exposed and unexposed unions in the control group would have evolved similarly to the gap between exposed and unexposed unions in the treatment group, if the unions in the treatment group did not experience RTW. While this assumption is ultimately untestable, as the treatment group's counterfactual is unobserved by the econometrician, this assumption is ex-ante plausible in this setting. Unions rely on their employer's economic performance, regardless of whether they operate under a union security agreement. Mandatory agency fee payments cannot offset variation in employment and wages resulting from state-level trends.

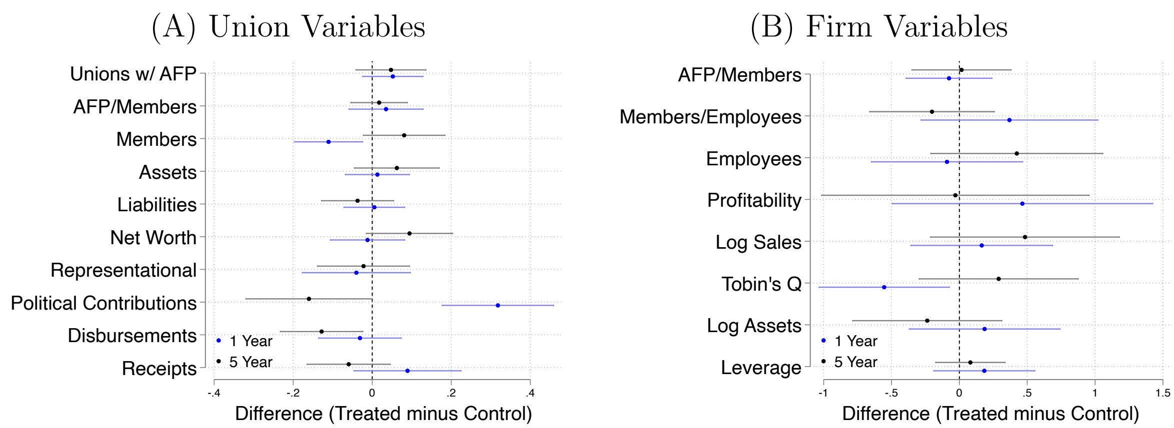
Second, differential exposure difference-in-differences research designs rely on the assumption that unions' adoption of agency shop agreements would be the same across states in the absence of RTW. This assumption is reasonable in this setting. Unions do not change agency shop agreements in anticipation of RTW. Unions cannot adjust their agency shop clause at will, so terms of agency fee payment cannot be adjusted outside of collective bargaining. It is also unlikely that unions would adjust their reliance on agency fee payers in anticipation of RTW. Raising dues on members is generally unpopular and requires union organizers to build membership support for raising the cost of membership. As organizing resources are limited, it is unlikely that unions would allocate effort towards building membership support for a dues increase, rather than allocating effort towards mobilizing membership in opposition of RTW.

In the rare case that a union is actively bargaining for a new contract when a state's legislature signs RTW into law, it is unlikely that a union without an agency shop agreement would use their bargaining power to establish a short-lived agency shop agreement prior to the RTW policy taking effect. That said, a union with a preexisting agency shop agreement may take steps to adjust the length of their contract in order to delay the loss of their agency shop agreement. In 2012, the Graduate Employees Organization AFT-MI 3550, AFL-CIO (GEO), a union with a preexisting agency shop agreement, bargained to extend the length of their contract from three to four years. This preemptive decision was possible because the union was actively bargaining a new contract when the Michigan Legislature passed RTW during its 2012 lame duck session. This bargaining decision was costly for GEO's members. To extend the length of their contract, members of GEO accepted historically lower raises.

This example shows that although unions with preexisting agency shop agreements can delay the effect of RTW policies, they cannot change their exposure to RTW even if they are in active contract negotiations.

Figure 1.1 reports differences in union and firm trends across RTW and non-RTW states in the estimation sample. Trends in the share of unions with reported agency fee payers do not vary systematically between RTW and non-RTW in the pre-treatment period. Union net worth, disbursements, and receipts exhibit a parallel trend across states in the sample period. There are limited departures from parallel trends for members, assets and liabilities, although those departures are less than 5% of the sample standard deviation of either variable.

Figure 1.1 – Difference in Pre-Treatment Trends Across RTW and Never-RTW Unions



Notes: This plot contains two subplots, which report the difference in pre-treatment trends across treated and untreated unions. Differences are scaled by the standard deviation of the entire sample. These data are from the LM-2 annual reports filed with the DOL. Sample spans from 2007 to 2019.
Sources: DOL, Compustat, FCMS, and author's calculations.

The observable characteristics of unions and firms in this data set also support this assumption. Figure 1.2 shows that unions, regardless of exposure to RTW, have similar pre-RTW, within-state trends. Figure 1.2 shows that unions with agency fee payers exhibit parallel trends across RTW and non-RTW states. Moreover, Figure 1.3 shows that the take-up of agency shop clauses is not systematically correlated with firms' capital structure or unions' membership or financial conditions. As such, exposed and unexposed unions appear to exhibit parallel trends, which supports the validity of this research design choice. Some unions with agency fee payers may not operate under an agency shop clause, so estimates using this research design may exhibit attenuation bias, as the exposure indicator variable may overstate the level of exposure of some local unions.

In this differential exposure design, a firm's exposure to RTW policies depends on their employees' local union's balance sheet. Local unions typically represent workers employed at

Figure 1.2 – Difference in Pre-Treatment Trends Across Agency-Shop and Open-Shop Unions

(A) Union Variables (B) Firm Variables

Notes: This plot contains two subplots, which report the difference in pre-treatment trends across exposed and unexposed unions. Differences are scaled by the standard deviation of the entire sample. These data are from the LM-2 annual reports filed with the DOL. Sample spans from 2007 to 2019.
Sources: DOL, Compustat, FCMS, and author's calculations.

Figure 1.3 – Difference in Pre-Treatment Levels Across Agency-Shop and Open-Shop Unions

(A) Union Variables (B) Firm Variables

Notes: This plot contains two subplots, which report the difference in pre-treatment levels across exposed and unexposed unions. Differences are scaled by the standard deviation of the entire sample. These data are from the LM-2 annual reports filed with the DOL. Sample spans from 2007 to 2019.
Sources: DOL, Compustat, FCMS, and author's calculations.

one firm, due to the cost of coordinating organizing across different companies or institutions. Some local unions operate across a number of similar, smaller firms, due to the fixed overhead cost of providing union services. While a few larger local unions operate across state lines due to the idiosyncratic governance structure of national organizations, inter-state coordination is typically coordinated through intermediate and national bodies. As such, I limit my sample to local unions who do not file F7 notices in two states.

1.3.3 Effect of RTW on Unions

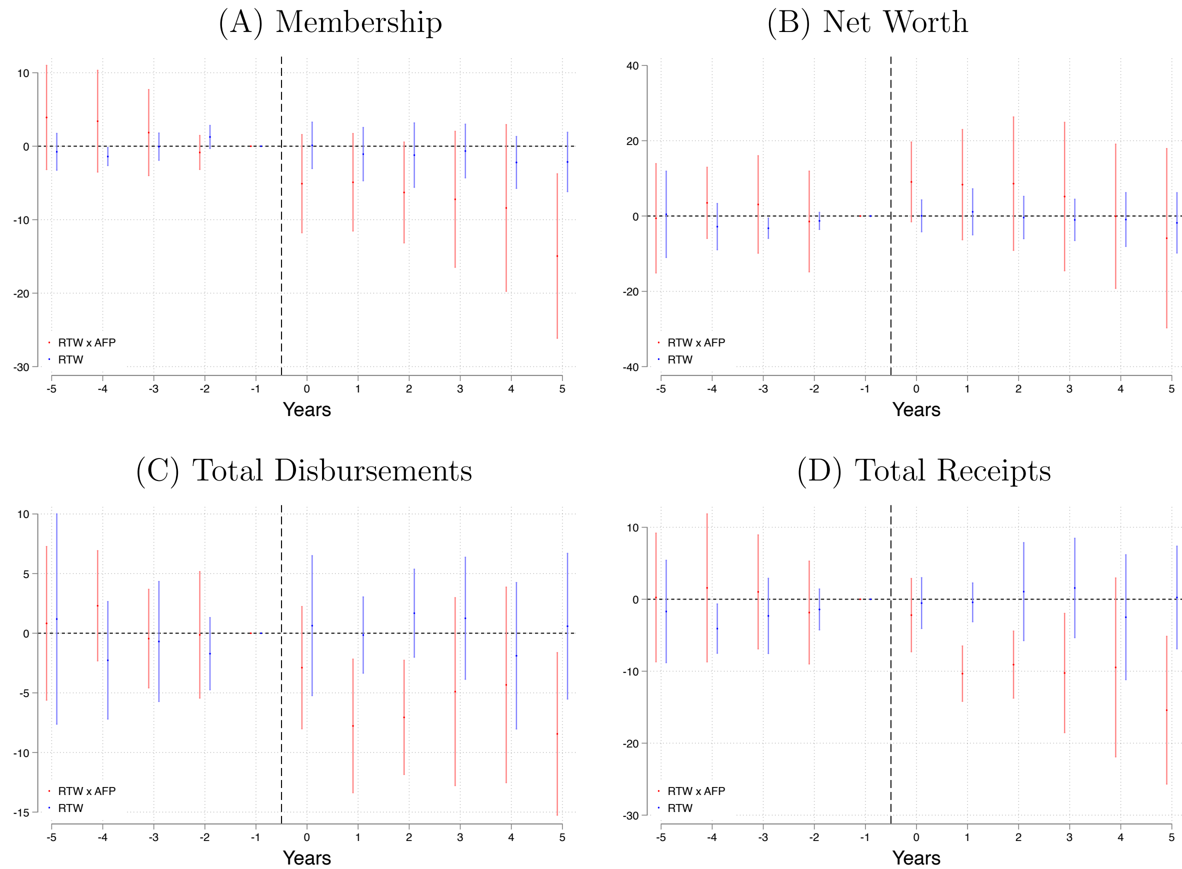
These institutional details inform my research design. I use a local projection difference-in-differences estimator with clean controls, following Dube et al. (2022) to estimate the RTW's effect on unions with agency fee payers, relative to unions without agency fee payers. This approach avoids improper comparisons between the just-treated group and previously treated groups, which can bias fixed-effects estimators in settings with heterogenous treatment effects and staggered treatment. For each dependent variable y_{it} at horizon h, I run the following baseline specification:

$$\Delta^h y_{i,t} = \alpha_t + \alpha_s + \beta^h_{RTW} RTW_{i,t} + \beta^h_{AFP}\ AFP_{i,t} + \beta^h_{RTW \times AFP} RTW_{i,t} \times AFP_{i,t} + \varepsilon_{i,t}, \quad (1.3.1)$$

where $RTW_{i,t}$ is a binary treatment indicator and equals 1 when RTW is enacted; $AFP_{i,t}$ is the exposure indicator and equals 1 if union i claims agency fee payers in the period t. The specification includes year and state fixed effects and cluster coefficients' standard errors at the state-year level. Clean controls refers to the process of restricting the sample to observations that either receive treatment in period t or do not receive treatment within a specified window. For clarity, I restrict the sample to unions that experience RTW in period t or have never experienced RTW. The main coefficient of interest is $\hat{\beta}^h_{RTW \times AFP}$, which measures the mean change in the outcome variable for unions that entered RTW with agency fee payers, relative to unions which entered RTW without any agency fee payers. The coefficient β^h_{RTW} measures the mean change in the outcome variables for unions that experienced RTW, relative to unions that never experienced RTW legislation. The coefficient β^h_{AFP} measure the mean change in the outcome variable for unions that with agency shop agreements, relative to unions that do not have these agreements.

Figures 1.4 and Figure 1.5 plot $\hat{\beta}^h_{RTW \times AFP}$ and $\hat{\beta}^h_{RTW}$ for the baseline specification. Panel A plots the percentage point change in union membership for exposed unions following the introduction of RTW. Membership in exposed unions fell by 15% over five years, relative to membership in unexposed unions. This decline in membership is consistent with previous estimates of Wexler (2022), who found that union coverage fell by nearly 14% percentage points following the introduction of RTW in the same sample state reforms used here, using CPS MORG data. The research design employed here provides additional texture to previous estimates of the effect of RTW on union membership. Specifically, Panel A shows that unexposed unions did not experience a statistically significant decrease in membership following RTW, relative to unexposed unions in the control group.

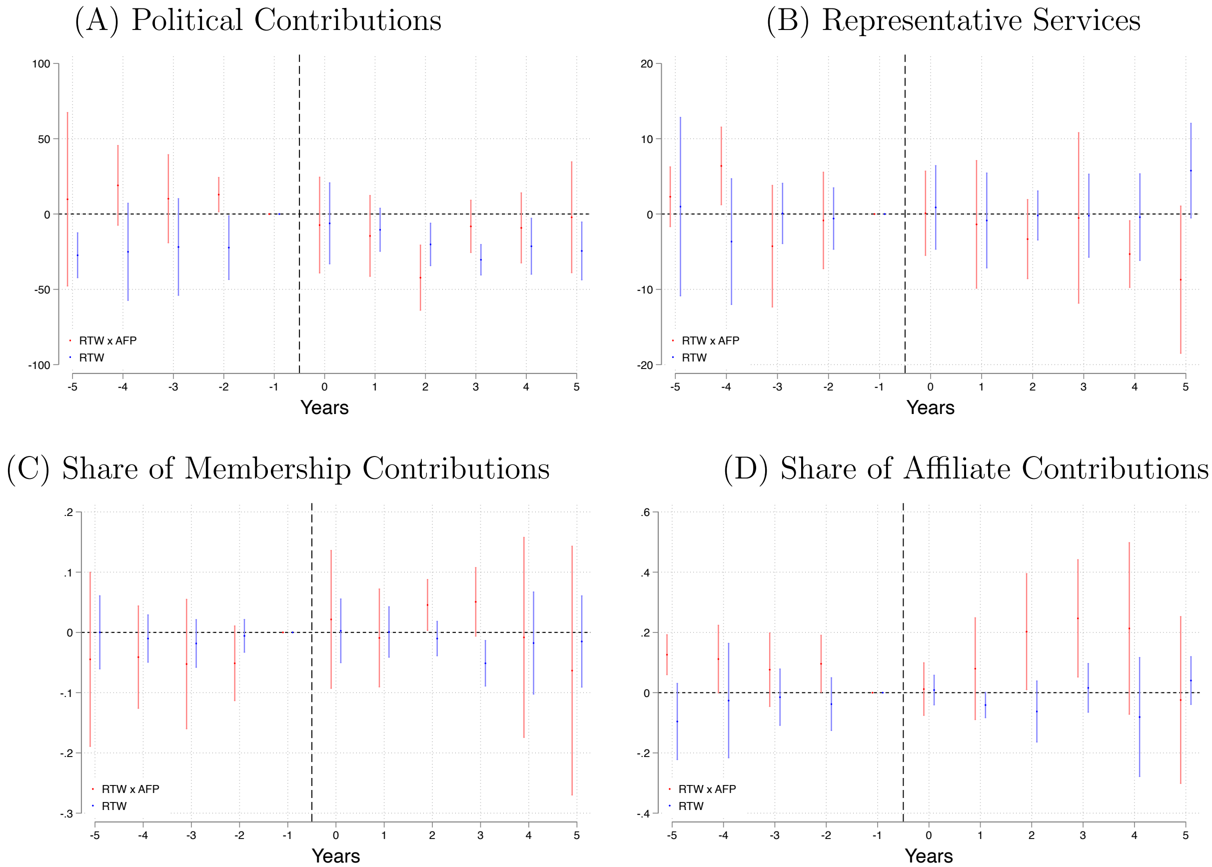
Figure 1.4 – Union Responses to RTW Reforms



Notes: This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from Dube et al. (2022). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots $\beta^h_{AFP\times RTW}$ for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. The blue line plots β^h_D for different horizons. This coefficient measures the effect of RTW on unions relative to unions that have never experienced RTW in the sample. Panel A plots the response of unions' membership. Panel B plots the response of unions' net worth. Panel C plots the change in the change of unions' disbursements. Panel D plots the change in unions' revenue. 95% confidence intervals reported. Sample spans from 2007 to 2019.
Sources: DOL, FCMS, and author's calculations.

The effect of RTW on union balance sheets also varies by unions' exposure. Panel C plots the log difference in total receipts or exposed unions relative to unexposed unions. Panel D plots the effect on total disbursements. In the year after RTW took effect, exposed unions' total disbursements fell by 7% and total receipts fell by 10% after 5 years. Unions without agency fee payers did not experience a reduction in total receipts and total distributions following RTW. This spending decrease is spread across various spending categories, including

Figure 1.5 – Union Responses to RTW reforms (Additional Effects)



Notes: This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from Dube et al. (2022). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots $\beta^h_{AFP\times D}$ for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. The blue line plots β^h_{RTW} for different horizons. This coefficient measures the effect of RTW on unions relative to unions that have never experienced RTW in the sample. Panel A plots the response of unions' spending on political causes. Panel B plots the response of unions' spending on representational services. Panel C plots the change in the change of the share of membership contributes of total union revenue. Panel D plots the change in the share of affiliate contributions of total union revenue. 95% confidence intervals reported. Sample spans from 2007 to 2019.
Sources: DOL, FCMS, and author's calculations.

political contributions and representative services.

Unions rely on members and affiliates to accommodate income shortfalls associated with RTW. Unions' income share of membership contributions to total receipts increases by 5% following RTW. Affiliate contributions as a share of union income increased by 20% in exposed unions, and decreased by 5% in unexposed unions. This asymmetric, heterogenous response to RTW suggests that affiliates insure local unions from aggregate shocks through transfers financed by other local unions in the same federation. Finger and Hartney (2021) document similar phenomenon. They find that unions transfer funds from states with high union security to states that experience RTW and other reductions in labor protection, which they call financial solidarity. These results show that financial solidarity between unions occurs within states as well. That said, additional contributions of members and affiliates does not offset the income loss associated with RTW.

While unions without agency fee payers do not experience a statistically or economically significant decrease in revenues after RTW, they exhibit a strategic shift in their spending allocation. Following RTW, the overall political donations of all unions experienced a persistent 10 percent decline over 5 years. This shift in unions' strategy, originally documented by Zullo (2020), suggests that unions' strategies depend on factors outside of their financial health. Rather, union strategy may depend more on the organizing incentives faced by unions subject to different labor regulations and political environments.

1.4 Analysis of Leverage and Unionization

In this section, I estimate the relationship between union income and firm leverage using repeated cross-sections of firms identified from 2000-2019. This section proceeds in three steps. First, I discuss the data limitations that motivate this approach and the assumptions present in this analysis. Second, I estimate the cross-sectional semi-elasticity of firm leverage with respect to unions' income. Third, I use these estimates and estimates of the effect of RTW on unions' income from the previous section to conduct a counterfactual analysis of the effect of RTW on firms' leverage.

1.4.1 Identification

I innovate on past strategies to estimate the effect of RTW on firms' leverage. Specifically, I use a fixed-effects panel specification to estimate the cross-sectional semi-elasticity of firms' leverage with respect to unions' income and membership. In the previous section, I show that RTW affects the revenue and membership of unions with pre-treatment agency fee payers. If a union's revenue and membership rate are first-order determinants of a union's bargaining power, the expected effect of RTW on firm's leverage is equal to the decomposition

$$\Delta^h \text{Lev}_{i,t} = \frac{\delta \text{Lev}}{\delta \text{REC}} \times \Delta^h \text{REC}_{i,t} + \frac{\delta \text{Lev}}{\delta \text{MEM}} \times \Delta^h \text{MEM}_{i,t} \tag{1.4.1}$$

where $REC_{i,t}$ is the log annual total receipts of unions associated with firm i; $MEM_{i,t}$ is the membership rate of union members to employees of firm i; $\Delta^h \text{MEM}_{i,t}$ is the expected effect of RTW on an exposed union's membership at horizon h; and, $\Delta^h \text{REC}_{i,t}$ is the expected effect of RTW on an exposed union's revenue at horizon h. In this section, I estimate the semi-elasticities $\frac{\delta \text{Lev}}{\delta \text{REC}}$ and $\frac{\delta \text{Lev}}{\delta \text{MEM}}$ using a panel regression. In the next section, I will combine these cross-sectional estimates with the LP-DD estimates from the previous section to construct a counterfactual estimate of the effect of RTW on firms' leverage.

The underlying assumptions of the panel specification are consistent with the assumptions required for the subsequent counterfactual analysis. The effect of RTW on unions' income measured in the previous section is for local unions that reported agency fee payers prior to the enactment of RTW. As such, the counterfactual analysis is consistent under the following set of assumptions. First, the firm's operations are limited to one state that unexpectedly enacts RTW. Second, the firm does not change their allocation of their production activities in the short-run. Third, the firm allowed for the mandatory collection of agency fees prior to the passage of RTW. While these assumptions provide a stylized counterfactual estimate, they provide a useful estimate of the effect of RTW on firms' leverage.

This approach innovates on prior corporate finance research on the affect of workers' bargaining power on firms' financing decision. Prior work measure the effect of workers' bargaining power on firms' leverage using state-level policy variation in the state where a firms' executive offices are located (see Chava et al. (2020) Matsa (2010)). This prior approach does not capture the firms' exposure to fluctuations in unions' bargaining power across states. Firms' business activity is seldom limited to the confines of the state in which their executive offices are located. Moreover, firms' may reallocate activity across establishments over time in response to changes in state-level changes in labor regulations. As such, limitations of the Compustat database preclude the accurate sorting of firms into treatment and control groups.

This panel specification assumes that firms' allocation of business activity is fixed in the short run, across states and bargaining units. Firms' production, financing options, and exposure to labor regulations vary across states, years, and industries. As such, I include fixed effects at the state, year, and 2-digit NAICS level. Information on firms' location is limited to the location of firms' executive offices. As prior studies show that firms' size, investment opportunities, capital tangibility, and profitability determine capital structure, I include these variables as controls in the panel regression.

Firms may adjust the location of production across states or within states based on collective bargaining outcomes. This behavior can take the form of simply relocating production to another establishment operated by the firm or sub-contracting production to another firm. If this behavior occurs, the fixed effects of the regression would be misspecified, as the union variables would not longer represent a firm's relative exposure to local unions. These behaviors are untestable in this data set. If they were to occur, we should expect that the estimates of the semi-elasticity of firms' leverage to unions' receipts would be biased downward, as firms' are incentivized to shift operations away from unionized workers when possible. This attenuation bias would result from union receipts affecting a smaller share of overall production.

1.4.2 Cross-sectional Estimates

For this exercise, I add measures of union income and membership into a typical leverage panel regression with financial controls. I estimate the following baseline specification:

$$Lev_{i,t} = \alpha_t + \alpha_n + \alpha_s + \beta_{REC}\ REC_{i,t} + \beta_{MEM}\ MEM_{i,t} + \ \beta_x \mathbf{X}_{i_t} \ + \ \varepsilon_{i,t}, \tag{1.4.2}$$

where $REC_{i,t}$ is the log annual total receipts of unions associated with firm i, and $MEM_{i,t}$ is the membership rate of union members to employees of firm i. I include state α_s, NAICS 2-digit industry α_n, and year α_t fixed effects. Standard errors are clustered at the state and year level. The vector of financial controls $\mathbf{X}_{i_t}$ includes log sales, asset tangibility, Tobin's q, and profitability.

Table 1.8 reports the relationship between firm leverage and union balance sheets and membership rates using a standard fixed effects panel estimator. The left hand side variable is leverage measured as the ratio of a firm's total debt to assets. There is a strong relationship between union revenue and firm leverage. A one percent increase in union receipts increases firm leverage by 110 basis points. Union revenue and membership determine the use of leverage; however, union revenue has a stronger relationship with firm leverage. Controlling for the membership rate of a firm, a one percent increase in log union receipts increases firm leverage by 90 basis points, while the effect of membership is economically small and statistically insignificant. Column 3 shows a one percent increase in the membership rate increases leverage by 13.4 basis points. This result likely follows from the contemporaneous correlation between dues and membership.

In addition to contemporaneous correlation between dues and membership, membership rates shape incentives faced by union organizers in the administering the collection of dues. Unions with higher membership rates may be able to organize members to contribute a greater share of their income to the union, as they face a lower risk of members leaving the organization. Unions with higher membership may also be able to ensure that their employer deducts dues from employees' paychecks, thereby increasing the rate of dues deductions

Table 1.8 – Leverage Regression Estimates: Baseline Specifications

	1	2	3	4	5
Log Receipts	0.01107**	0.00901*			
	(0.00472)	(0.00505)			
Membership Rate		0.00039	0.00134**	0.00096*	0.00086
		(0.00067)	(0.00060)	(0.00049)	(0.00060)
Receipts/Members				0.22483	
				(0.21635)	
Receipts					0.00006*
					(0.00004)
Log Sales	0.00843*	0.00846*	0.00874*	0.00861*	0.00860*
	(0.00465)	(0.00463)	(0.00461)	(0.00463)	(0.00462)
Tobin's Q	0.02907***	0.02907***	0.02907***	0.02907***	0.02907***
	(0.00132)	(0.00132)	(0.00132)	(0.00132)	(0.00132)
Tangibility	0.35428***	0.35424***	0.35465***	0.35419***	0.35491***
	(0.05070)	(0.05077)	(0.05085)	(0.05072)	(0.05078)
Profitability	-0.35144***	-0.35146***	-0.35165***	-0.35157***	-0.35154***
	(0.00613)	(0.00622)	(0.00615)	(0.00623)	(0.00612)
R-squared	0.422	0.422	0.422	0.422	0.422
Observations	65943	65943	65943	65943	65943

Notes: This table reports leverage panel regression results. Standard errors are clustered at the state and industry level. These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports firm financial data from Compustat. *Sources:* DOL, FCMS, Compustat, and author's calculations.

within the bargaining unit. If the employer refuses to deduct dues, unions must set up separate contributions systems at the union's expense, which require more staff and volunteer hours to manage and provides a less reliable stream of income. Union receipts may also correlate with members' wages. Wages are an outcome of union bargaining power, labor productivity, and firm financial health. Labor productivity affects union bargaining power and union income through different channels. First, more productive workers pay more dues to their unions on average, as most locals collect a percentage of workers' wages as dues. Workers with higher wages may also be able to provide more organizing hours to the union, as they are less likely to hold additional employment. Second, more productive workers may have greater bargaining power with the firm. More productive workers may be more capable of disrupting production with a labor dispute.

To address these considerations, I consider an alternative specification that regresses firm leverage on the membership rate and the ratio of total receipts to membership. A ratio of total receipts to membership of $10,000 is associated with a statistically insignificant 0.225

Table 1.9 – Leverage Regression Estimates: Additional Specifications

	6	7	8	9	10
Log Union Assets	0.01121**			0.00896*	
	(0.00441)			(0.00442)	
Membership Rate		0.00096		0.00043	
		(0.00059)		(0.00067)	
Net Worth		0.00006**	0.00010***		
		(0.00002)	(0.00003)		
Voting Membership					0.11964
					(0.07230)
Log Sales	0.00842*	0.00858*	0.00863*	0.00846*	0.00883*
	(0.00465)	(0.00462)	(0.00460)	(0.00463)	(0.00458)
Tobin's Q	0.02907***	0.02907***	0.02907***	0.02907***	0.02907***
	(0.00132)	(0.00132)	(0.00132)	(0.00132)	(0.00132)
Tangibility	0.35430***	0.35489***	0.35561***	0.35424***	0.35502***
	(0.05075)	(0.05087)	(0.05092)	(0.05079)	(0.05084)
Profitability	-0.35143***	-0.35153***	-0.35154***	-0.35146***	-0.35171***
	(0.00612)	(0.00611)	(0.00614)	(0.00616)	(0.00615)
R-squared	0.422	0.422	0.422	0.422	0.422
Observations	65943	65943	65943	65943	65943

Notes: This table reports leverage panel regression results. Standard errors are clustered at the state and industry level. These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports firm financial data from Compustat. *Sources:* DOL, FCMS, Compustat, and author's calculations.

increase in leverage. The effect of membership is significant, as a 1 percent increase in membership increases leverage by 9.6 basis points. These results suggest that a union's total income is a stronger determinant of firm leverage than a union's income per member. This relationship has a number of possible explanations. First, great membership and income allows unions to cover overhead, legal expenses, or wages distributed to a union's staff and officer wages. Second, this relationship could suggest that unions' receipts and membership rate proxy a unions' size, and these effects capture a unions' size effect on a firm's leverage. While these specifications are not able to evaluate these different explanations, they highlight the importance of union income for shaping firms' use of leverage as a strategic variable.

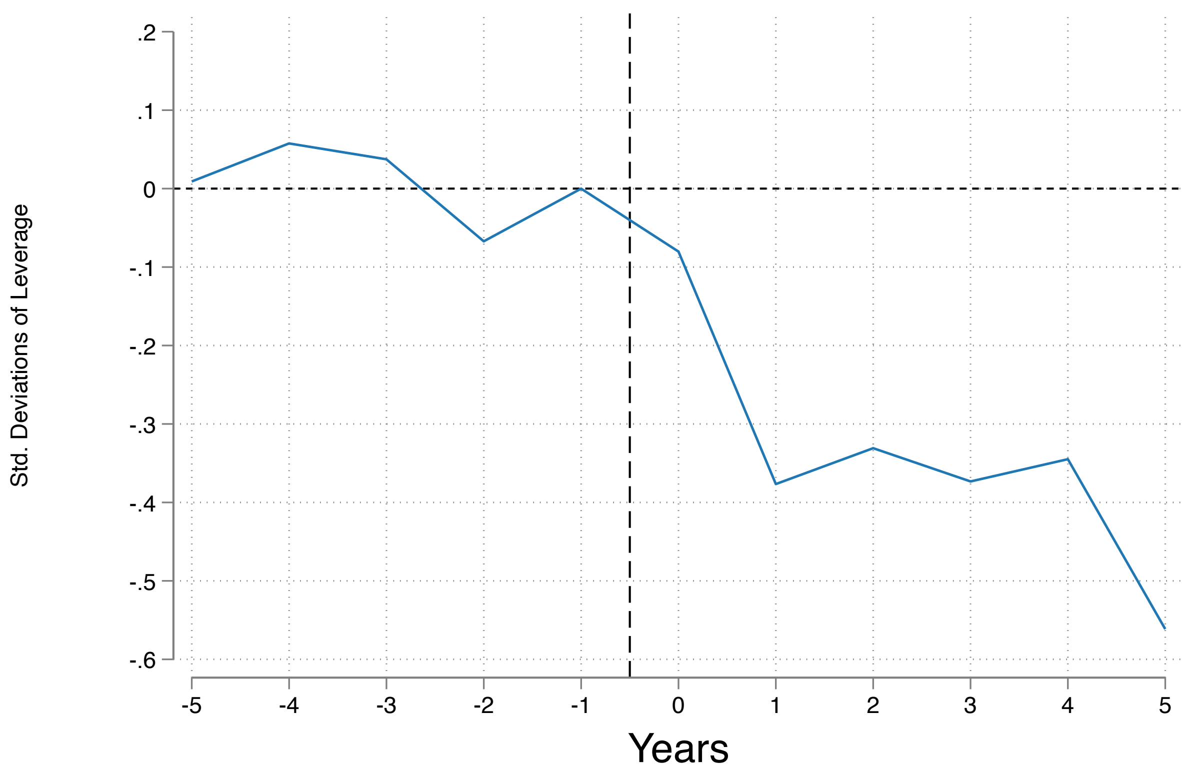
I also consider additional specifications that measure the relationship between firms' use of leverage and other measures of unions' membership and financial conditions. Table 1.9 reports the estimates of these specifications. I estimate the cross-sectional relationship between unions' log assets and firms' leverage. There is a strong relationship between unions' log assets and firm leverage. A one percent increase in union assets increases firm leverage by 112 basis points. Controlling for the membership rate of a firm, a one percent increase in log union receipts increases firm leverage by 89 basis points, while the effect of membership is economically small and statistically insignificant. This point estimates coincide with the point estimates on log receipts in the baseline specification. As unions' assets and receipts are positively correlated, these point estimates suggest that these point estimates indicate the relationship between local unions' size and firms' use of leverage. That said, a large union may face financial constraint. Unions' with greater net worth may exhibit greater ability to respond to members' organizational needs. I estimate the cross-sectional relationship between unions' net worth and firms' leverage. A $100,000 increase in unions' net worth increases firm leverage by 6 basis points. These results, taken together, suggests that union's size and net worth codetermine firms' leverage.

These estimates confirm and expand our understanding of the relationship of worker bargaining power and leverage. The relationship between membership rate and firm leverage is slightly larger than the coefficient estimated in Matsa (2010), which found that leverage is 10.2 basis points higher when an additional one percent of a firm is covered by a collective bargaining agreement. These two point estimates are not directly comparable. First, Matsa (2010) uses an earlier sample period of Compustat firms, during which the private sector unionization rate was higher. It follows that firms with unionized employees may now face a stronger incentive to raise leverage to maintain their competitiveness against other market participants without unionized employees. Second, Matsa (2010) regresses leverage on firms' coverage rate, defined as the share of a firm's workers covered by a collective bargaining agreement. not the membership rate. Membership rate, relative to the coverage rate, includes other features of union strength and capacity, not proxied by the coverage rate. That said, limitations of the available administrative data prevents further investigation of this relationship in this context.

1.4.3 Counterfactual Analysis

The relationship between union income and firm leverage highlights the importance of agency fees for union bargaining power within firms. A decrease in agency fee payers decreases the ratio of union receipts to members. Following RTW, union receipts decrease more than membership, decreasing the ratio of income to members. This decrease affects unions ability to perform outreach to members and to organize collective bargaining campaigns. Using the point estimates of the panel estimator and the causal estimates of the LP-DD estimator, we can construct a counterfactual effect of RTW on firm leverage. As the membership rate is not observed for each union, I restrict the counterfactual analysis to the revenue component of the total derivative in Equation 1.4.1. For the average firm in the sample, the decrease in exposed union income following RTW is associated with a 0.6 standard deviation decrease in firm leverage. This finding assumes that the average firms' activity is limited to a state affected by the reforms included in the sample. Figure 1.6 plots the counterfactual response. This result suggests that firms exposed to RTW face heterogenous outcomes of the policy. Firms without agency shop agreements do not experience an increase in bargaining power related to diminished union income.

Figure 1.6 – Counterfactual Effect of Union Receipts on Firm Leverage



Notes: This figure plots the counterfactual effect of RTW on firms' leverage if the firm's operations are limited to one state that experiences RTW and the firm's collective bargaining is limited to unions with agency fee payers. The point estimate plotted is the product of the revenue effect from the baseline LP-DD reported in Figure 1.4 and the point estimate of union receipts in the first leverage regression in Table 1.8.
Sources: DOL, FCMS, Compustat, and author's calculations.

This counterfactual estimate of the effect of RTW is consistent with findings by Chava et al. (2020) and Matsa (2010). Assuming firms' business activity is limited to the state in which their executive offices are located, Chava et al. (2020) finds a transitory effect of RTW on firm leverage, which bottoms out at -5 percentage points after four years. Using a two-way fixed effects estimator, Matsa (2010) finds that firms reduce leverage by 5 percentage points after the enactment of RTW.

1.5 Robustness

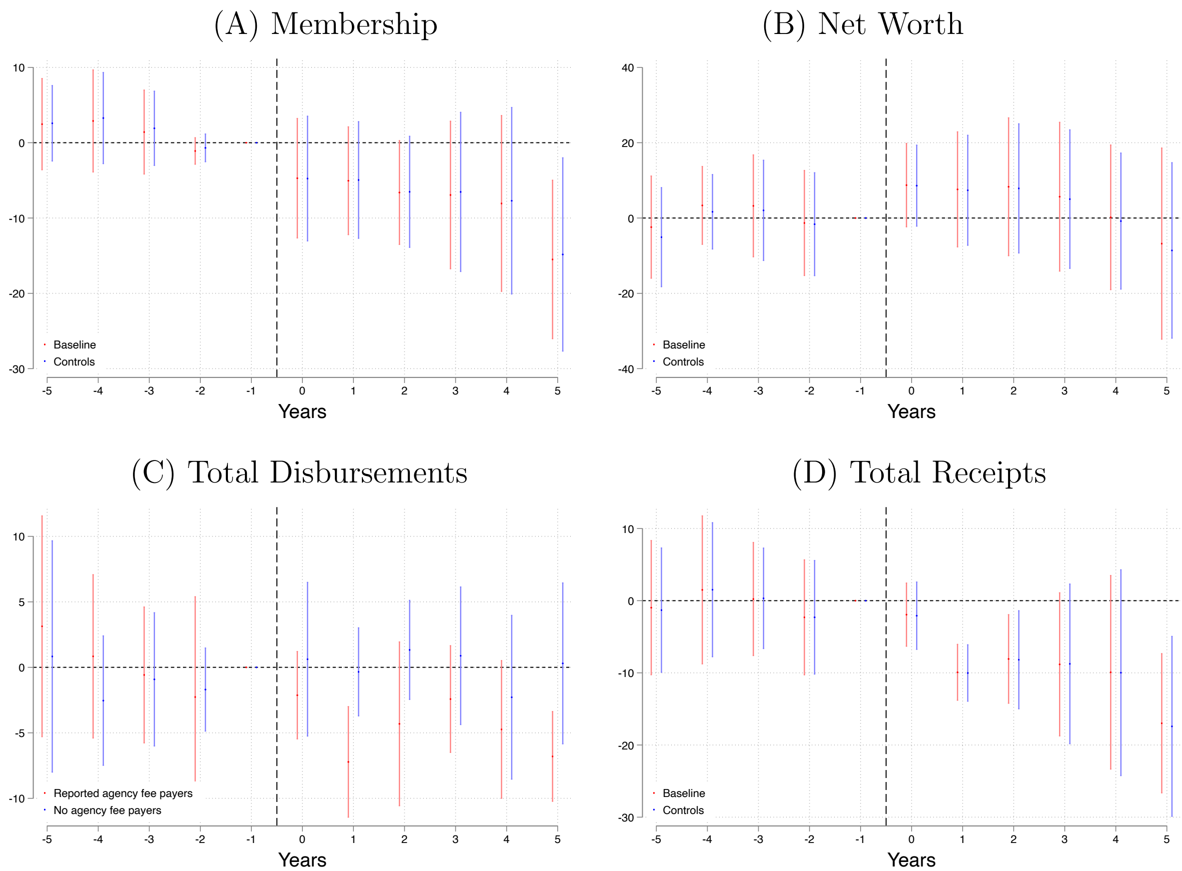
In this section, I evaluate the robustness of the LP-DD and panel estimates presented above. First, I evaluate whether the effect of RTW on unions' balance sheets is the result of differences in pre-reform membership, financial conditions, or labor federation. These characteristics shape unions' ability to organize and may affect their capacity to respond to changes in labor regulations. Second, I evaluate whether the effect of RTW on union membership is the result of unions misreporting agency fee payers as members by estimating the effect of RTW on two other measures on union membership reported in LM-2 filings. Third, I evaluate the effect of mismeasured firm characteristics in the leverage regressions using the Erickson et al. (2014) panel estimator, which provides a consistent point estimates when multiple regressors are mismeasured.

Figure 1.7 and Figure 1.8 report the effect of RTW on unions' balance sheets and membership controlling for pre-reform log membership, log assets, and labor federation. The inclusion of these controls do not significantly affect the baseline estimates. As such, there is no evidence that the effect of RTW on unions' balance sheets varies across unions of different sizes or federation affiliations. This result is not at odds with Zullo (2020), which found that skilled trade unions were less affected by RTW, as they served a second role of providing occupational licensing. Rather, this result implies that all unions, regardless of their constituencies, face similar financial and organizational consequences of losing agency fee payers.

The results of the LP-DD estimates are also robust to alternative measures of union membership. Union membership is measured in two ways in LM-2 filings. First, unions report their total share of members. Second, unions report their share of voting members. This second field is not required and is not always filled out by a union's designated filer. Additionally, one may be concerned that unions are reporting agency fee payers twice, first as members and again as agency fee payers. To address these sources of mis-measurement, Figure 1.9 presents estimates the baseline LP-DD specification of all three measures of active membership: total membership, voting membership, total membership less agency fee payers, alongside estimates of the effect of RTW on agency fee payers. The choice of membership measure has an insignificant effect on the statistical inference. In both alternative estimates, the response is consistent with the baseline estimate of total membership. The estimates of all measures of active membership do not conform to the effect of RTW on agency fee payers. which decreases upon impact.

As firms' asset tangibility and Tobin's q may be mismeasured, I also estimate the panel specifications using the Erickson et al. (2014) panel estimator. I present the estimates of

Figure 1.7 – Effect of RTW Laws: Additional Controls

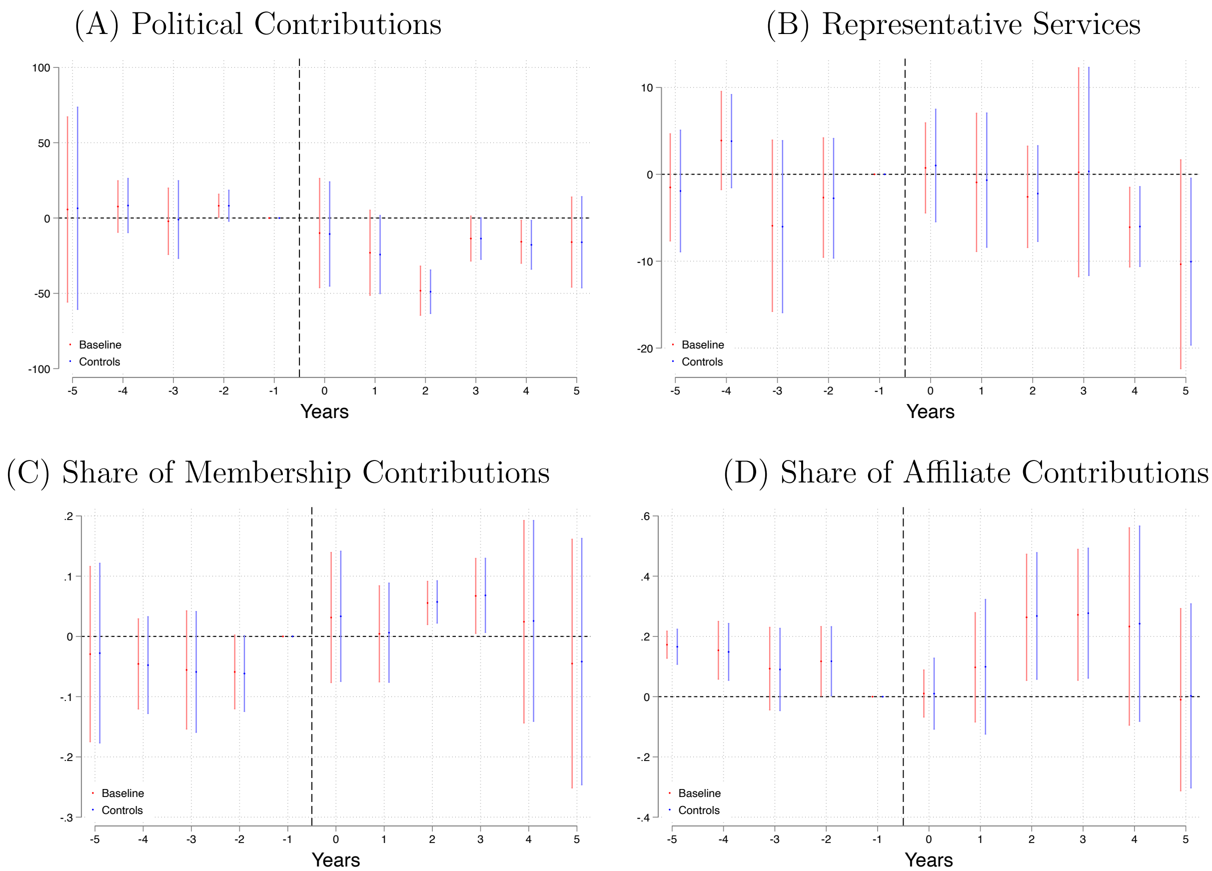


Notes: This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from Dube et al. (2022). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots $\beta^h_{AFP \times D}$ for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. Panel A plots the response of unions' membership. Panel B plots the response of unions' net worth. Panel C plots the change in the change of unions' disbursements. Panel D plots the change in unions' revenue. 95% confidence intervals reported. Sample spans from 2007 to 2019.
Sources: DOL, FCMS, and author's calculations.

different specifications of this estimator for the leverage regression of unions' membership and firms' financial controls for different measures of firms' financial health. Table 1.11 reports estimates for the leverage regressions on log union receipts; Table 1.10 reports estimates for the leverage regressions on total union receipts; And, Table 1.12 reports estimates for the leverage regressions on union net worth.

Correcting for measurement error decreases the effect of log receipts and membership on firm leverage. This is an expected outcome, as firm size and tangibility correlate with

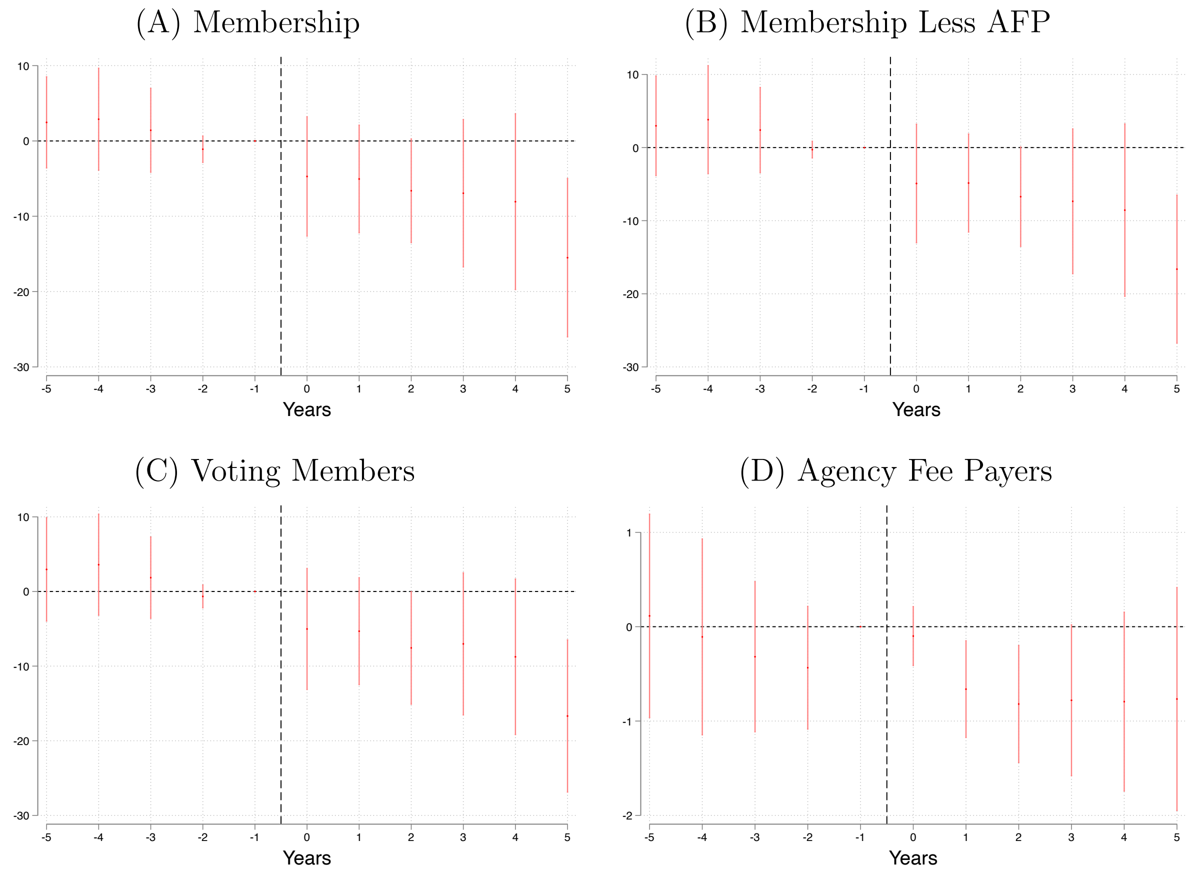
Figure 1.8 – Effect of RTW Laws: Additional Controls (Additional Effects)



Notes: This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from Dube et al. (2022). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots $\beta^h_{AFP\times D}$ for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. Panel A plots the response of unions' spending on political causes. Panel B plots the response of unions' spending on representational services. Panel C plots the change in the change of the share of membership contributes of total union revenue. Panel D plots the change in the share of affiliate contributions of total union revenue. Sample spans from 2007 to 2019.
Sources: DOL, FCMS, and author's calculations.

union's size and financial health. Historically, union density and membership has been higher in manufacturing and resource extraction industries. As such, the estimator should decrease the size of coefficients on union coefficients. While the point estimates on log receipts is insignificant for all specifications, the point estimates for total receipts are significant. That said, the effect of net worth, while smaller is significant for most specifications. In all specifications, the effect of the membership rate on firm leverage remains insignificant.

Figure 1.9 – Union Responses to RTW Reforms: Alternative Membership Measures



Notes: This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from Dube et al. (2022). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots $\beta^h_{AFP \times D}$ for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. Panel A plots the response of unions' membership. Panel B plots the plots the response of unions' membership less agency fee payers. Panel C plots the change in the change of unions' voting membership. Panel D plots the change in unions' agency fee payers. 95% confidence intervals reported. Sample spans from 2007 to 2019.
Sources: DOL, FCMS, and author's calculations.

Table 1.10 – Error-in-Variables Leverage Regressions with Union Receipts

	Measurement Error in q			Measurement Error in q & Tangibility		
	3rd Order	4th Order	5th Order	3rd Order	4th Order	5th Order
Receipts	0.0002*	0.0001*	0.0001*	0.0004	0.0002**	0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Member Rate	0.0011	0.0010	0.0010	-0.0051	-0.0033	-0.0024
	(0.0010)	(0.0007)	(0.0007)	(0.0062)	(0.0031)	(0.0017)
Tobin's Q	0.2006***	0.0887***	0.0979***	0.1634***	0.0880***	0.0968***
	(0.0536)	(0.0051)	(0.0039)	(0.0383)	(0.0045)	(0.0035)
Tangibility	0.3539***	0.3364***	0.3378***	5.2832	3.7723*	3.0229***
	(0.0743)	(0.0394)	(0.0416)	(4.4917)	(1.9747)	(0.7061)
Log Sales	0.0064	0.0081*	0.0080*	-0.0407	-0.0251	-0.0180**
	(0.0076)	(0.0044)	(0.0046)	(0.0449)	(0.0203)	(0.0089)
Profitability	0.8358**	0.0610	0.1252***	0.6156**	0.0830**	0.1382***
	(0.3827)	(0.0372)	(0.0391)	(0.2783)	(0.0415)	(0.0396)
Observations	59963	59963	59963	59963	59963	59963
ρ^2	0.7173	0.5224	0.5386	0.6949	0.5509	0.5598
J-Statistic	0.0000	20.5894	49.9437	3.7418	26.3393	71.7737
$\tau -$ q	0.5625	0.6538	0.6383	0.5800	0.6562	0.6409
$\text{SE}_\tau -$ q	0.0392	0.0383	0.0368	0.0400	0.0383	0.0368
$\tau -$ Tangibility	.	.	.	0.0823	0.1051	0.1273
$\text{SE}_\tau -$ Tangibility	.	.	.	0.0577	0.0502	0.0317

Notes: This table reports leverage panel regression results from the higher-order cumulant estimators of Erickson et al. (2014). The first three columns report regression results where firms' market to book ratio is assumed to be measured with error. The columns report the higher-order cumulant estimator used in the estimator. The fourth, fifth, and sixth columns report regression results where firms' market to book ratio and their tangibility is assumed to be measured with error. ρ^2 reports an estimate of the R^2 of the regression. τ_i reports the measurement quality for proxy variable i. J-statistic reports the model over-identifying statistic from Sargan (1958). These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports firm financial data from Compustat.
Sources: DOL, FCMS, Compustat, and author's calculations.

Table 1.11 – Error-in-Variables Leverage Regressions with Union Log Receipts

	Measurement Error in q			Measurement Error in q & Tangibility		
	3rd Order	4th Order	5th Order	3rd Order	4th Order	5th Order
Log Receipts	0.0071	0.0078	0.0077	-0.0166	-0.0089	-0.0053
	(0.0112)	(0.0066)	(0.0069)	(0.0310)	(0.0182)	(0.0136)
Member Rate	0.0016	0.0010	0.0010	-0.0008	-0.0005	-0.0002
	(0.0012)	(0.0008)	(0.0008)	(0.0036)	(0.0023)	(0.0018)
Tobin's Q	0.2006***	0.0887***	0.0979***	0.1633***	0.0880***	0.0968***
	(0.0536)	(0.0051)	(0.0039)	(0.0383)	(0.0045)	(0.0035)
Tangibility	0.3529***	0.3357***	0.3371***	5.2685	3.7667*	3.0106***
	(0.0743)	(0.0394)	(0.0416)	(4.5128)	(1.9756)	(0.7065)
Log Sales	0.0066	0.0081*	0.0080*	-0.0394	-0.0243	-0.0173*
	(0.0076)	(0.0045)	(0.0047)	(0.0441)	(0.0199)	(0.0088)
Profitability	0.8358**	0.0609	0.1252***	0.6144**	0.0822**	0.1376***
	(0.3827)	(0.0372)	(0.0391)	(0.2781)	(0.0414)	(0.0396)
Observations	59963	59963	59963	59963	59963	59963
ρ^2	0.7173	0.5224	0.5386	0.6947	0.5508	0.5596
J-Statistic	0.0000	20.5914	49.9538	3.7429	26.3412	71.9027
$\tau -$ q	0.5625	0.6538	0.6383	0.5800	0.6562	0.6409
$\text{SE}_\tau -$ q	0.0392	0.0383	0.0368	0.0399	0.0383	0.0368
$\tau -$ Tangibility	.	.	.	0.0823	0.1051	0.1275
$\text{SE}_\tau -$ Tangibility	.	.	.	0.0581	0.0503	0.0319

Notes: This table reports leverage panel regression results from the higher-order cumulant estimators of Erickson et al. (2014). The first three columns report regression results where firms' market to book ratio is assumed to be measured with error. The columns report the higher-order cumulant estimator used in the estimator. The fourth, fifth, and sixth columns report regression results where firms' market to book ratio and their tangibility is assumed to be measured with error. ρ^2 reports an estimate of the R^2 of the regression. τ_i reports the measurement quality for proxy variable i. J-statistic reports the model over-identifying statistic from Sargan (1958). These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports firm financial data from Compustat.
Sources: DOL, FCMS, Compustat, and author's calculations.

Table 1.12 – Error-in-Variables Leverage Regressions with Union Net Worth

	Measurement Error in q			Measurement Error in q & Tangibility		
	3rd Order	4th Order	5th Order	3rd Order	4th Order	5th Order
Member Rate	0.0013	0.0012*	0.0012*	-0.0035	-0.0021	-0.0014
	(0.0010)	(0.0007)	(0.0007)	(0.0050)	(0.0026)	(0.0016)
Net Worth	0.0002	0.0001*	0.0001*	0.0002	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Tobin's Q	0.2006***	0.0887***	0.0979***	0.1633***	0.0880***	0.0968***
	(0.0536)	(0.0051)	(0.0039)	(0.0383)	(0.0045)	(0.0035)
Tangibility	0.3532***	0.3360***	0.3374***	5.2863	3.7663*	3.0179***
	(0.0742)	(0.0394)	(0.0416)	(4.4987)	(1.9732)	(0.7061)
Log Sales	0.0064	0.0081*	0.0080*	-0.0404	-0.0248	-0.0177**
	(0.0076)	(0.0044)	(0.0046)	(0.0446)	(0.0201)	(0.0089)
Profitability	0.8358**	0.0610	0.1252***	0.6151**	0.0827**	0.1379***
	(0.3827)	(0.0372)	(0.0391)	(0.2781)	(0.0415)	(0.0396)
Observations	59963	59963	59963	59963	59963	59963
ρ^2	0.7173	0.5224	0.5386	0.6948	0.5508	0.5597
J-Statistic	0.0000	20.5874	49.9393	3.7452	26.3437	71.8084
$\tau -$ q	0.5625	0.6538	0.6383	0.5800	0.6562	0.6409
$\text{SE}_\tau -$ q	0.0392	0.0383	0.0368	0.0400	0.0383	0.0368
$\tau -$ Tangibility	.	.	.	0.0820	0.1050	0.1272
$\text{SE}_\tau -$ Tangibility	.	.	.	0.0576	0.0503	0.0318

Notes: This table reports leverage panel regression results from the higher-order cumulant estimators of Erickson et al. (2014). The first three columns report regression results where firms' market to book ratio is assumed to be measured with error. The columns report the higher-order cumulant estimator used in the estimator. The fourth, fifth, and sixth columns report regression results where firms' market to book ratio and their tangibility is assumed to be measured with error. ρ^2 reports an estimate of the R^2 of the regression. τ_i reports the measurement quality for proxy variable i. J-statistic reports the model over-identifying statistic from Sargan (1958). These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports firm financial data from Compustat.
Sources: DOL, FCMS, Compustat, and author's calculations.

1.6 Conclusion

The effect of RTW is large and persistent for unions and firms with agency shop agreements. Using a differential exposure design, I show that unions with agency fee payers lose 15 percent of their total receipts and 16 percent of their membership over five years, relative to unions without agency fee payers. A panel regression estimated on a novel data set that links local unions' balance sheet data to firms' balance sheet data shows that unions' financial conditions shape firms' financing decisions. A one percent increase in union receipts increases firm leverage by 110 basis points. These two research designs allow for a counterfactual analysis. This counterfactual analysis shows that, for the average firm in the sample, the decrease in exposed union income following with RTW is associated with a 0.5 standard deviation decrease in firm leverage, assuming that the firms' activity is limited to a state affected by the reforms included in the sample and the firms' allocation of activity remains fixed in the short run. The counterfactual effect of RTW on firms' leverage is consistent with prior estimates found by Matsa (2010) and Chava et al. (2020).

Labor unions' financial conditions determine the effect of labor regulations on firms' capital structure. Unions' financial health is not only central to their capacity to organize and retain membership, but also to their bargaining power in negotiations with firms. As unions' revenues shape the strategic use of debt by firms, so too may the financial health of other input suppliers affect a firm's capital structure. Future work should consider the effect of supplier's revenue and net worth on firms' financing decisions.

The heterogenous response of labor federations to RTW suggests the importance of labor federations in unions' membership dynamics. Local unions partially offset the loss of income from RTW through financial transfers within state and national labor federations. The presence of these transfers presents another question: Is the fiscal solidarity provided by national federations optimal? Labor federations often allocate funding across political lobbying, external consultancies, and state-level organizing differently. Future work should consider the determinants of spending patterns within federations, and whether these spending patterns are responsive to the organizational needs of their affiliates.

CHAPTER II

A New Measure Global Series of Corporate Tax Shocks

with Isaac Baley[1] and Andrés Blanco

2.1 Introduction

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

[1]Baley acknowledges financial support from the ERC Grant MacroTaxReforms and the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Programme for Centres of Excellence in R&D (CEX2019-000915-S).

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 policy. 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.

2.2 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.

2.2.1 Data Description

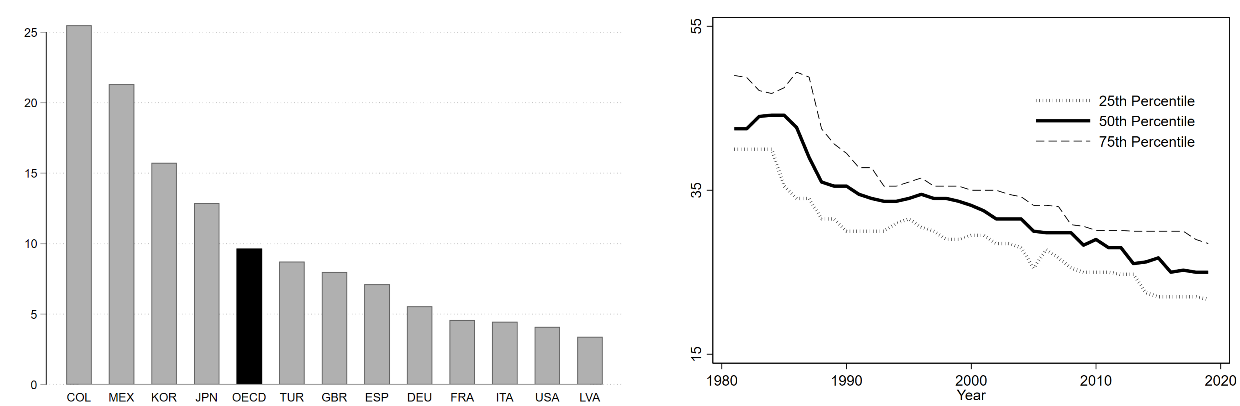
Our data consists of an annual unbalanced cross-country panel for 36 countries for the period 1960–2019. To assemble the cross-country panel on statutory corporate tax reforms, we gather several dispersed sources. Data on statutory corporate tax rates comes from Vegh and Vuletin (2015) for the corporate 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 B.1 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 3.1). The importance of corporate taxation remains large, despite a generalized falling trend in tax rates over the last four decades (Panel B in Figure 3.1); 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 2.1 – 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.

2.2.2 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.

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 2.2 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.

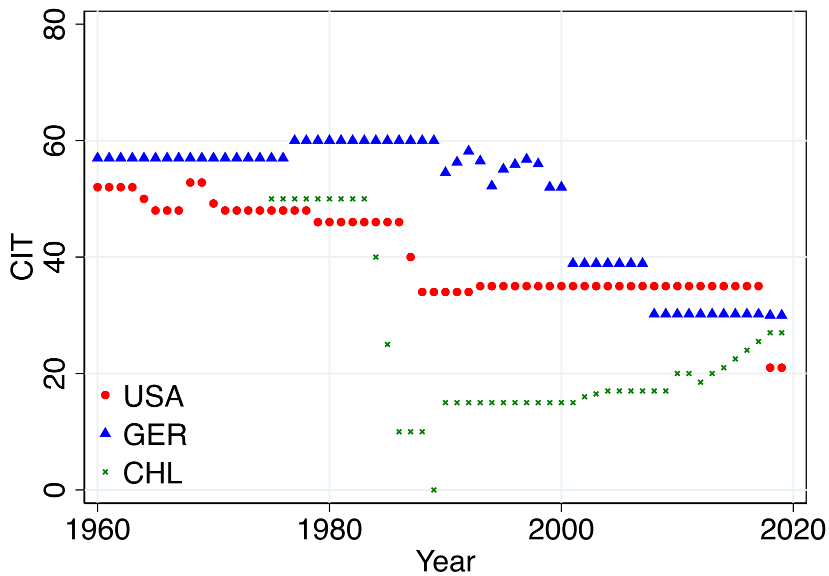
Focusing on the US (red dotted series), there are several adjacent periods of particular interest. In 1964, the Kennedy Administration lowered corporate tax rates with the *Tax Reduction Act*.[3] The motivation for this tax decrease was to improve long-run growth, and thus we consider it as exogenous 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] These episodes clearly highlight the aim of our strategy: eliminate the

[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 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

Figure 2.2 – Corporate Income Tax Rate



Notes: Top statutory marginal corporate income tax rate in US, Germany and Chile between 1960 and 2020. See Appendix B.1 for details.
Source: Vegh and Vuletin (2015)

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.

2.2.3 Strategy

Our strategy uses a non-parametric filter to identify permanent tax changes by their time-series properties. As we show in the previous section, the permanent changes in the top statutory rate identified by our filter are typically motivated by long-run considerations. This is may not always be the case. In certain cases, it is optimal for the government to enact a persistent tax hike following a temporary change in government spending, as shown

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.

by Barro (1979).

The narrative literature uses the text of government documents to classify changes in tax revenue 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. If permanent tax changes are motivated by long-run objectives, then the permanent reforms identified by our filter should coincide with the reforms motivated by long-run objectives identified by narrative approaches. Our non-parametric approach does not incorporate changes in the tax base or distribution of effective rates. The following equation describes the relationship between our tax instrument and the tax instrument

$$\underbrace{\Delta\text{revenue}}_{\text{narrative}} = \underbrace{\Delta\tau}_{\text{non-parametric}} \times \text{base} + \tau \times \Delta\text{base} \tag{2.2.1}$$

where τ is the top statutory tax rate. 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 2.2, is far from normal. While the Kalman filter provides the 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

[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.

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 $X_{t,i}^P$ and a transitory component $X_{t,i}^T$ in the following way:

$$\tau_{t,i} \;=\; \frac{1}{1+e^{X_{t,i}^P+X_{t,i}^T}} \;\in\; [0,1]. \tag{2.2.2}$$

The permanent component follows a markov process

$$X_{t,i}^P \;=\; \begin{cases} X_{t-1,i}^P & \text{with probability } 1-\lambda_R \\ X_{t-1,i}^P \;+\; R_{t,i} & \text{with probability } \lambda_R \end{cases}, \tag{2.2.3}$$

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 μ_R and dispersion σ_R. The parameter λ_R 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

$$X_t^T \;=\; \begin{cases} 0 & \text{if } S = 1 \\ \eta_t & \text{if } S = 2 \end{cases}. \tag{2.2.4}$$

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

[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 2.1 – 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_R, σ_R)	$(0.393, 0.076)$
Arrival rate of permanent reform	λ_R	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.

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 $(\mu_R, \sigma_R, \sigma_T, \lambda_R, q_{11})$ 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 2.1 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 2.2 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 2.2 – Moments in the Model and Filtered Moments

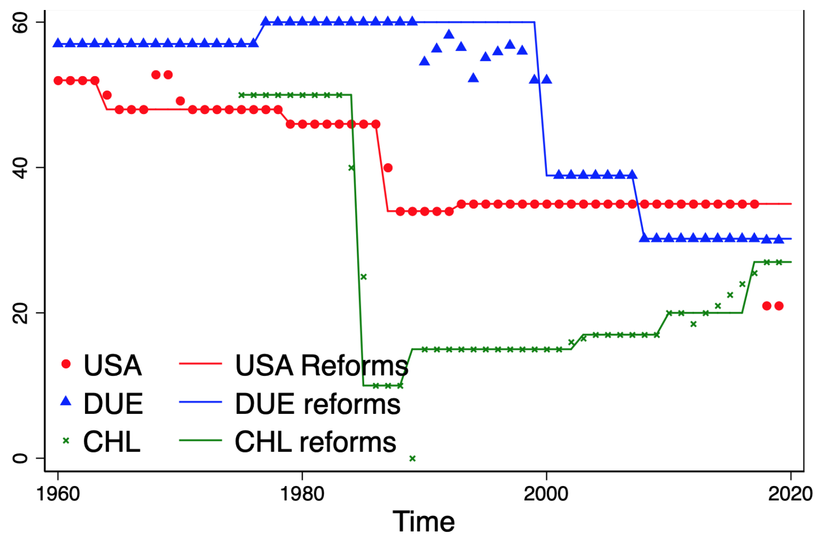
Moment	Model	Break-Test	Kehoe-Midrigan
Frequency reforms	0.052	0.052	0.041
Type I error for no reforms			
Reform t given no reform t		0.009	0.0112
and $T-3>t>3$		0.009	0.0112
Type II error for no reforms			
No reform t given reform t		0.160	0.396
and $T-3>t>3$		0.110	0.338
and no reform in $t-1$, t, $t+1$		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.2 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 2.2 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 2.3 – 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 B.1 for details. Source: Vegh and Vuletin (2015) and authors' calculations.

Figure 2.3 shows the same corporate tax raw series (dots) as Figure 2.2 and the filtered permanent reforms series (solid lines). In Appendix B.1, Figures B.2 and show these series for each country in the sample.

2.2.4 New Series of Corporate Tax Reforms

Let us describe the statistical properties of our new series. Table 2.3 presents the summary statistics of our new measure against the unfiltered corporate income tax changes in our sample.

Table 2.3 – Statistics of Permanent Corporate Tax Reforms

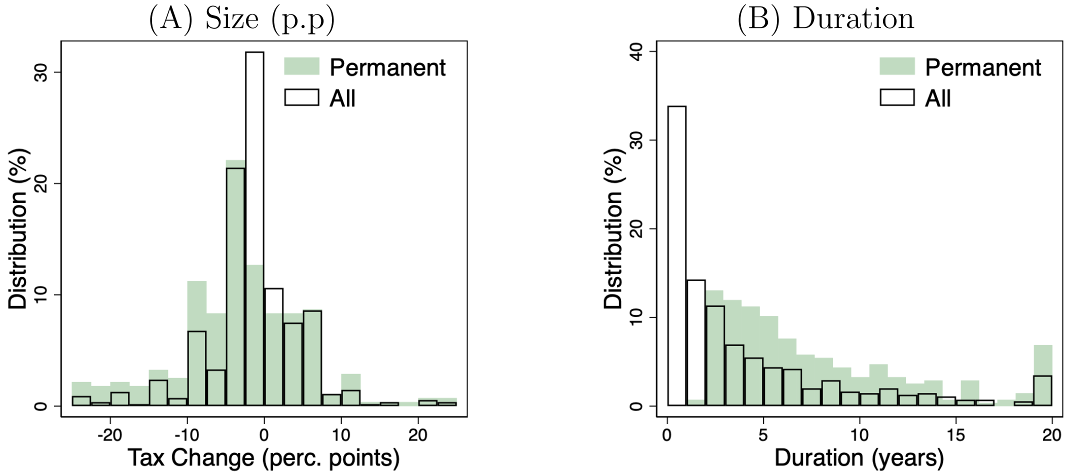
	All Tax Changes	Permanent
Size (p.p.)	-1.95	-3.51
	(6.41)	(8.81)
Duration (years)	3.80	7.93
	(5.46)	(6.10)
N	546	276

Notes: Authors' calculations using filtered series of permanent tax reforms. See Appendix B.1 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 2.4 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, 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 2.4 reports how the number, size, and duration

Figure 2.4 – Size Distribution of Corporate Income Tax Reforms



Notes: Distribution of filtered corporate tax reforms. See Appendix B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

of permanent tax reforms have evolved over the preceding decades.

Table 2.4 – 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 B.1 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.

2.2.5 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.

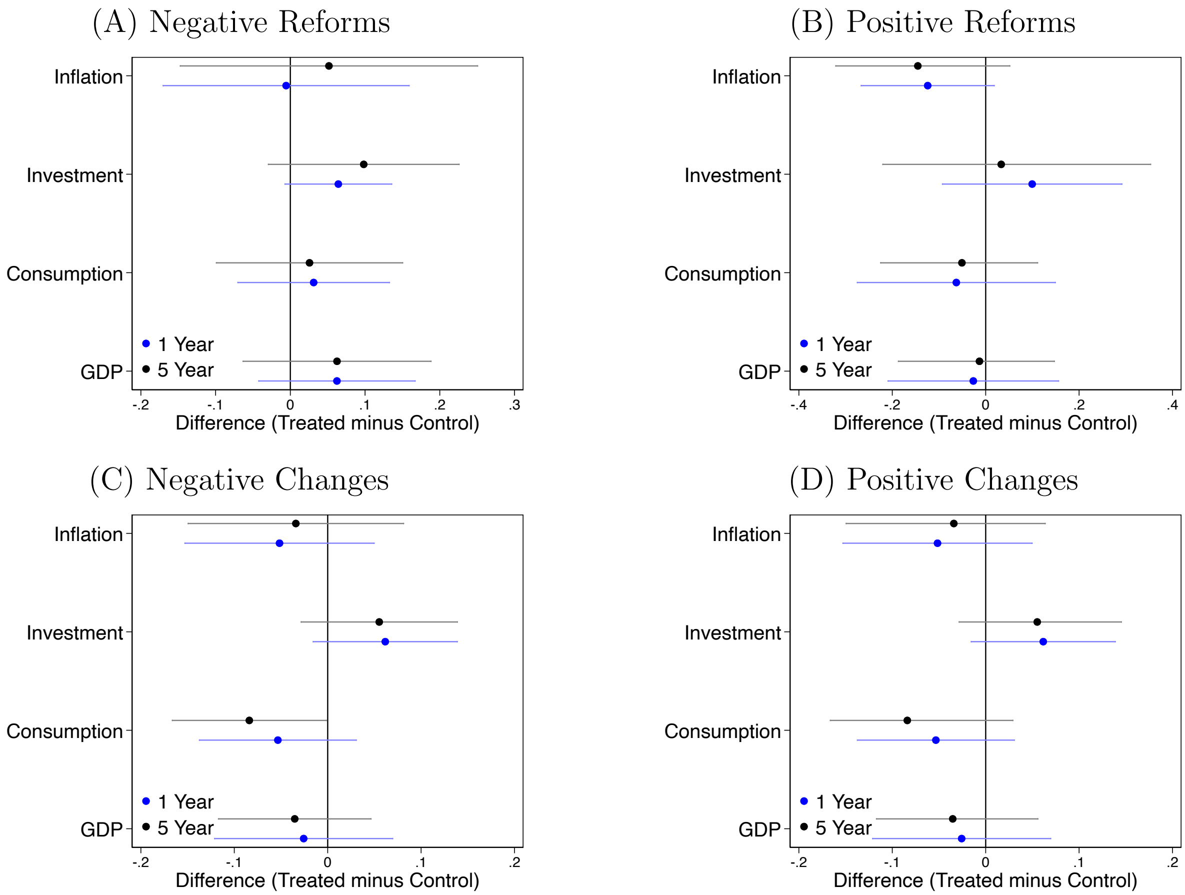
Are permanent tax reforms random? To test whether permanent tax reforms occur more frequently in certain economic states, we test the null hypothesis of "balance" across treatment (reform) and control groups. Figure 2.5 shows that we fail to reject the null of equality for contemporaneous variables and 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 2.5 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

2.2.6 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

Figure 2.5 – Covariate Balance across Treatment and Control Groups

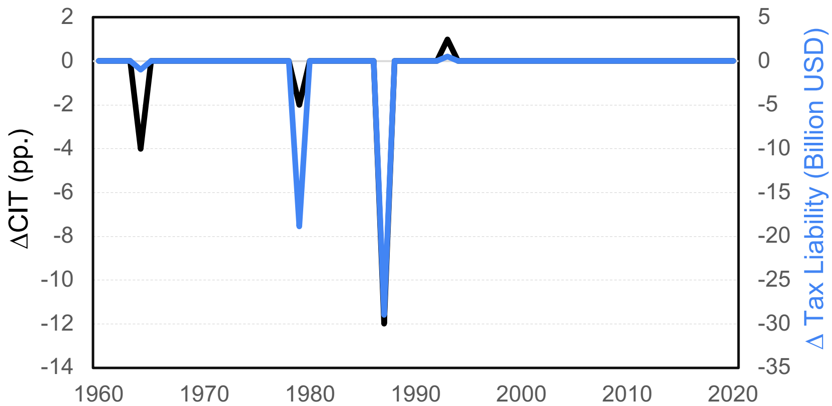


Notes: The table shows balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. See Appendix B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

the change in tax liabilities in billions. The variation in liabilities due to corporate income tax changes comes from Romer and Romer (2010) appendix.

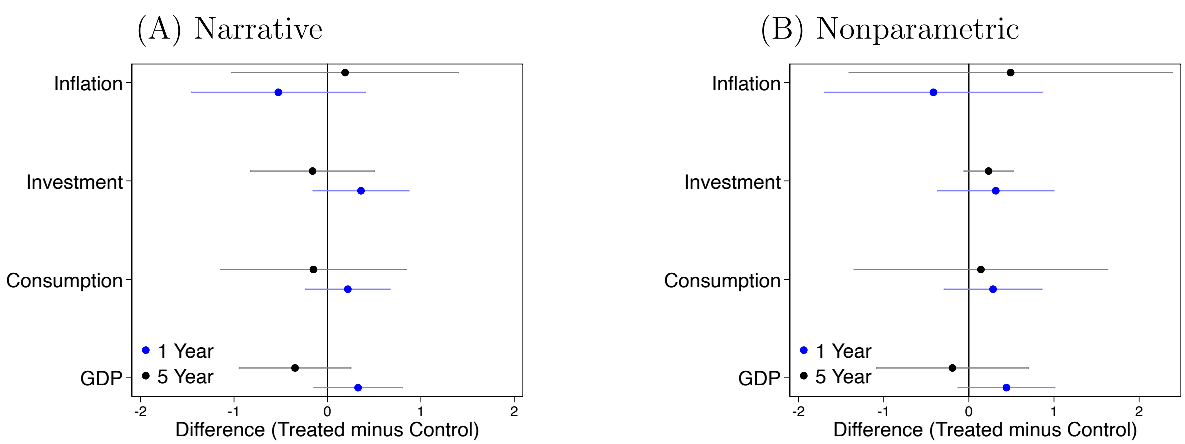
Figure 2.6 plots our baseline series against the current value of the change in liabilities of the three permanent negative changes. Our baseline tax reform measure includes all four exogenous reforms found in the narrative measure. Moreover, records show that policymakers intended for all of these tax 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 2.7 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

Figure 2.6 – U.S.A. Tax Reforms: Nonparametric and Narrative Approaches



Notes: See Appendix B.1 for details.
Source: Romer and Romer (2010) and authors' calculations.

Figure 2.7 – 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 B.1 for details.
Source: Romer and Romer (2010) and authors' calculations.

groups, such that the passage of permanent corporate tax reforms is not correlated with certain economic states. Appendix B.2 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).

2.3 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.

2.3.1 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:

$$\Delta y_{i,t+h} \;=\; \alpha_i^h \;+\; \gamma t \;+\; \sum_{j=0}^{M} \beta_j^h \Delta T_{i,t-j} \;+\; \sum_{j=0}^{N} \gamma_j^h \Delta y_{i,t-j} \;+\; \varepsilon_{i,t+h}, \tag{2.3.1}$$

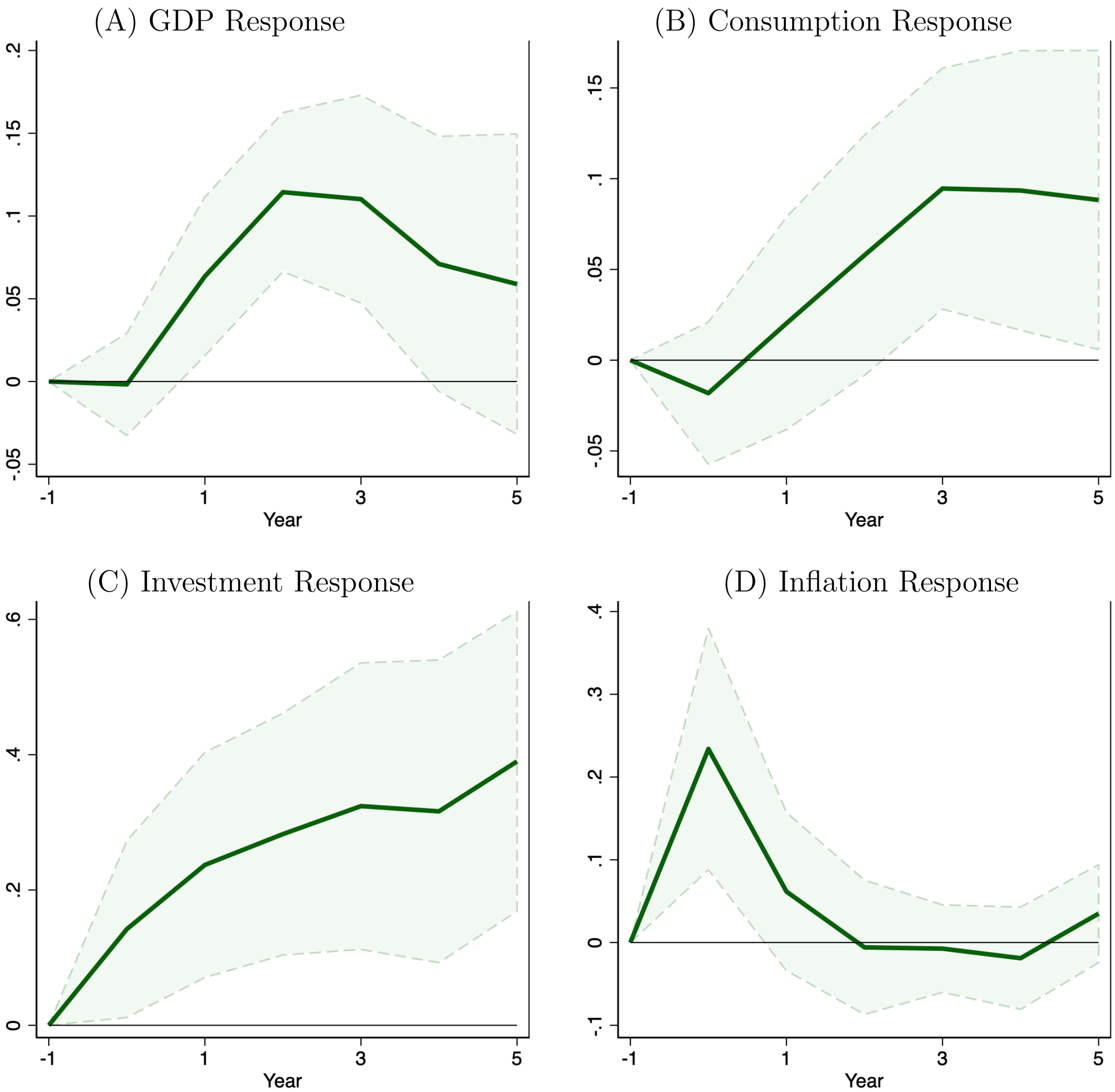
where $\Delta y_{i,t+h} \equiv y_{i,t+h} - y_{i,t}$, α_i^h is a set of country fixed-effects that controls for idiosyncratic trends; $\gamma_t^h = \gamma t$ is a time fixed effect that control for global trends; $\Delta T_{i,t-j}$ is a change in the permanent tax rate, and $\varepsilon_{i,t+h}$ is a 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 β^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 2.8 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 2.8 presents our main result from our simple regression without controls. It plots the coefficient β_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

Figure 2.8 – Response of Macro Aggregates to Corporate Income Tax Reform

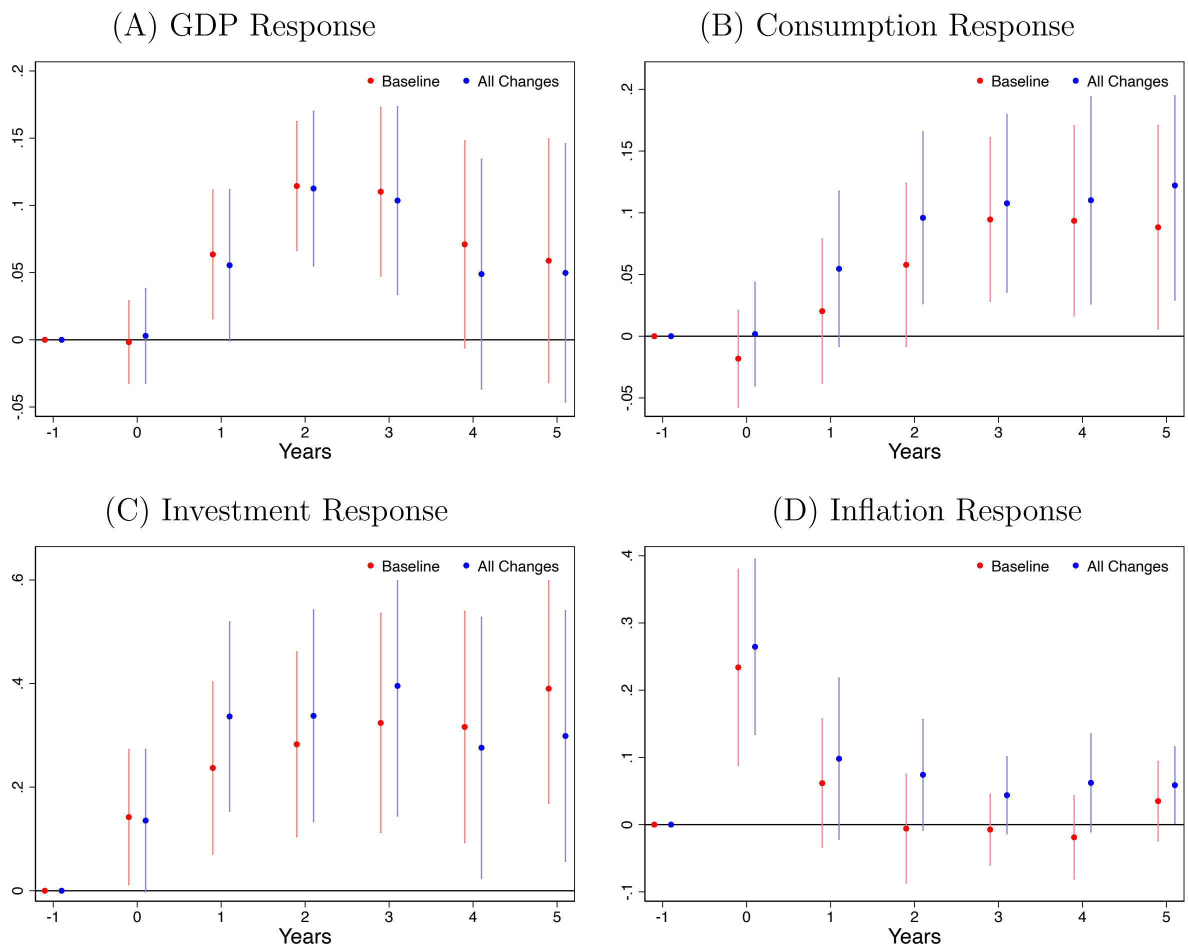


Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cut. Solid = Coefficient β^h for various horizons h. Dashed = 95% confidence intervals. Panel (A) considers output response to permanent corporate tax reforms. 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 B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

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 2.9 presents estimates from our simple regressions without controls for all corporate tax changes, and Figure 2.10 presents estimates of the effect of temporary tax changes without controls.

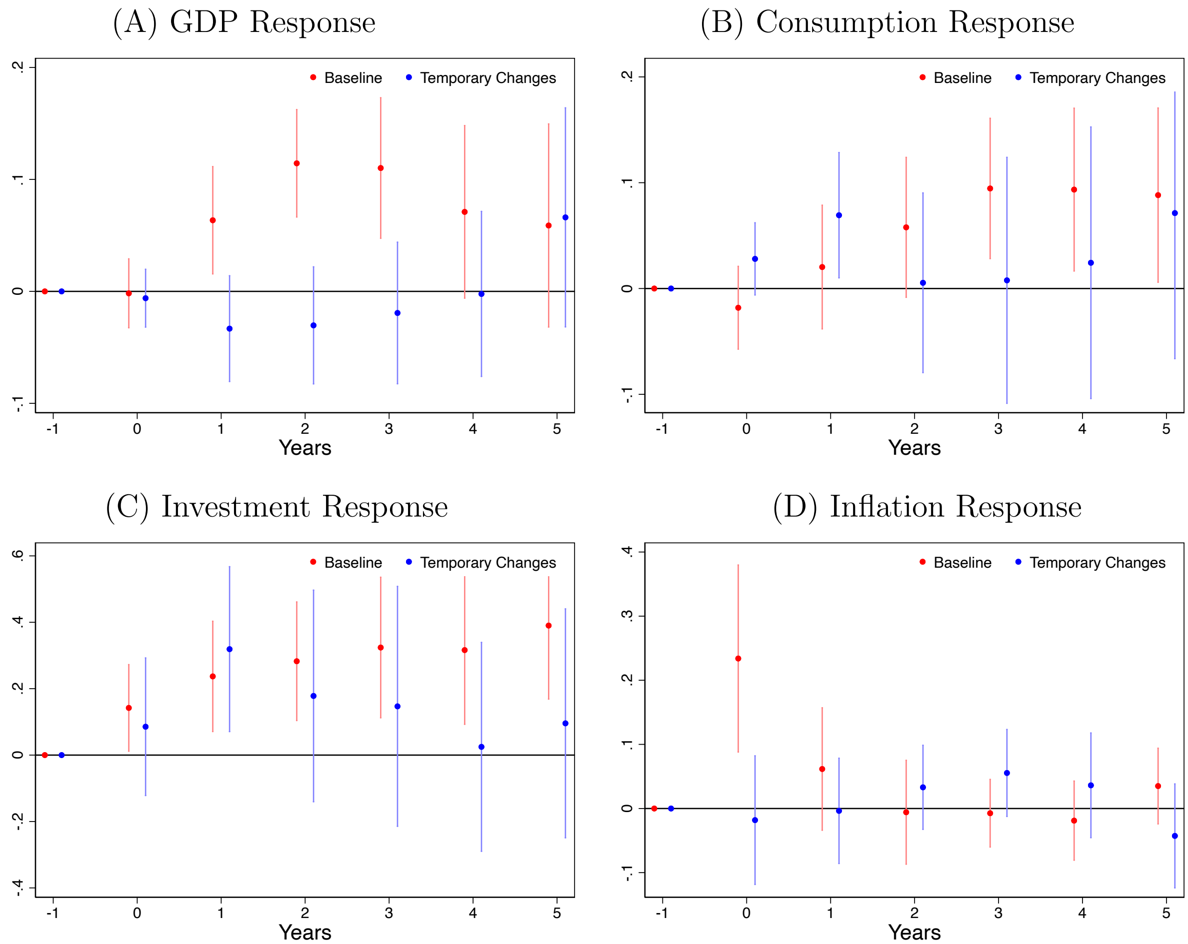
Figure 2.9 – Response of Macro Aggregates to All Corporate Income Tax Changes



Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cut. Solid = Coefficient β^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 B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

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

Figure 2.10 – 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 β^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 B.1 for details. Source: Vegh and Vuletin (2015) and authors' calculations.

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. 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.

2.3.2 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 (2.3.1). 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.

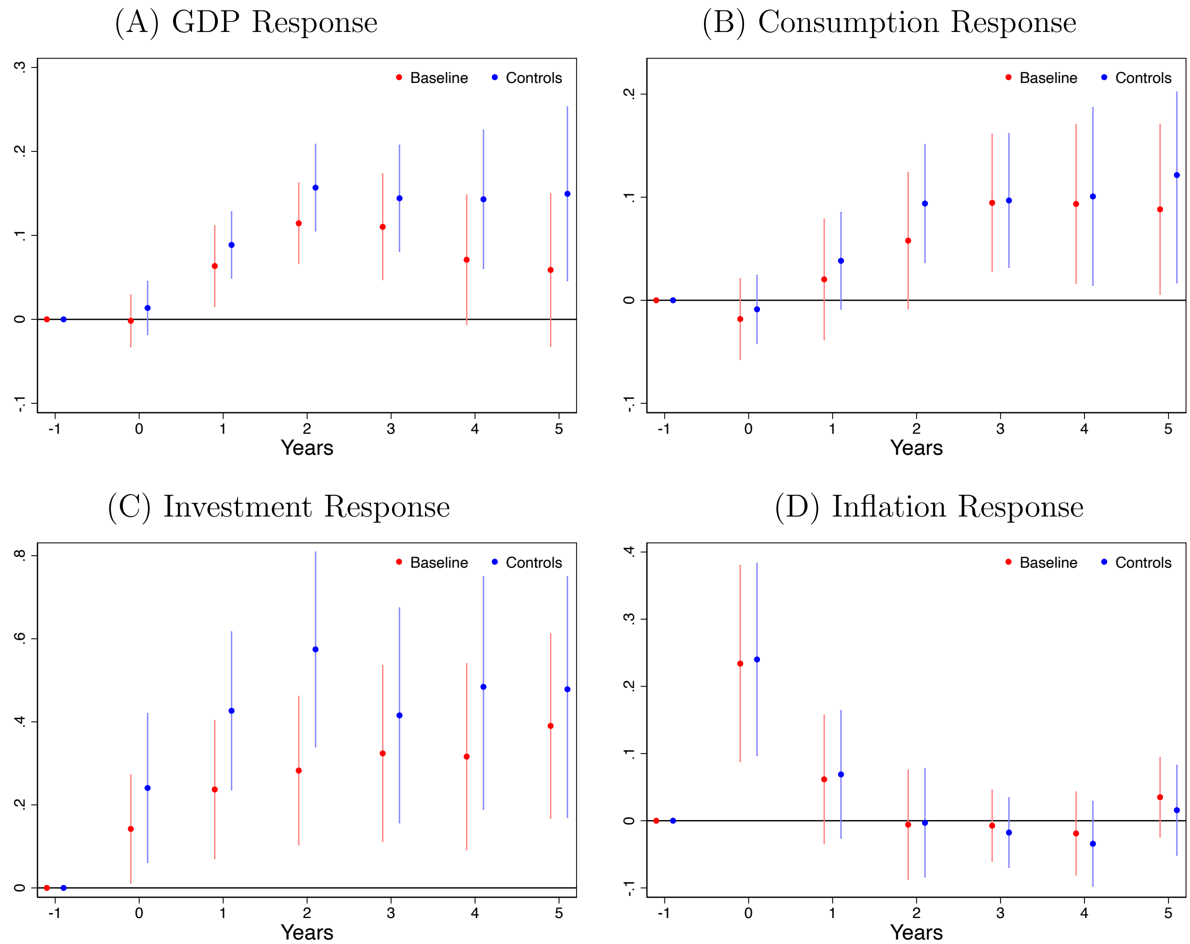
$$\Delta y_{i,t} \ = \ \alpha_i^h \ + \ \gamma t \ + \ \sum_{j=0}^{M} \beta_j \Delta T_{i,t-j} \ + \ \sum_{j=0}^{N} \gamma_j \Delta y_{i,t-j} \ + \ \sum_{j=0}^{N} \nu_j x_{i,t-j} \ + \ \varepsilon_{i,t}, \qquad (2.3.2)$$

Figure 2.11 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 2.13 plots local projections for all tax changes, and Figure 2.12 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

Figure 2.11 – Response of Macro Aggregates to Corporate Income Tax Reform with Controls

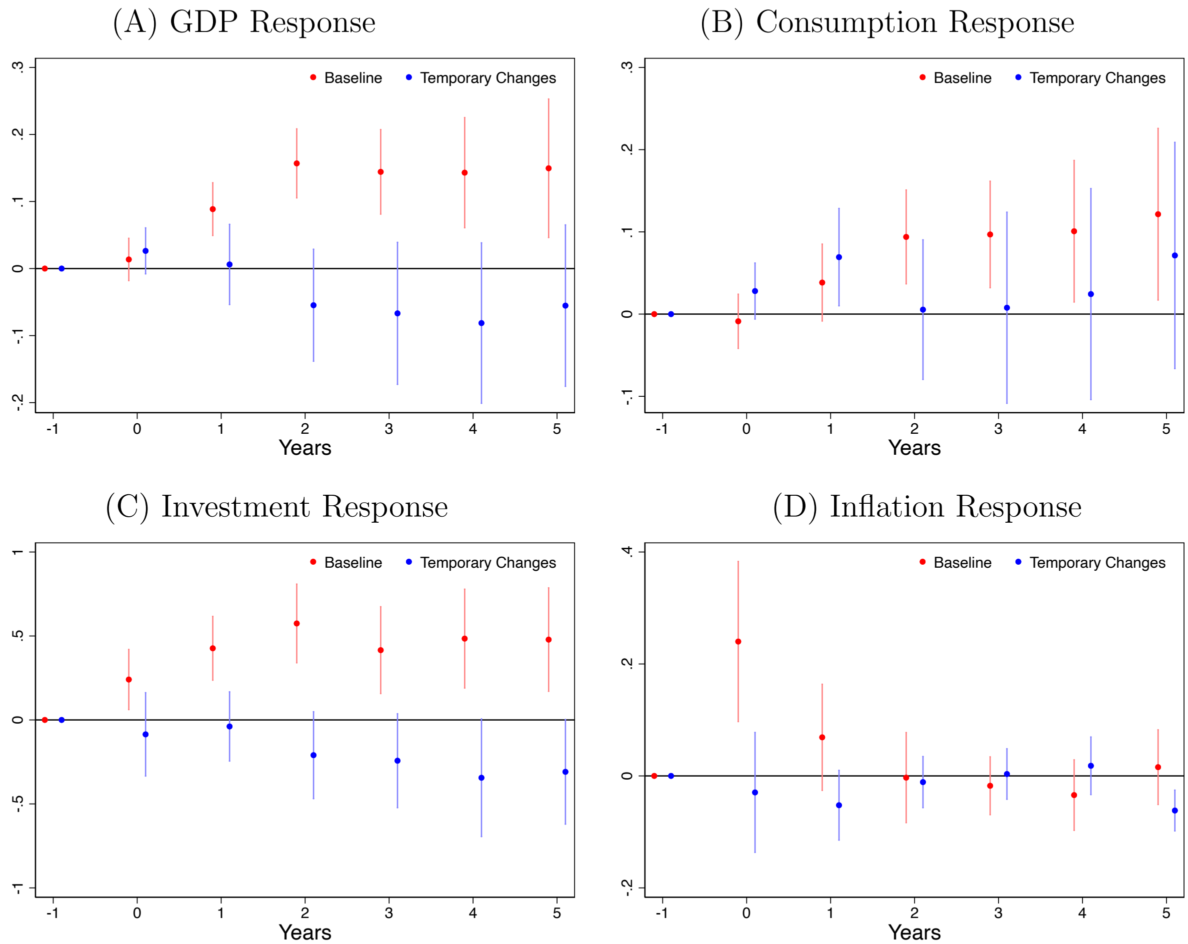


Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cuts, including controls for either output growth or inflation for real and nominal variables, respectively. Solid = Coefficient β^h for various horizons h. Dashed = 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 B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

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

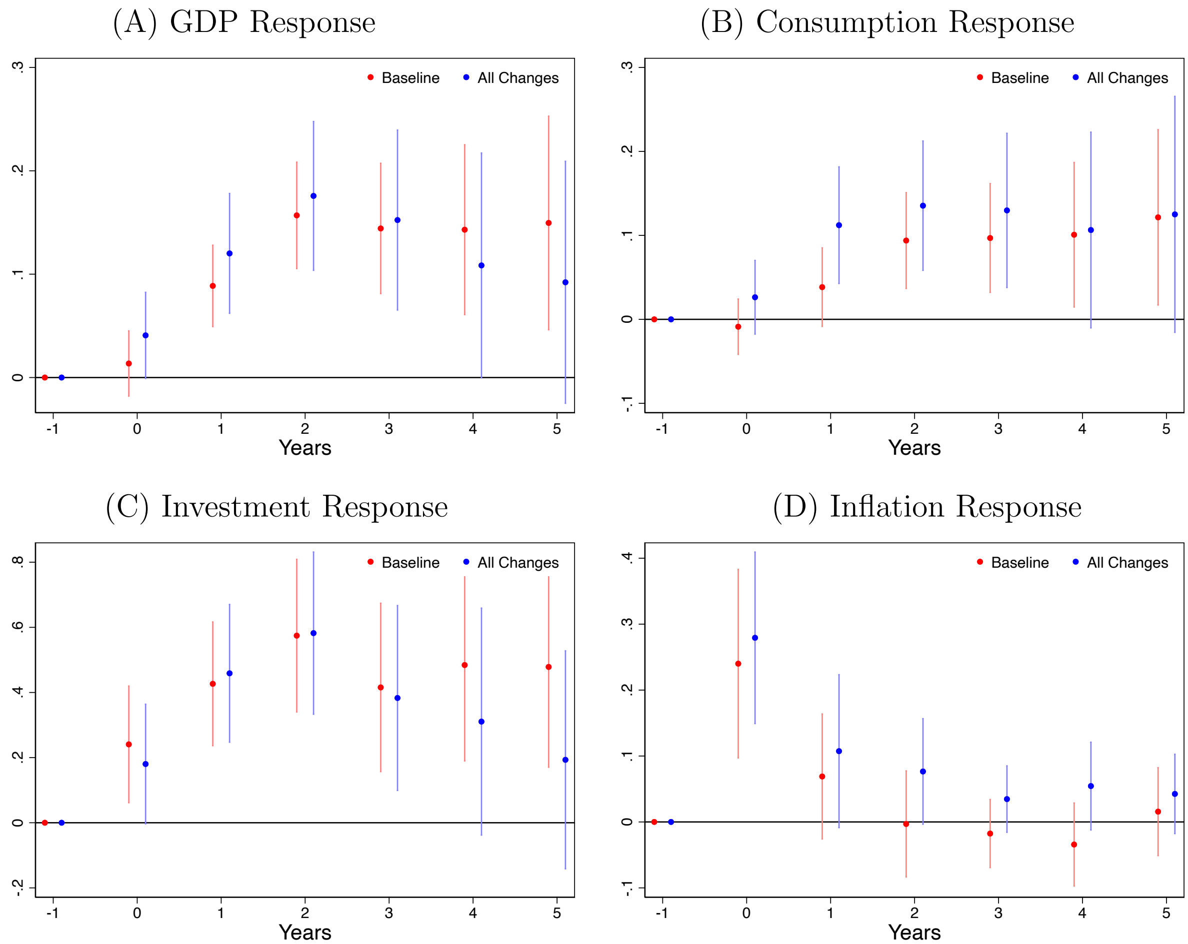
Figure 2.12 – Permanent v. Temporary Corporate Income Tax Changes with Controls



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 β^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 B.1 for details. Source: Vegh and Vuletin (2015) and authors' calculations.

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

Figure 2.13 – 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 β^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 B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

tax reforms.

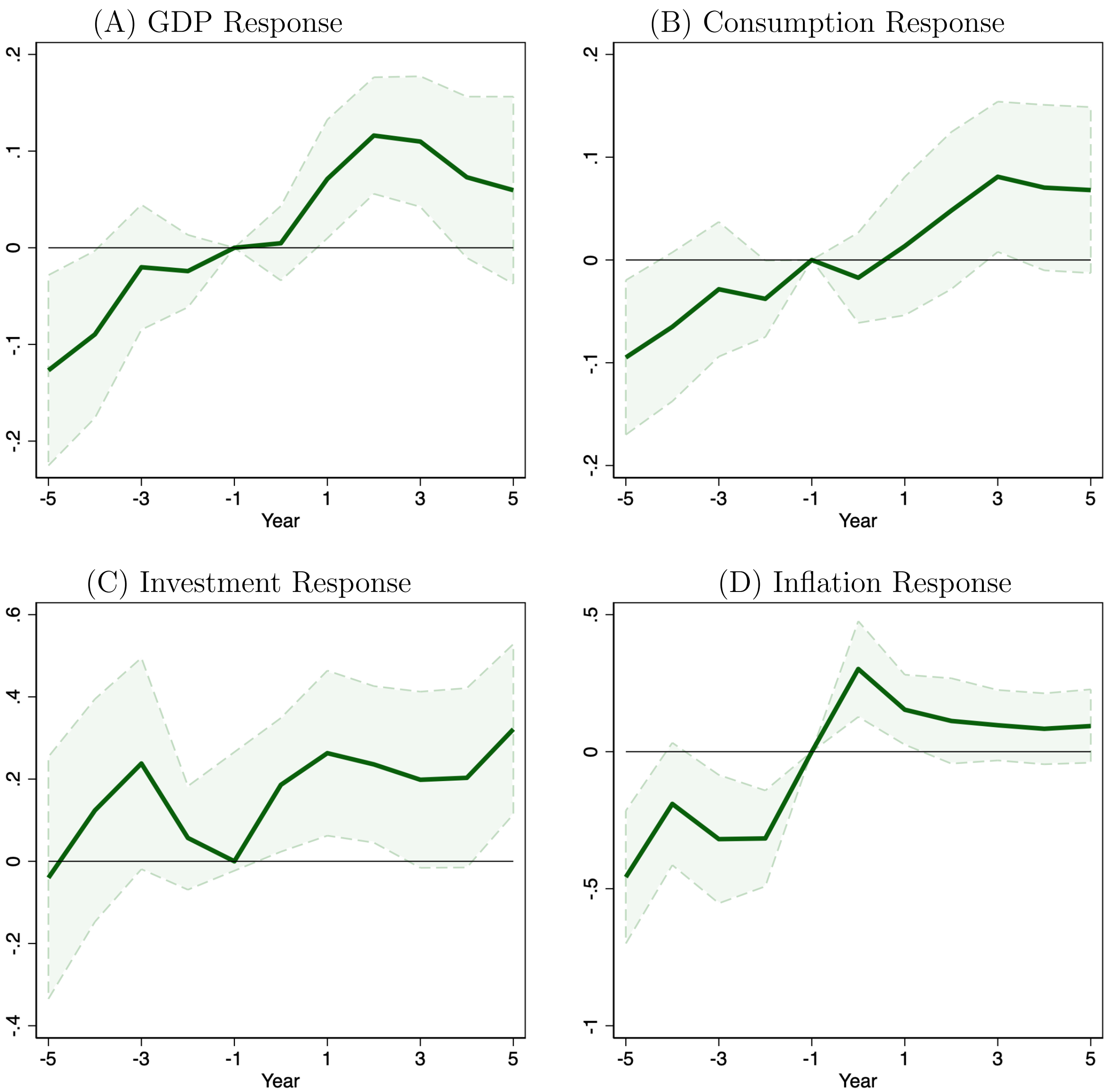
2.3.3 Anticipatory Effects of Permanent Corporate Tax Reforms

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 2.14 plots the result of this specification.

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 2.15 plots local projection for all tax changes, and Figure 2.16 plots local projection for temporary tax changes.

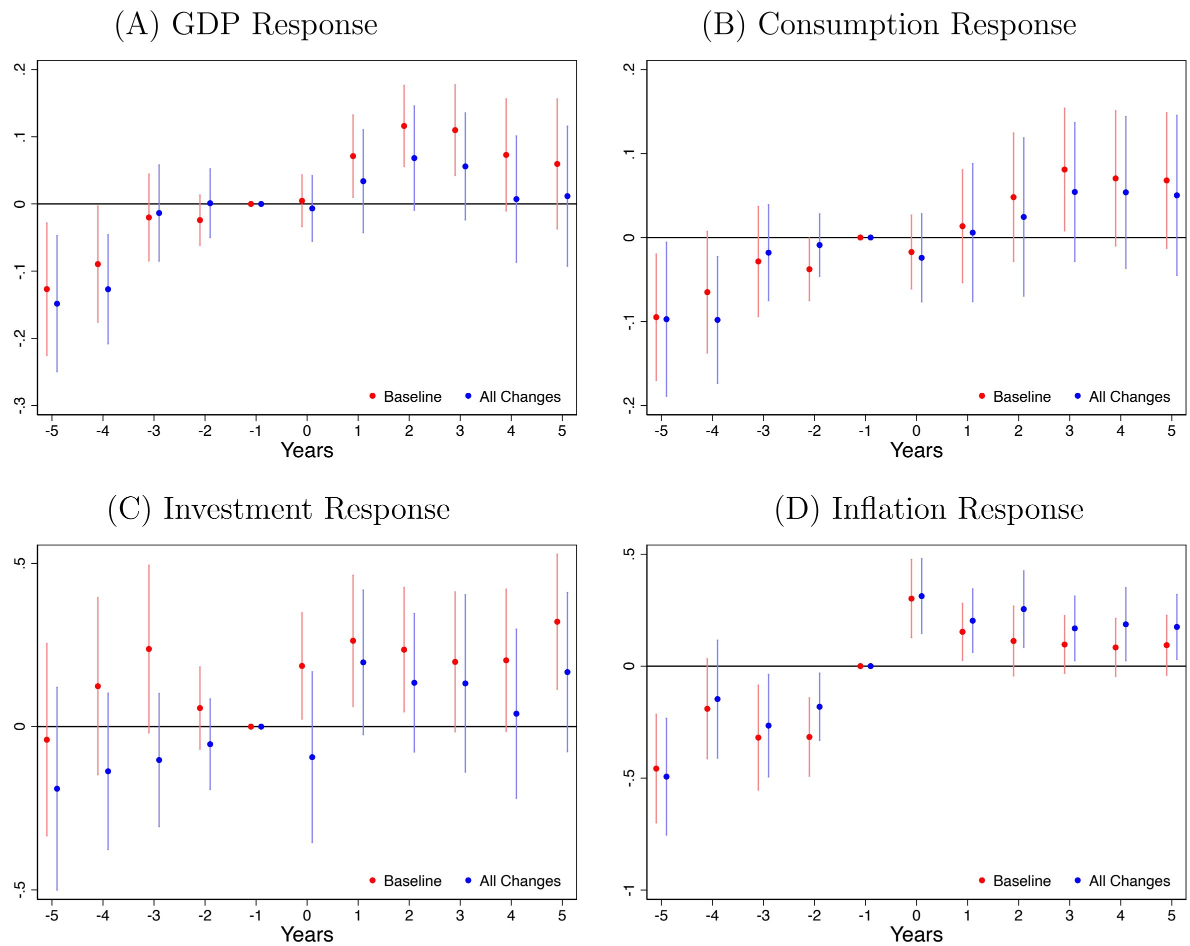
We observe greater anticipatory response of GDP and investment preceding arbitrary and temporary corporate tax changes, while the anticipatory response of consumption is smaller. While there is anticipation in prices for permanent and temporary tax changes, this may be due to the limited lags of the 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 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.

Figure 2.14 – Anticipatory Effects of Permanent Corporate Tax Reforms



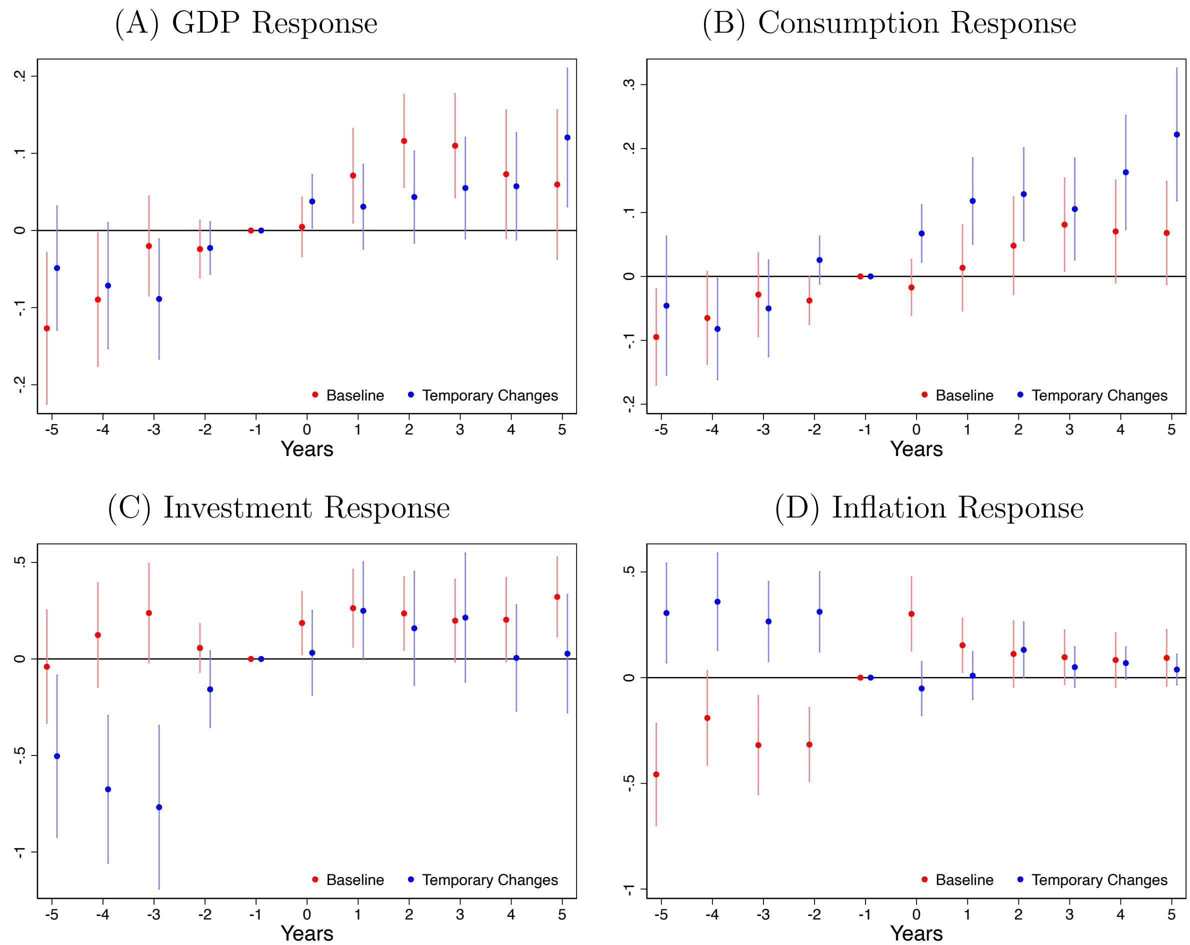
Notes: All effects reported in percentage points. Output, consumption, investment, and inflation responses to persistent corporate income tax cuts, including controls for either output growth or inflation for real and nominal variables, respectively. Solid = Coefficient β^h for various horizons h. Dashed = 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 B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

Figure 2.15 – 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 β^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 B.1 for details. Source: Vegh and Vuletin (2015) and authors' calculations.

Figure 2.16 – 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 β^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 B.1 for details.
Source: Vegh and Vuletin (2015) and authors' calculations.

2.4 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.

CHAPTER III

Cyclicality of Investment Volatility: Implications of Specification Choice

3.1 Introduction

Firms face nonlinear frictions (e.g. fixed costs, adverse selection) when they invest, which prevent them from continuously adjusting their capital stock (Caballero et al. (1995), Baley and Blanco (2021)). When the firms cannot adjust their capital stocks frictionlessly, their investment patterns exhibit periods of inaction followed by moments of activity. In structural investment models, firms' implied propensity to invest explains this observed behavior. When firms' propensity to invest varies over time, the aggregate investment rate exhibits conditional heteroskedasticity. Specifically, aggregate shocks and past investment decisions determine the conditional volatility of the aggregate investment rate. Bachmann et al. (2013) (BCE, hereafter) document that periods of heightened aggregate investment volatility follow protracted periods of high aggregate investment. While their specifications measure the state-dependence of the volatility of aggregate investment, their specifications only indirectly measure the cyclicality and persistence of the volatility of aggregate investment.

In this paper, I estimate the conditional volatility of aggregate investment and its components as an autoregressive-moving-average process (ARMA) using standard generalized autoregressive conditional heteroskedasticity (GARCH) estimators. Unlike the specifications in BCE, these alternative specifications do not measure the volatility of aggregate investment as a function of the lagged average of aggregate investment. As such, they offer a more direct estimate of the persistence of the conditional volatility of aggregate investment. Using the implied volatility of aggregate investment recovered from these alternative specifications, I test whether volatility systematically varies across business cycles. I show that the aggregate volatility of total and equipment investment are acyclical and exhibit low persistence, while the volatility of structure investment is countercyclical and exhibits high persistence.

First, I motivate the choice to estimate the conditional volatility of aggregate investment as an ARMA process. To do so, I examine the squared residuals of a univariate autoregression on the aggregate investment rate. I present the auto-correlation functions of these squared residuals, alongside the auto-correlation functions of aggregate investment. The squared residuals of aggregate investment exhibit low persistence. I then estimate the correlation between these squared residuals and output across different horizons. The squared residuals of total and equipment investment are acyclical, while the squared residuals of structure investment are countercylical. These results do not motivate the choice of BCE to use the lagged average of aggregate investment, a highly persistent and cyclical variable, to measure the persistence and cyclicality of the conditional heteroskedasticity of aggregate investment. Instead, these results motivate the use of alternative specifications which estimate aggregate investment's conditional volatility's autoregressive and moving-average components directly.

Second, I estimate the implied aggregate investment volatility using alternative GARCH estimators. These specifications, which assume that aggregate investment volatility follows an ARMA process, estimate the auto-regressive component and moving average component of the volatility of aggregate investment. I find that this autoregressive component is small and statistically insignificant for total and equipment investment, and large and statistically significant for structure investment. That said, the moving average component is positive and significant for all components and total investment. This suggests that past innovations are informative of the future investment volatility, but that the conditional volatility of aggregate investment exhibits low persistence.

Third, I benchmark the performance of these alternative GARCH estimators against the BCE specifications in US data and simulated environments. First, I show that the BCE specification is isomorphic to an asymmetric GARCH(∞) estimator, which estimates the relationship between the conditional volatility of aggregate investment and previous investment residuals. This implicit weighting scheme explains why BCE specifications are sensitive to sample selection and outliers. Second, I compare the performance of the two specifications in environments simulated using the canonical heterogenous firm models of Khan and Thomas (2008) and Winberry (2021). These comparisons clarify the use of the BCE specification. The BCE specifications test whether the volatility of aggregate investment depends on past investment behavior, but does not measure the cyclicality or persistence of the volatility of aggregate investment.

This project informs previous empirical and quantitative work linking aggregate dynamics to microeconomic firm investment dynamics. The former motivated the latter. Specifically, Caballero et al. (1995) document that higher-order moments of US aggregate investment rate are non-gaussian. In simulated environments, non-gaussian higher-order moments can result from firms responding to gaussian shocks in the face of non-convex investment costs. BCE show that aggregate investment is more sensitive following periods of increased investment. Under certain structural assumptions, these papers present direct evidence of the relevance of lumpy investment dynamics for analyzing higher-order moments of country-level investment data.

This project also contributes to the literature that studies firms' investment models with non-convex adjustment costs. Researchers evaluate the performance of these models indirectly, through simulated method of moments, matching the time-series dynamics of the interest-rate demand elasticity of investment across firms. Following the insights of Winberry (2021) and Baley and Blanco (2021), researchers have been able to match the underlying distributional dynamics of the microeconomic data, but have not evaluated whether these models can match the cyclicality and persistence of the conditional volatility of the aggregate investment rate.

This project also contributes to the heterogenous agent literature more broadly. The use of conditional heteroskedasticity as an empirical target for structural models is not limited to investment models. Berger and Vavra (2015) also observe that aggregate durable expenditures exhibit conditional heteroskedasticity. As such, this work offers guidance on how to best estimate and benchmark future models, wherein state dependence may produce aggregate conditional heteroskedasticity.

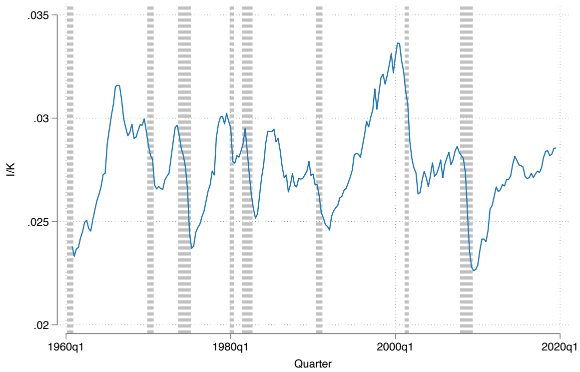
3.2 Data and Framework

This section summarizes the data and empirical specifications used in the subsequent analysis. I construct quarterly aggregate investment rates for total, equipment, and structure investment using data from the Bureau of Economic Analysis (BEA), using the process employed in BCE. The analysis sample spans 59 years from 1960 through the end of 2019. Appendix C.1 discusses the data construction and validation process in detail.

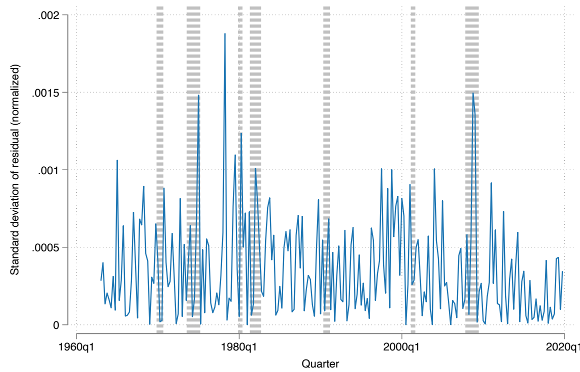
Figure 3.1 plots the U.S. aggregate investment rate. The aggregate investment rate exhibits observable volatility clustering, suggesting that the underlying data generating process is heteroskedastic. This clustering is most apparent when one views the standard deviation of a univariate autoregression of the aggregate investment rate. Panel B reports the standard deviation of the residual, which is calculated by taking the square root of the squared residuals of a univariate autoregression of the aggregate investment rate with a lag order of 6. The standard deviation exhibits limited, short-lived volatility clusters. This observable feature of the data motivates the use of GARCH models to explain variation in the time series.

Figure 3.1 – US Non-Residential Private Fixed Investment

(A) Total Investment



(B) Standard Deviation of Residual



Notes: Panel A reports the U.S. aggregate investment rate calculated following the procedure in BCE. Panel B reports the standard deviation of the residual, which is calculated by taking the square root of the squared residuals of a univariate autoregression of the aggregate investment rate with a lag order of 6. Appendix C.1 discusses the data construction process in detail.
Sources: BEA and author calculations.

This paper features two families of GARCH estimators. The BCE estimator is a GARCH-X estimator, which includes explanatory variables outside of the autoregressive and moving average terms. The alternative estimators presented here are standard GARCH estimators, which include a combination of autoregressive and moving average terms. These specifications can be described by the following system of equations:

$$x_t = \sum_{j=1}^{p} \phi_j x_{t-j} + \varepsilon_t, \tag{3.2.1}$$

$$\varepsilon_t = \sigma_t e_t, \tag{3.2.2}$$

$$\sigma_t^2 = f_v(\sigma_{t-1}^2, \varepsilon_{t-1}^2, \mathbf{m}_t). \tag{3.2.3}$$

with a conditional mean function of $f_m(.)$, a conditional variance function of $f_v(.)$, conditional variance covariates $\mathbf{m}_t$, conditional mean lag order p, and i.i.d. structural innovations $e_t \sim N(0,1)$. The estimation procedure proceeds in two steps. First, I estimate the autoregression of the aggregate investment rate with the same lag order as BCE. Second, I estimate the conditional variance function $\hat{\sigma}_t$ using the squared residuals ε_t^2 recovered from the first stage. The conditional variance function can be estimated using a least-squared or maximum likelihood estimator. This paper presents the following two alternative GARCH estimators:

$$\sigma_t^2 = \alpha + \beta_a \varepsilon_{t-1}^2, \tag{3.2.4}$$

$$\sigma_t^2 = \alpha + \beta_g \sigma_{t-1}^2 + \beta_a \varepsilon_{t-1}^2. \tag{3.2.5}$$

The first estimator is a GARCH(0,1) with a single moving average term, and the second estimator is a GARCH(1,1) with a moving average and autoregressive term. GARCH models with an autoregressive coefficient cannot be estimated using OLS, because lagged volatility σ_{t-1} is a latent variable. As these estimators include latent variables, I estimate them using MLE. The original BCE specification estimates $\hat{\sigma}_t$ as a function of the lagged average of aggregate investment using OLS. BCE estimates $\hat{\sigma}_t$ using the following specification:

$$\left(\sigma_t^{BCE}\right)^2 = \alpha + \eta \bar{x}_{t-1}^k. \tag{3.2.6}$$

$$\hat{\eta} = \frac{\mathbb{C}ov\left(\hat{\varepsilon_t}^2, x_{t-1}^k\right)}{\mathbb{V}\left(x_{t-1}^k\right)} \tag{3.2.7}$$

where the lag order k is chosen to maximize the AIC for their initial sample (1960q1-2005q4). The parameter η measures the relationship between the conditional volatility and the lagged average of the aggregate investment rate. They estimate $\hat{\eta}$ in two steps. In the first step, they estimate equation 3.2.1 using OLS and recover the residuals ε_t. In the second step, they square the residuals and regress them onto an intercept and the lagged average of aggregate investment. As structural innovations have a zero mean, this second step estimates equation 3.2.6, where the coefficient of interest represents the linear relationship between the volatility of the residual and the lagged average of aggregate investment.

Following BCE, I assume that the underlying process is stationary, because the investment rate is a finite ratio by definition. If the underlying process is nonstationary, estimates of conditional heteroskedasticity (sensitivity, elsewhere) will be biased upwards (see Lamoureux and Lastrapes (1990)). As such, estimates on HP-filtered data are also included to evaluate the importance of stochastic trends in this setting. The estimates of the BCE specification are not sensitive to the choice of estimator. Appendix C.1 presents a replication of the BCE empirical analysis using MLE, as a robustness exercise. For clarity, I use OLS to estimate the BCE specification and MLE to estimate the alternative GARCH estimators in the benchmark analysis.

3.3 Empirical Motivation

To motivate my analysis, I examine the squared residuals of a univariate autoregression on the aggregate investment rate in two steps. First, I estimate the autocorrelations of ε_t^2 and x_t. If these autocorrelation functions exhibit significant differences in persistence, then the use of alternative GARCH specifications will provide more direct estimates of the persistence of the conditional variance of aggregate investment, relative to the BCE specification. Second, I estimate the correlation between ε_t^2 and log gross domestic product y. If the squared residuals are cyclical, then σ_t^2 is likely cyclical. If the squared residuals are acyclical or countercyclical, it is unlikely that σ_t^2 exhibits the same level of cyclicality as the aggregate investment rate.

Figure 3.2 plots the autocorrelation function for the aggregate investment series and the squared residuals. While the aggregate investment rate exhibits high autocorrelation across many lags, the squared residuals of the aggregate investment rate series are not persistent. The difference in the persistence of investment and the squared residuals suggests that alternative specifications may provide improved estimates of the persistence of the conditional volatility of aggregate investment

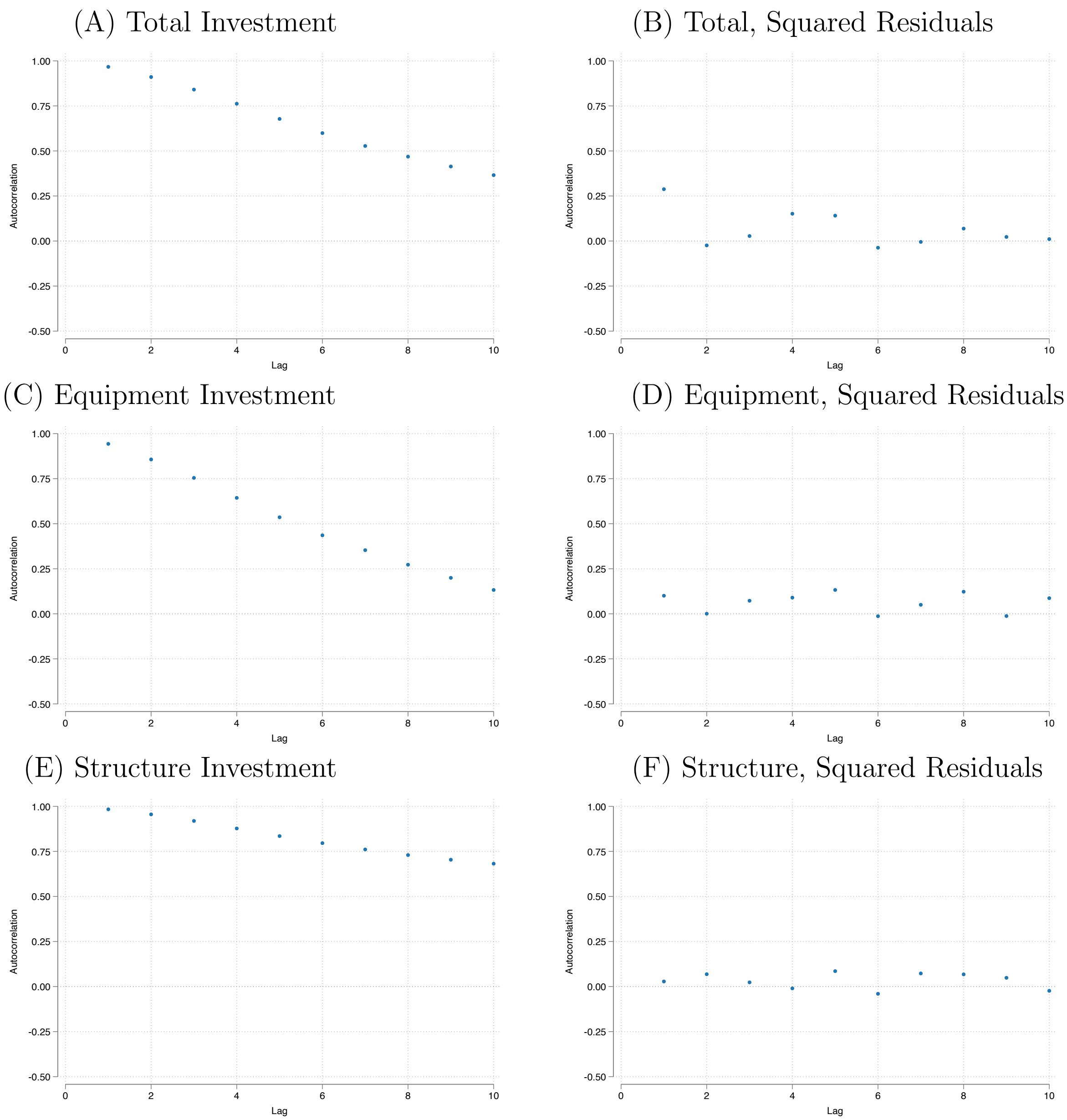
Figure 3.3 plots the correlation between a set of output lags and the squared residuals of equipment, structure, and total investment. The squared residuals of the aggregate investment rate series are not procyclical. Equipment and total investment are not cyclical regardless of the filter choice. Unfiltered structure investment is counter-cyclical. When structure investment is hp-filtered, it is not cyclical. For all series, the cyclicality of lagged investment does not coincide with the cyclicality of the squared residuals. These results motivate the use of alternative GARCH specifications to measure the persistence and cyclicality of aggregate investment rate series.

3.4 Conditional Volatility of Aggregate Investment

In this exercise, I estimate the cyclicality of the conditional volatility of the aggregate US investment rate. Table C.6 reports the estimation results for the GARCH and BCE specifications on unfiltered data. Table C.7 reports the estimation results for the GARCH and BCE specifications on HP-filtered data. Figure 3.4 plots the cyclicality of the alternative GARCH specifications against the BCE specification.

The estimates in Table C.6 and able C.7 illicit three key insights. First, the autoregressive coefficient β_g is insignificant for equipment and total investment. The coefficient for structure investment is positive and significant, suggesting meaningful persistence of the

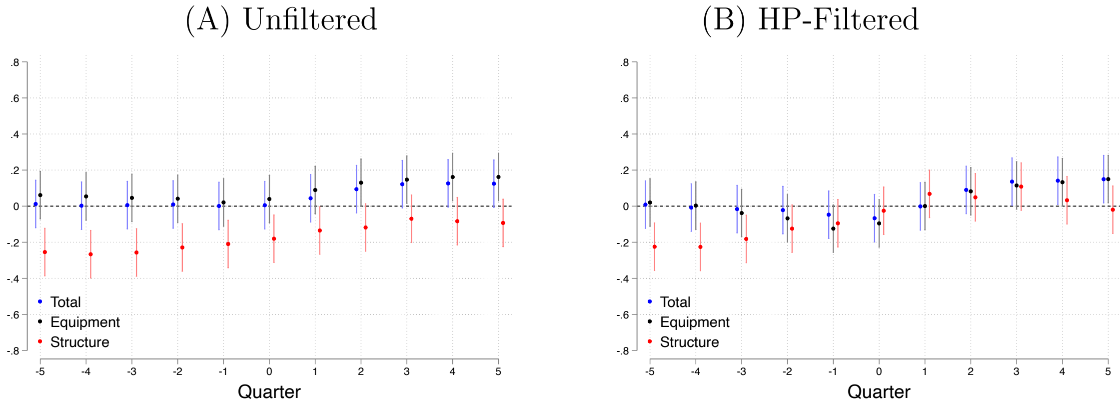
Figure 3.2 – Autocorrelation of Aggregate Investment and Squared Residuals



Notes: This figure features six autocorrelation functions. Panels A, C, E plot the autocorrelation function for U.S. aggregate total investment, U.S. aggregate equipment investment, and U.S. structure investment, respectively. Panels B, D, F plot the autocorrelation function for the squared residuals of univariate autoregressions that maximize the AIC. The autoregression of total and structure investment rate residuals has a lag order of 6. The autoregression of the equipment investment rate residuals has a lag order of 7.
Sources: BEA and author calculations.

conditional variance. Second, the moving average coefficient is significant for total, structure, and equipment investment. Third, the conditional volatility exhibits different business cycle properties, depending on whether lagged aggregate investment is included as an explanatory

Figure 3.3 – Cyclicality of Squared Residuals



Notes: This figure features two subplots. Panel A plots correlograms of different output lags and the squared residuals of unfiltered U.S. aggregate total, equipment, and structure investment. Panel B plots correlograms of different output lags and the squared residuals of HP-filtered U.S. aggregate total, equipment, and structure investment. Total and structure investment rate residuals are recovered from an autoregression with 6 lags. Equipment investment rate residuals are recovered from an autoregression with 7 lags. The unfiltered correlation is estimated on output detrended using a second order deterministic filter. The hp-filtered correlation is estimated on output detrended using an HP-filter with a smoothing parameter of 1600. *Sources:* BEA and author calculations.

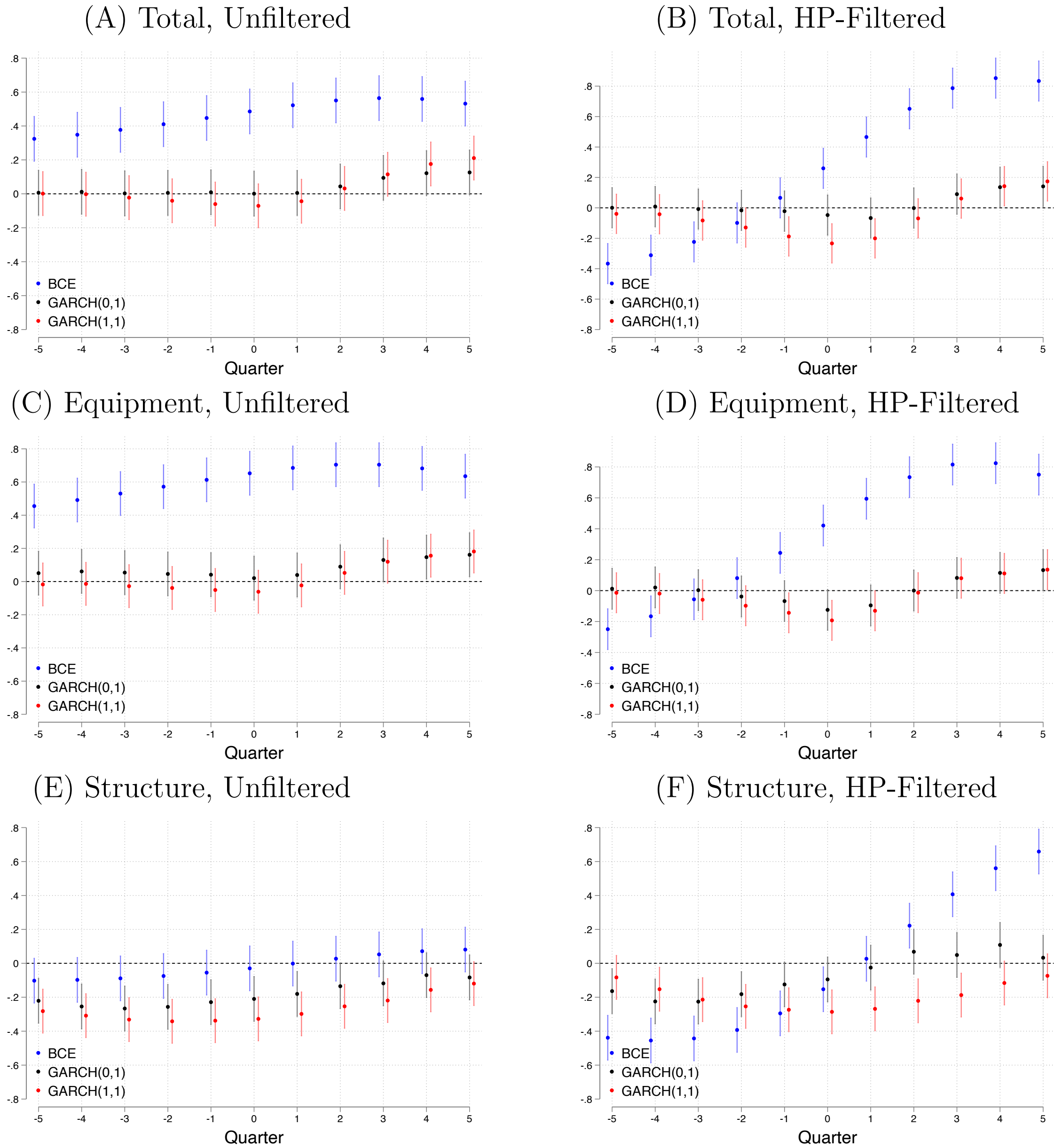
variable.

When estimated using the BCE specification, the conditional volatility exhibits different business cycle properties. In the case of equipment and total investment, the behavior of σ_t^{BCE} is more cyclical and less persistent than the σ_t recovered from the alternative GARCH specifications. In the case of structure investment, the behavior of σ_t^{BCE} is either more or less countercyclical, depending on whether the data is filtered beforehand, respectively. Moreover, σ_t^{BCE} is more persistent than estimates recovered from the alternative specifications for all series, regardless of filter choice.

This exercise also clarifies the inference associated with the BCE specification. The parameter η tests whether the conditional volatility of aggregate investment depends on the lagged average of aggregate investment. That said, the size of the η is not correlated with the cyclicality of the conditional volatility, contrary to the original interpretation of the specification. For example, η^{eq} is larger than η^{st}, but the volatility of equipment investment is less cyclical than the volatility of structure investment.

Figure 3.5 compares the implied conditional volatility of aggregate investment implied by the BCE specification against that of the GARCH(1,1) specification. The condition volatility implied by the BCE specification is strongly persistent and fails to capture the spikes in the standard deviation of residuals. The GARCH(1,1) specification captures the spikiness and limited persistence of the standard deviation of residuals. The BCE specification also implies

Figure 3.4 – Cyclicality of Conditional Volatility



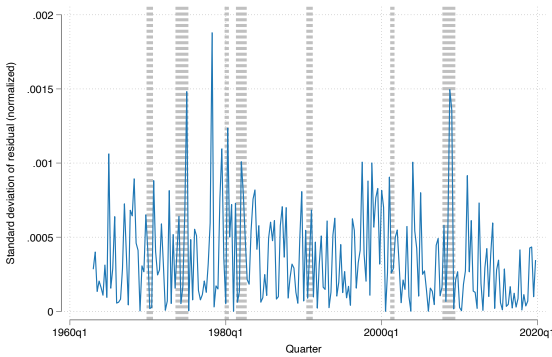
Notes: This plot contains six subplots. Panels A and B plot correlograms of different output lags and the conditional volatility of U.S. aggregate total investment with different specification and filtering choices. Panesl C and D plot correlograms of different output lags and the conditional volatility of U.S. aggregate equipment investment with different specification and filtering choices. Panels E and F plot correlograms of different output lags and the conditional volatility of U.S. aggregate structure investment with different specification and filtering choices. The unfiltered correlation is estimated on output detrended using a second order deterministic filter. The hp-filtered correlation is estimated on output detrended using an HP-filter with a smoothing parameter of 1600.
Sources: BEA and author's calculations.

that the conditional volatility of structure investment is lower in the latter portion of the sample, a feature that is not apparent in the standard deviation of residuals.

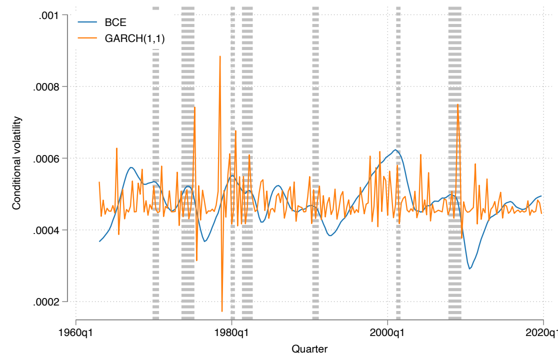
These results, taken together with the results of the previous subsection, suggest that the lagged average of aggregate investment does not measure the persistence and cyclicality of the conditional volatility of aggregate investment. If it were, then one should expect that the time series behavior of σ_t^{BCE} to track the dynamics depicted in Figure 3.3.

Figure 3.5 – Comparison of Conditional Variance

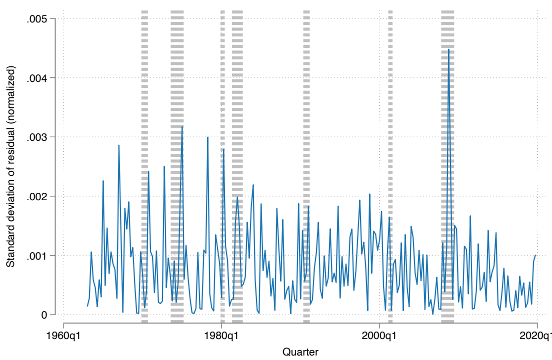
(A) Total, Standard Deviation



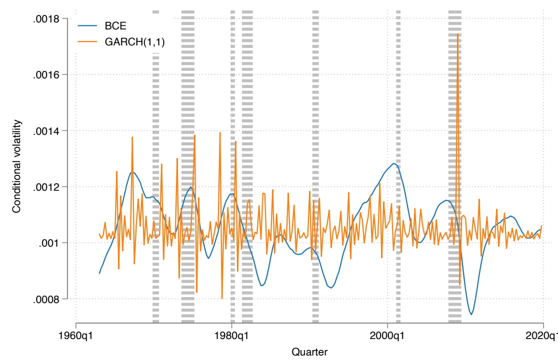
(B) Total, Conditional Volatility



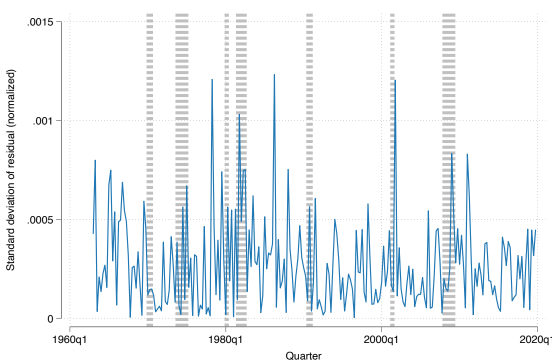
(C) Equipment, Standard Deviation



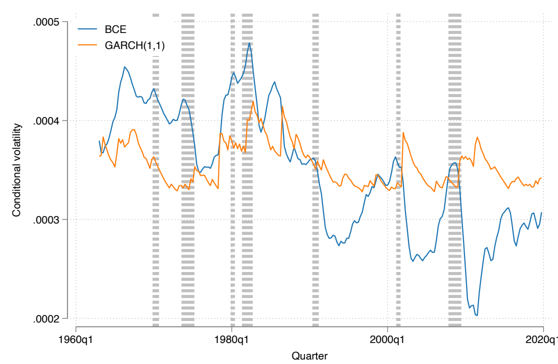
(D) Equipment, Conditional Volatility



(E) Structure, Standard Deviation



(F) Structure, Conditional Volatility



Notes: This plot contains six subplots. Panel A plots the normalized residuals, otherwise referred to as the standard deviation of residuals, from a univariate autoregression of lag order 6 against the lagged average of investment for the U.S. aggregate total investment rate series. Panel C plots the normalized residuals, otherwise referred to as the standard deviation of residuals, from a univariate autoregression of lag order 7 against the lagged average of investment for the U.S. aggregate equipment investment rate series. Panel E plots the normalized residuals, otherwise referred to as the standard deviation of residuals, from a univariate autoregression of lag order 6 against the lagged average of investment for the U.S. Subplots B, D, and F plot the conditional volatility recovered from BCE and GARCH(1,1) specfications for U.S. aggregate total investment rate series, U.S. aggregate equipment investment rate series, U.S. aggregate structure investment rate series, respectively.aggregate structure investment rate series.
Sources: BEA and author calculations.

3.5 Discussion

In this section, I discuss why the two families of estimators produce different estimates of the conditional volatility. First, I show that the BCE estimator is isomorphic to an asymmetric GARCH(∞) estimator. Using this insight, I then show that BCE's measure of conditional volatility's state dependence η is not a direct measure of cyclicality and is sensitive to outliers. As a result, BCE's findings result from the interaction between the sensitivity of their filter and the investment volatility spikes in the data that their models do not predict.

The relationship between these two families of GARCH estimators is not immediately apparent. To provide intuition for my analysis, I use Wold's theory to show that the BCE specification is actually a type of asymmetric GARCH(∞) estimator:

$$
\begin{aligned}
\left(\sigma_t^{BCE}\right)^2 &= \alpha + \eta \bar{x}_{t-1}^k, \\
\left(\sigma_t^{BCE}\right)^2 &= \alpha + \eta \left(\sum_{j=1}^{k-1} \left(\frac{\sum_{i=1}^{j} b_{j-i}}{k} \right) \varepsilon_{t-j} + \sum_{j=k}^{\infty} \left(\frac{\sum_{i=1}^{k} b_{j-i}}{k} \right) \varepsilon_{t-j} \right), \\
\left(\sigma_t^{BCE}\right)^2 &= \alpha + \eta \sum_{j=1}^{\infty} \left(\frac{d_j}{k} \right) \varepsilon_{t-j}, \ d_j = \sum_{i=1}^{\min\{j,k\}} b_{j-i},
\end{aligned}
\tag{3.5.1}
$$

where b_j is the moving average weight of a residual at lag i in equation 3.2.1 (see Engle (1990) for initial discussion of asymmetric GARCH). Posing the BCE specification in this form yields two insights. First, the weight of each residual does not decrease monotonically, if the lag order of the moving average (k) is greater than 1. This is the case for all specifications included in BCE's empirical analysis. Rather, the weight of a residual increases between the first lag and the k^{th} lag before decreasing. The BCE specification exhibits higher persistence by construction, relative to the alternative specifications. Moreover, the BCE specifications that maximize the AIC place equal weight on residuals in the recent and distant past. To demonstrate this feature of the specification, I use an AR(1) to estimate the residuals of aggregate investment. This specification provides an intuitive closed-form representation of the BCE specification.

$$x_t = \rho x_{t-1} + \varepsilon_t, \tag{3.5.2}$$

$$\varepsilon_t = \sigma_t e_t, \tag{3.5.3}$$

$$\left(\sigma_t^{BCE}\right)^2 = \alpha + \eta \sum_{j=1}^{\infty} \left(\frac{d_j}{k}\right) \varepsilon_{t-j},\ d_j = \sum_{i=1}^{\min\{j,k\}} \rho^{j-i}, \tag{3.5.4}$$

Figure 3.6 plots the residual weights of the following specifications. Outliers may lead to spurious asymmetries even in large samples in asymmetric GARCH models Carnero and Prez (2021), while outliers may lead to downward bias in the coefficients in symmetric GARCH models Kim and Meddahi (2020). Due to the size of their filter, the BCE specification places more weight on residuals from ten years ago than the residual from last quarter.

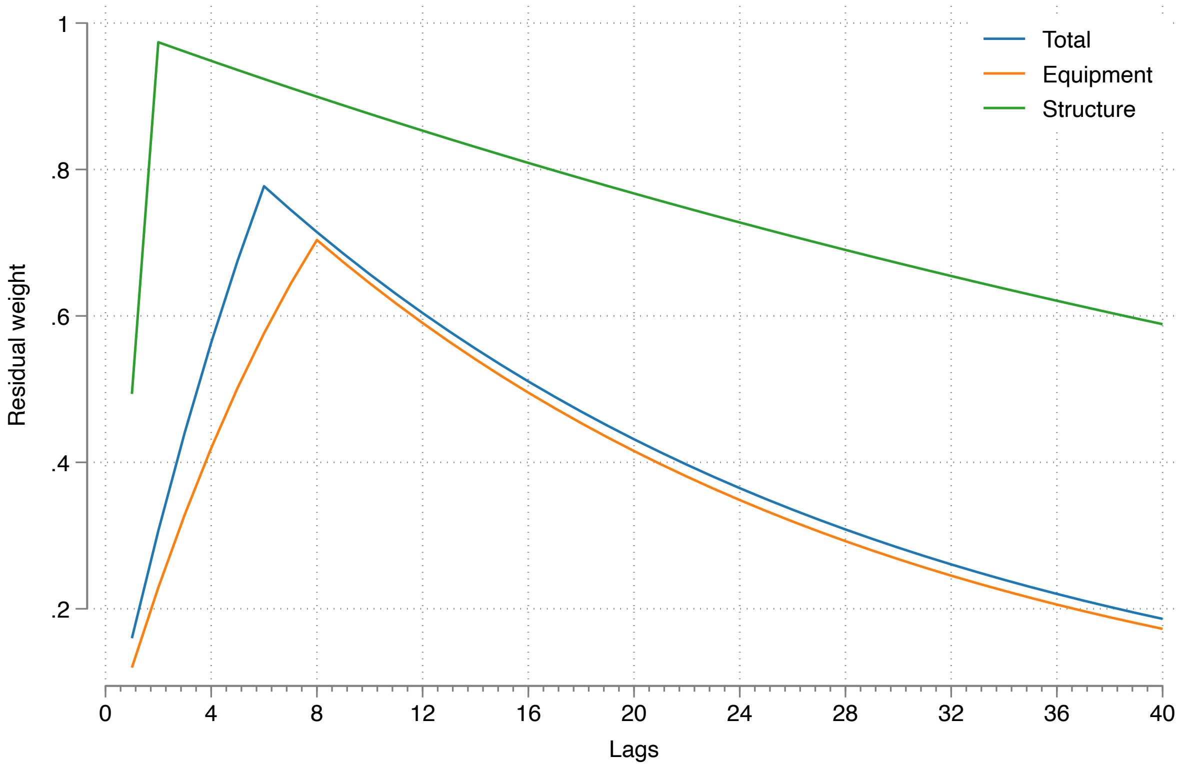
This features clarifies the economic intuition and statistical inference of the BCE specification. While the BCE specification tests for whether periods of heightened investment predict periods of increased volatility of investment, these periods do not coincide with the business cycle. The economic expansions of the 1990s and 2010s are the longest in the sample with durations of approximately 10 years. As a result, the BCE specification often places significant weight on residuals from previous expansions to predict future volatility. The implicit residual weighting scheme of BCE specifications also assumes that the effect of a single residual on the explanatory variable does not dissipate for multiple years. Figure 3.6 can also be interpreted as a rescaled impulse response of conditional volatility to an exogenous shock to aggregate investment.

This implicit weighting scheme of the BCE specification also explains why η is not proportional to the cyclicality of the conditional volatility measured by the BCE specification. Different values of η do not change the persistence or shape of the impulse responses for an arbitrary shock to an economy at baseline. These features are determined by the selection of the lag order of the lagged average and the serial correlation of aggregate investment.

The functional form of the BCE estimator also increases the weight of large investment residuals on the conditional volatility of aggregate investment. It follows that their result may depend on a small number of influential observations. To measure the influence of each observation, I estimate the DFTBETA for each observation in the time series. The DFBETA measures the change in the coefficient caused by removing a certain observation, scaled by the standard deviation of the point estimate in the regression on the restricted sample. Outliers are considered sufficiently influential if they change the point estimate by more than $2/\sqrt(N)$ Belsley et al. (1980).

Figure 3.7 plots the DFBETA for total investment and its components. This analysis

Figure 3.6 – Weight of Residuals in BCE Specifications

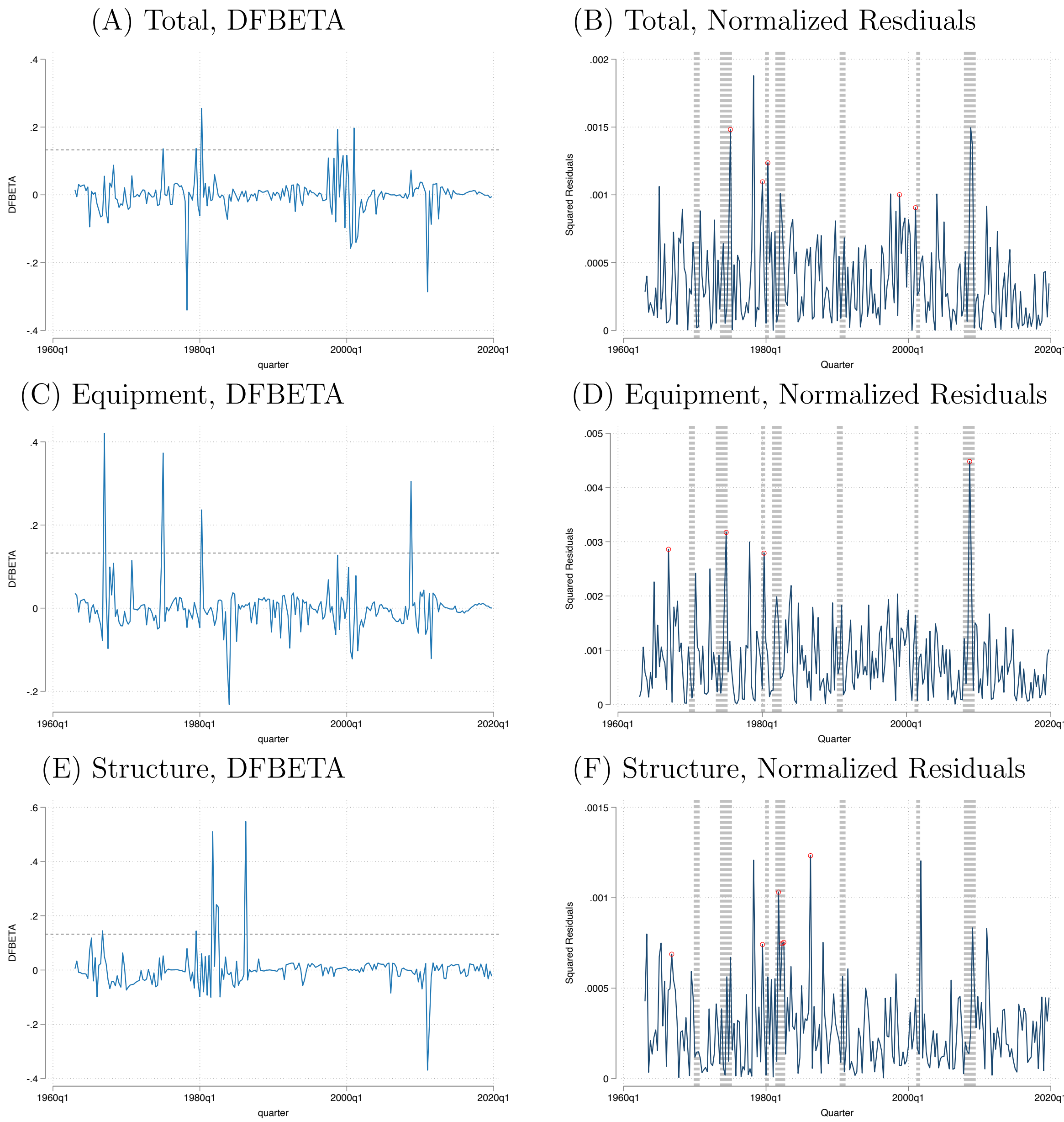


Notes: This figure plots the implicit residual weights for the asymmetric GARCH(∞) representation of the BCE specifications. The weights correspond to a univariate autoregression with lag order of 1 for U.S. aggregate total investment, U.S. aggregate structure investment, and U.S. equipment investment. The lag order of lagged aggregate investment for the BCE specifications is 6 for total and equipment investment and 2 for structure investment.
Sources: BEA and author calculations.

shows that the empirical results of BCE are sensitive to a small number of observations. These observations are the volatility spikes in the sample that the BCE estimator does not predict. These spikes typically occur in and around recessions. Figure 3.8 plots the standard deviation of residuals against the lagged average of investment. It shows a few influential observations drive the empirical analysis in BCE. When those observations are excluded from the sample, there is no longer a significant relationship between the conditional volatility of aggregate investment and lagged average investment.

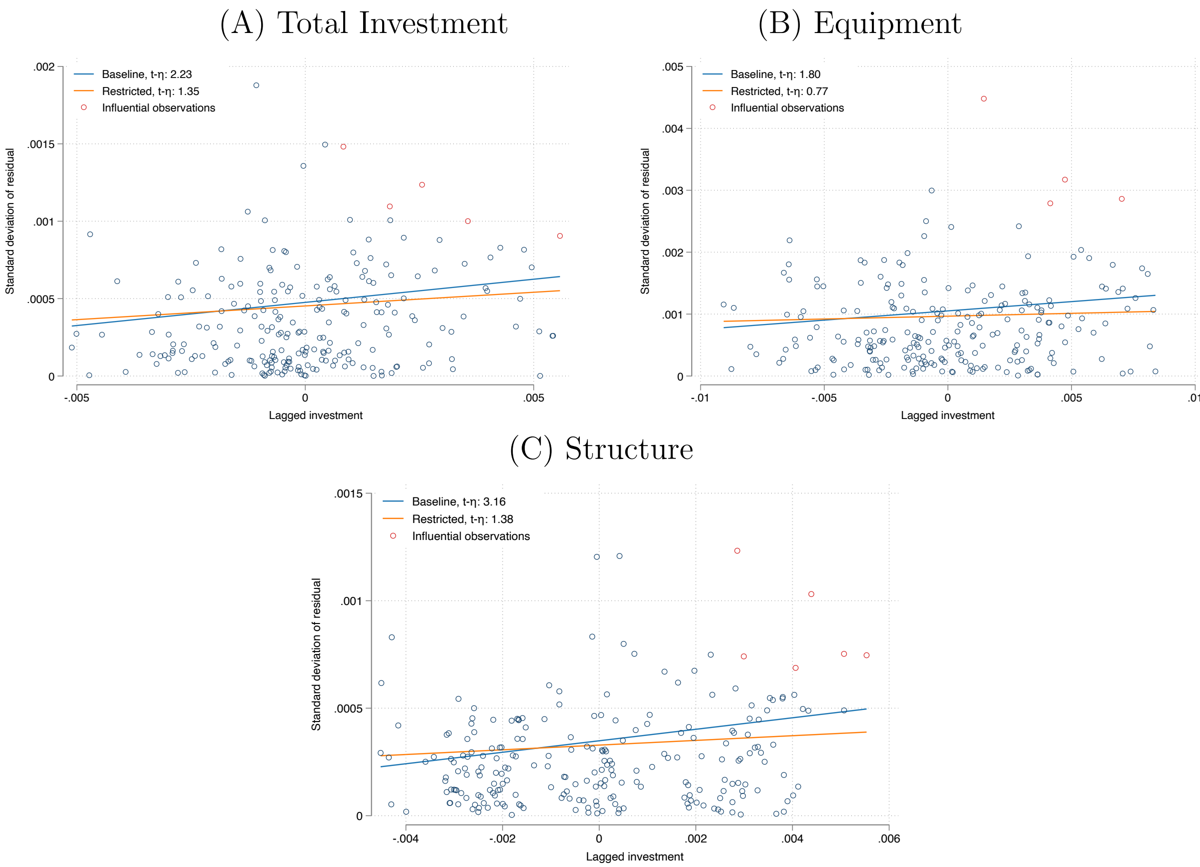
The alternative GARCH specifications are not equivalent to the BCE specification. Reposing the BCE specification as an asymmetric GARCH(∞) model shows that the two families of specifications use the information from residuals in distinct ways. Thus, it is unlikely that the two specifications provide similar estimates of the conditional volatility

Figure 3.7 – Influential Observations in BCE Specifications



Notes: This plot contains six subplots. Subplots A, C, and E plot the DFBETA statistic for U.S. aggregate total investment rate series, U.S. aggregate equipment investment rate series, U.S. aggregate structure investment rate series, respectively. Panel B plots the normalized residuals, otherwise referred to as the standard deviation of residuals, from a univariate autoregression of lag order 6 against the lagged average of investment for the U.S. aggregate total investment rate series. Panel D plots the normalized residuals, otherwise referred to as the standard deviation of residuals, from a univariate autoregression of lag order 7 against the lagged average of investment for the U.S. aggregate equipment investment rate series. Panel F plots the normalized residuals, otherwise referred to as the standard deviation of residuals, from a univariate autoregression of lag order 6 against the lagged average of investment for the U.S. aggregate structure investment rate series.
Sources: BEA and author calculations.

Figure 3.8 – Effect of Influential Observations on BCE Coefficient



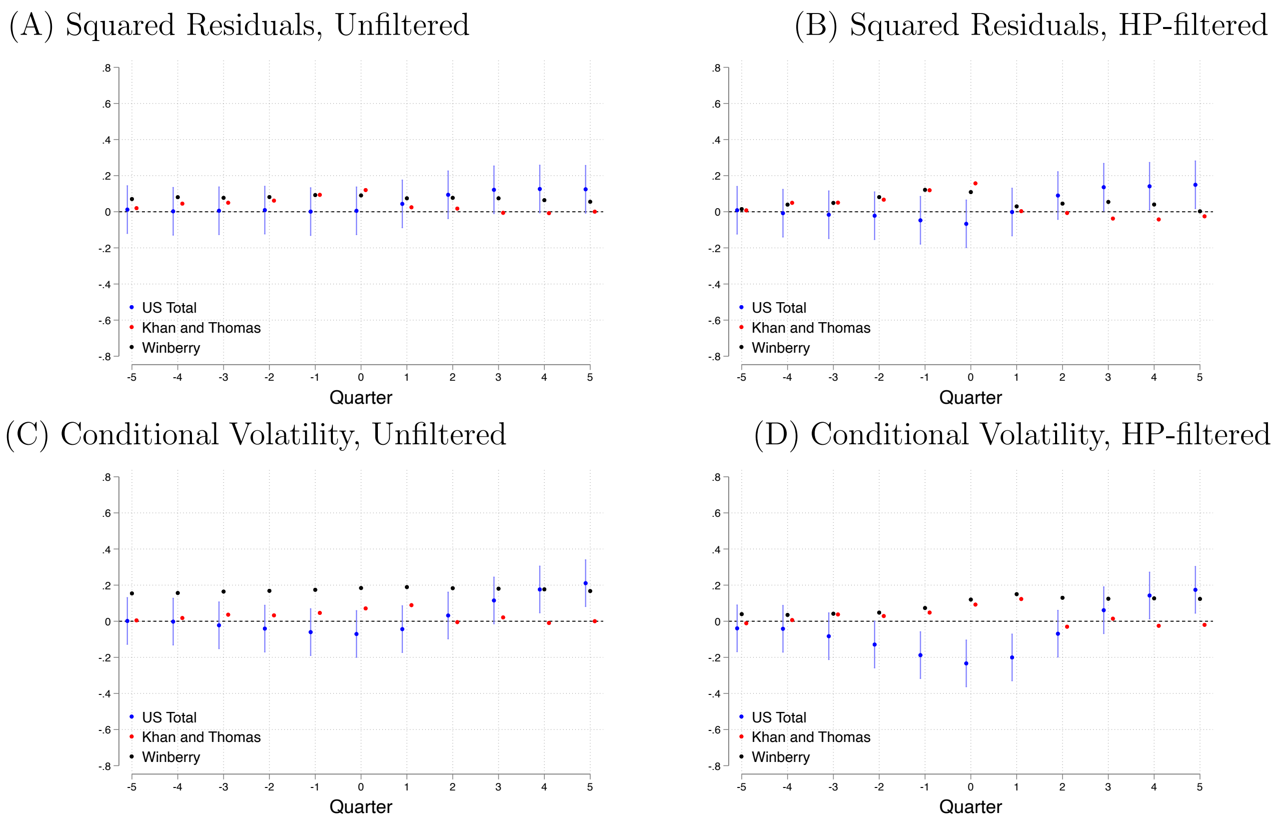
Notes: This plot contains three subplots. Panel A plots the estimation results of the BCE specification against a scatter plot of the standard deviation of residuals from a univariate autoregression of lag order 6 against the lagged average of investment for the U.S. aggregate total investment rate series. Data points with a DFBETA above $2/\sqrt{(N)}$ are highlighted in red, and the legend reports the t-statistic of η for the baseline sample and the restricted sample, which excludes the influential observations. Panel B plots the estimation results of the BCE specification against a scatter plot of the standard deviation of residuals from a univariate autoregression of lag order 7 against the lagged average of investment for the U.S. aggregate equipment investment rate series. Panel C plots the estimation results of the BCE specification against a scatter plot of the standard deviation of residuals from a univariate autoregression of lag order 6 against the lagged average of investment for the U.S. aggregate structure investment rate series.
Sources: BEA and author calculations.

of aggregate investment. That said, one may be interested in whether the data generating processes implied by these estimators are somehow related. Appendix C.2 responds to this question using two Monte Carlo experiments and confirms that the two data generating processes implied by these estimators are not equivalent.

3.6 Conditional Volatility of Simulated Environments

In this section, I estimate the two families of GARCH specifications on simulated data from two benchmark lumpy investment models with varying levels of state dependence: Khan and Thomas (2008) and Winberry (2021).[1] First, I recover the point estimates of the two simulated samples. Next, I compare the point estimates of the simulated data to the confidence intervals of the US data to test against the null that a given quantitative model could have produced the observed US data. Table 3.1 and 3.2 report the regression results. Figure 3.9 plots the cyclicality of the simulated squared residuals and conditional variances.

Figure 3.9 – Cyclicality of Simulated Squared Residuals and Conditional Volatilities



Notes: Panels A and B report correlograms of different output lags and the squared residuals recovered from autoregressive processes with a lag order of 6 estimated on U.S. aggregate total investment data and data from simulated Khan and Thomas (2008) and Winberry (2021) models. Panels C and D report correlograms of different output lags and the conditional variances estimated using a GARCH(1,1) specification.The unfiltered correlation is estimated on output detrended using a second order deterministic filter. The hp-filtered correlation is estimated on output detrended using an HP-filter with a smoothing parameter of 1600. *Sources:* BEA and author calculations.

[1]I use the codes provided by Winberry (2021) and Winberry (2018) to run the simulations for consistency with other studies.

Table 3.1 – Regression Results, BCE Specification, Simulated Data

Series	Filter	$\hat{\eta}$	SE
Khan and Thomas	HP	-.00012	.00021
Winberry	HP	.00008	.00004
Khan and Thomas	NF	.00006	.00013
Winberry	NF	.00007	.00002

Notes: This table reports conditional variance regression results using the BCE specification on simulated data from Khan and Thomas (2008) and Winberry (2021), using OLS. The first column denotes the model that generates the underlying data. The second column denotes the filter used prior to estimation. NF implies that the data is not filtered prior to estimation. HP denotes that the data is filtered using an HP-filter with a smoothing parameter of 1600 prior to estimation. The third column denotes the point estimate of η, which measures the relationship between the conditional volatility and the lagged average of aggregate investment.
Sources: Winberry (2021), Winberry (2018), and author calculations.

I fail to reject the null that cyclicality of squared residuals from both Khan and Thomas (2008) and Winberry (2021) models are statistically different from the residuals recovered from aggregate US investment rates. This is relevant because the two classes of models produce significantly different aggregate dynamics, most notably their interest rate elasticity discussed in House (2014) and Winberry (2021), among others. Second, both models exhibit procyclical conditional variances. Third, the simulated series both fail to capture the relative size of the moving average component relative to the autoregressive component. In models with high state dependence akin to Winberry (2021), the conditional variance exhibits significant mean reversion and persistence, as the β_g coefficient is large, while undershooting the importance of recent innovations, as the β_a coefficient is statistically insignificant. Unsurprisingly, models with low state dependence like Khan and Thomas (2008) exhibit insignificant persistence, as β_g and β_a are statistically insignificant at the 5% level for all specifications.

Table 3.2 – GARCH Results, Simulated Data

Series	Filter	Specification	β_a	SE_a	β_g	SE_g
K & T	NF	GARCH(0,1)	.0331634	.0338382		
K & T	NF	GARCH(1,1)	.032849	.0335836	-.3376483	.4932293
K & T	HP	GARCH(0,1)	.065429	.0374582		
K & T	HP	GARCH(1,1)	.0688493	.0383658	-.299037	.3152482
Winberry	NF	GARCH(0,1)	.0110846	.0357373		
Winberry	NF	GARCH(1,1)	.0305299	.0276434	.7427631	.2682626
Winberry	HP	GARCH(0,1)	.0098734	.0360306		
Winberry	HP	GARCH(1,1)	.0368468	.0307327	.7355672	.2557767

Notes: This table reports conditional variance regression results using the GARCH specifications on simulated data from Khan and Thomas (2008) and Winberry (2021). The first column denotes the model that generates the underlying data. The second column denotes the filter used prior to estimation. NF implies that the data is not filtered prior to estimation. The third column reports the point estimate of β_a, the coefficient on the moving average component. The fourth column reports the standard error of β_a. The fifth column reports the point estimate of β_g, the coefficient on the autoregressive component. The sixth column reports the standard error of β_g. HP denotes that the data is filtered using an HP-filter with a smoothing parameter of 1600 prior to estimation.
Sources: Winberry (2021), Winberry (2018), and author calculations.

3.7 Conclusion

The presence of conditional heteroskedasticity neither implies cyclical nor persistent heteroskedasticity. The conditional volatility of aggregate total and equipment investment is acyclical and impersistent, while the sensitivity of structure investment is countercyclical and persistent. The cyclicality of the investment heteroskedasticity documented in BCE mechanically follows from the cyclicality of the lagged investment.

Since the cyclicality of aggregate investment varies across capital types, future quantitative work should consider possible explanations for why the conditional volatility of aggregate structure investment is more persistent than that of aggregate equipment investment. Possible work should consider the interaction of maintenance investment with fixed costs and other nonlinear investment frictions. For example, partial irreversibility, whether due to adverse selection or adjustment costs, creates a wedge between the buying price and selling price of different capital goods. As equipment tends to have a thicker resale market, the importance of maintenance investment may vary across capital types. Alternatively, equipment is also more tradable than structures, so the price elasticity of equipment supply is more elastic. This feature, outlined in numerous studies, could explain why the conditional volatility of investment varies across capital goods.

APPENDICES

APPENDIX A

Appendix to Chapter 1

A.1 Construction of Linked Panel

In this appendix, I discuss the construction of the linked union and firm panel. I begin with a discussion of the data sources that I use to construct the crosswalk between firms and local unions. I then discuss the construction of the crosswalk.

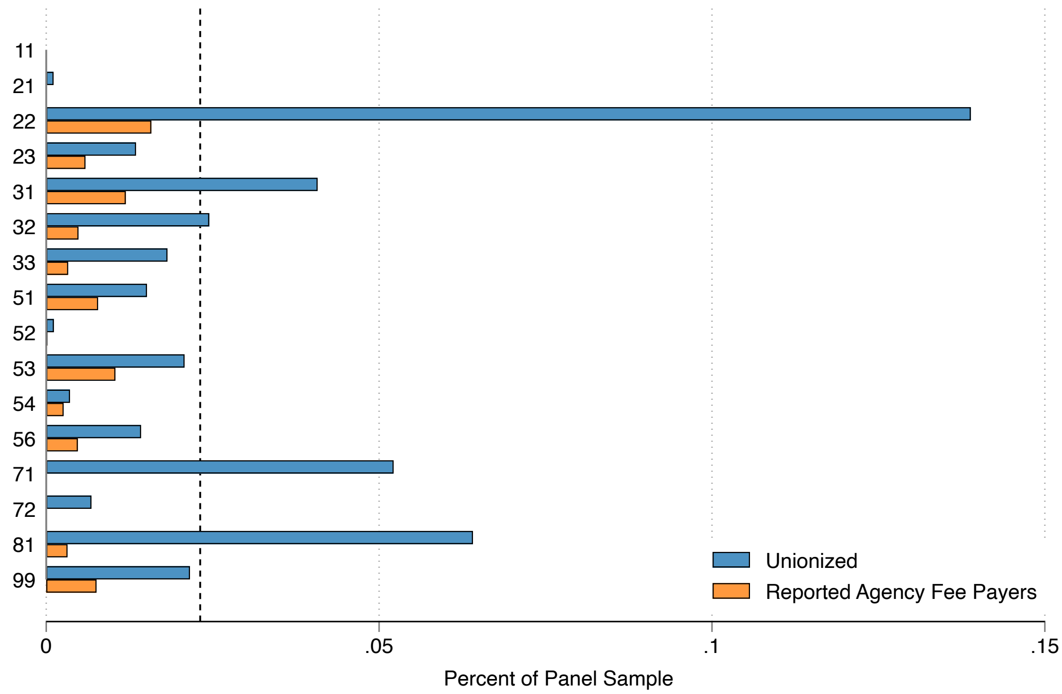
When a firm or union intends to engage in collective bargaining, whether they intended to bargain for a new contract or renegotiate a current contract, they must file a notice to FMCS of upcoming collective bargaining (F7). As such, these administrative data provide the universe of collective bargaining activities for a given period. The sample of F7 filings spans from 2015 to 2022, and the sample of DOL collective bargaining agreements data set (hereafter, CBA) spans agreements with expiration dates from 1969 to 2029. These data sets were accessed electronically on December 21, 2022.

First, I use the fuzzy matching algorithm of Raffo and Lhuillery (2009) to construct a crosswalk between firms in Compustat and their collective bargaining records of FCMS and DOL. Before I apply the fuzzy matching algorithm, I construct stems for each firm name in each dataset. The process of stemming text strings is standard data cleaning process used in text analysis, which reduces strings to a string of stems. Stems are not necessarily the morphological root of each word, but often provide sufficient information on the meaning of each word. Fuzzy matching without stemming increases the false positive rate of the algorithm, as derivations of different words may share a large number of characters. Company names in Compustat often share entire words that do not uniquely identify the company (e.g. industries, ventures, international), so stemming is necessary to recover accurate matches. For this process, I remove all spaces, articles, conjunctions, and symbols from firm names. I also remove all firm name suffixes from Compustat names that represent the type of non-trading firm a record represents. I also remove the following words that do not uniquely identify firms: company, industries, ventures, technology, restaurant, technologies, corp, communications, energy, services, construction, enterprises, casino, holdings, pharmaceuticals. I record all firm name matches with above an 85% match rate. This process creates two crosswalks: the Compustat-F7 crosswalk and the Compustat-CBA crosswalk. Of the 2981 unique employer names listed in the DOL collective bargaining agreement sample, this approach identified unique 118 Compustat firm matches. For the FCMS sample, this approach identified 335 Compustat firms of 25593 unique employer names. I then combine these two crosswalks into one Compustat-union crosswalk that uniquely identifies 436 Compustat firms with exposure to unions

Second, I match LM-2 filings to these two crosswalks using union affiliate abbreviation and local number. Local numbers are unique identifiers assigned to local unions within affiliate unions during the local's recognition process. I verify each match by hand. This crosswalk identifies the link between 322 Compustat firms and 737 local unions. All local unions matched to the crosswalk uniquely identify firms. I exclude 114 Compustat firms that were listed on FCMS or DOL filings that did not match with LM-2 filings. This match rate could be the result of firms negotiating with local unions with less than $250,000 dollars in total receipts, intermediate labor organizations, or national labor organizations. As these data do not provide information on the firms' exposure to local unions revenue and membership, I exclude them from the analysis. Table A.1 reports the summary statistics for Compustat firms matched with local unions in the sample. Most Compustat firms only match with one local union. The firms that match with multiple unions include the Big 3 automobile manufacturers, steel manufacturers, and aerospace engineering firms. Table 1.1 reports the summary statistics of all firms in the matched sample. Table A.2 reports the summary statistics of all unions in the sample. Table A.3 reports the summary statistics of unions with and without agency fee payers in this sample. Figure A.1 plots the share of firms in each industry that is unionized. The distribution of unionized firms across industries conforms to expectations, with the largest shares in manufacturing and mining. The relative financial health and spending patterns of unions with and without agency fee payers persists in this sample. Unions with agency fee payers are larger, reporting greater income, assets, and members. They also spend more on representational services, political action, and general overhead.

While this data set does not capture the universe of local union and firm linkages, it provides a useful sample for analyzing the relationship between local union balance sheets

Figure A.1 – Industry Share of Unionized Firms in Matched Sample



Notes: This figure plots industry share of unionized firms in matched LM-2 and Compustat sample. The dotted line plots the sample average. Sample ranges from 2000-2019.
Sources: DOL, FCMS, Compustat and author's calculations.

and firm income. As this data set likely does not include all local union-firm pairs, one should expect that the estimates using this data set may exhibit bias.

Table A.1 – Local Unions Matched to Compustat Firms

	Mean	Std. Dev.	5^{th}	10^{th}	Median	90^{th}	95^{th}
Local Unions	2.289	3.575	1	1	1	5	6
Observations	322						

Notes: This table reports the count of local unions with annual receipts above $250,000 that matched with each Compustat firm. The sample spans from 2000 to 2019.

A.2 Membership Trends Across States

In this appendix, I present additional information on trends in agency fee payers across states in the sample period. The share of unions with reported agency fee payers and the

Table A.2 – Unions in Matched Panel

	Mean	Std. Dev.	10^{th}	25^{th}	Median	75^{th}	90^{th}
Receipts	361.00	744.84	32.94	58.39	126.28	322.60	854.31
Disbursements	354.23	744.71	32.66	57.19	124.06	320.02	833.41
Assets	363.35	827.79	18.18	41.81	113.75	315.61	784.49
Liabilities	37.58	169.02	0.00	0.05	2.42	14.56	67.47
Net Worth	320.28	699.56	13.63	36.43	100.86	290.15	717.08
Members	3609.48	6144.54	435.00	804.00	1564.00	3600.00	7743.00
AFP Rate	0.63	2.26	0.00	0.00	0.00	0.02	1.25
Rec/Members	0.15	0.69	0.04	0.05	0.08	0.12	0.22
Representation	84.70	159.35	5.07	13.62	31.56	84.76	212.39
Political	4.82	16.18	0.00	0.00	0.46	2.46	10.09
Overhead	43.51	103.83	1.31	5.17	14.28	35.41	94.85
Observations	6043						

Notes: This table reports summary statistics for unions in the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports union financial and membership data at the union level.
Sources: DOL, FCMS, Compustat and author's calculations.

ratio of agency fee payers to members summarize the uptake of agency fee payment in unions. That said, one may be interested in general trends in members and agency fee payers across certain cross-sections. The results of this section provide additional support for the findings in the main paper.

Table A.4 reports the average membership level and standard error of the average outside of the treatment groups. This table shows that the average union in the sample experienced an increase in membership and decrease in agency fee payers in states which allow agency shop clauses over the sample period. In states with RTW policies in effect, the size of unions varied insignificantly, while the number of agency fee payers increased slightly. Table A.5 reports the membership trends in treatment groups in the years preceding and following RTW. The average union in the treatment groups did not experience a significant variation in membership after RTW, while agency fee payers fell significantly in all states, at least temporarily. Now, as this table reports the average across all unions in a specific cross-section, it does not consider the trends specific to unions that report agency fee payers.

Table A.6 reports the membership trends in unions with reported with agency fee payers outside of the treatment groups. This table shows that the average union with reported agency fee payers in the sample experienced an insignificant increase in membership and

Table A.3 – Unions with and without Agency Fee Payers in Matched Panel

	Reported Agency Fee Payers			No Agency Fee Payers		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Receipts	516.60	888.41	235.65	304.68	676.86	101.71
Disbursements	509.41	893.19	232.46	298.06	674.43	99.68
Assets	468.85	1012.36	133.48	325.17	746.33	105.91
Liabilities	61.44	207.01	4.70	28.94	152.07	1.98
Net Worth	390.58	832.42	109.22	294.84	642.97	98.48
Members	5896.56	7758.10	3157.00	2781.66	5202.71	1310.00
Rec/Members	0.09	0.06	0.07	0.17	0.80	0.08
Representation	140.68	210.20	66.55	64.43	130.58	24.89
Political	8.47	22.56	1.12	3.50	12.88	0.31
Overhead	65.26	135.72	22.13	35.64	88.24	11.92
Observations:	1606			4437		

Notes: This table reports summary statistics for local unions without and without agency fee payers in the baseline LM-2 sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2007-2019.
Sources: DOL, FCMS, and author's calculations.

decrease in agency fee payers in states which allow agency shop clauses over the sample period. In states with RTW policies in effect, unions with agency fee payers experience an insignificant increase in membership while the number of agency fee payers significantly. The increase in agency fee payers is either the result of voluntary agency fee payers or misreporting. An increase in voluntary agency fee payers suggests a lack of organizing capacity at the union level or a shift in workers preferences for union representation. Table A.7 reports the membership trends in unions with reported with agency fee payers in treatment groups in the years preceding and following RTW. The average union in the treatment groups experience a decrease in members and agency fee payers in all states, at least temporarily, although it is statistically insignificant. In general, changes in average membership across unions does not capture changes in average union's bargaining power, as the membership rate varies across unions and across firms.

Table A.4 – Trends in Membership Outside of Treatment Groups

	Members			AFP		
Year	Total	RTW Never	RTW Pre-2007	Total	RTW Never	RTW Pre-2007
2007	2503.5	2806.9	1572.4	95.0	123.2	8.5
	(122.2)	(158.2)	(102.4)	(24.8)	(32.9)	(2.8)
2008	2560.8	2884.3	1612.4	80.0	104.0	9.5
	(130.1)	(170.3)	(105.3)	(19.5)	(26.1)	(3.4)
2009	2549.2	2877.2	1582.2	113.7	149.6	8.0
	(129.6)	(169.4)	(105.4)	(36.7)	(49.2)	(2.2)
2010	2640.2	2951.8	1659.5	144.4	186.3	12.5
	(145.3)	(186.2)	(134.0)	(42.4)	(55.9)	(3.4)
2011	2463.1	2781.6	1516.0	133.5	168.3	29.7
	(126.5)	(164.7)	(106.3)	(37.3)	(49.5)	(16.8)
2012	2479.4	2795.6	1531.7	127.7	167.3	9.1
	(129.4)	(168.2)	(108.4)	(36.2)	(48.2)	(2.3)
2013	2508.5	2832.3	1553.9	132.7	171.5	18.3
	(131.1)	(170.6)	(115.3)	(33.9)	(45.3)	(7.4)
2014	2568.3	2898.6	1583.2	74.7	96.4	9.9
	(136.9)	(178.4)	(112.6)	(13.9)	(18.6)	(2.6)
2015	2544.3	2874.5	1578.3	69.3	89.3	10.7
	(129.2)	(168.6)	(111.1)	(11.9)	(15.9)	(2.9)
2016	2609.7	2971.5	1553.0	73.9	93.6	16.5
	(136.9)	(179.6)	(104.5)	(12.1)	(16.1)	(4.8)
2017	2608.8	2955.6	1606.2	73.4	91.0	22.7
	(134.5)	(176.8)	(104.1)	(12.1)	(16.0)	(8.1)
2018	2664.0	3056.4	1571.5	34.9	40.8	18.6
	(138.4)	(183.8)	(102.8)	(6.1)	(7.9)	(7.0)
2019	2675.7	3042.2	1627.9	26.8	29.6	18.9
	(138.0)	(181.9)	(106.6)	(4.1)	(5.0)	(6.6)

Notes: This table reports means and their standard errors in agency fee payers in local unions located in states which were unaffected by RTW reforms from 2007-2019.
Sources: DOL

Table A.5 – Trends in Membership Relative to the Enactment of RTW

	Members						AFP					
h	Total	MI	IN	WI	WV	KY	Total	MI	IN	WI	WV	KY
-5	1843.5	2238.8	1430.7	2048.3	778.9	1894.1	7.1	9.3	2.7	6.3	9.4	9.9
	(168.7)	(359.2)	(173.7)	(315.2)	(124.5)	(514.9)	(2.5)	(5.7)	(1.2)	(3.2)	(9.0)	(5.9)
-4	1824.7	2178.9	1507.0	1844.8	863.1	1952.8	7.4	7.8	4.5	10.7	8.1	7.3
	(175.7)	(380.8)	(193.2)	(273.3)	(130.7)	(520.2)	(2.4)	(5.0)	(2.1)	(6.4)	(7.8)	(5.3)
-3	1803.9	2188.0	1471.3	1796.7	765.4	1980.0	9.1	10.5	4.1	7.7	4.3	20.3
	(187.8)	(425.6)	(195.8)	(264.3)	(130.1)	(577.5)	(3.0)	(5.6)	(2.0)	(5.3)	(4.3)	(15.1)
-2	1829.4	2164.7	1487.8	1785.7	751.6	2161.5	7.0	9.4	4.0	9.6	1.3	4.9
	(185.9)	(386.1)	(224.2)	(248.5)	(116.8)	(632.1)	(2.3)	(4.8)	(2.2)	(6.1)	(1.3)	(4.7)
-1	1809.9	2155.3	1485.9	1782.1	728.6	2166.7	9.6	14.7	9.7	3.9	0.8	5.9
	(184.6)	(391.8)	(207.3)	(265.9)	(111.8)	(636.7)	(3.4)	(7.9)	(5.1)	(1.5)	(0.8)	(3.8)
0	1747.9	1995.1	1500.5	1707.7	726.2	2334.6	13.0	8.7	15.4	31.7	0.8	4.7
	(146.1)	(276.2)	(202.7)	(256.2)	(108.6)	(665.0)	(4.8)	(3.2)	(8.9)	(25.5)	(0.8)	(3.0)
1	1747.9	2021.3	1508.4	1737.7	760.8	2103.8	6.0	2.4	11.1	13.0	0.0	1.0
	(143.7)	(276.4)	(206.9)	(247.3)	(112.5)	(612.5)	(2.1)	(0.9)	(6.8)	(7.5)	(0.0)	(0.5)
2	1701.7	1932.9	1497.3	1743.5	819.5	1928.1	3.1	2.1	6.5	4.1	0.0	0.9
	(137.1)	(256.2)	(207.1)	(258.5)	(138.6)	(533.4)	(0.9)	(0.9)	(3.0)	(1.6)	(0.0)	(0.6)
3	1716.6	1994.0	1534.5	1777.5	788.0		6.6	9.9	5.7	2.9	0.0	
	(141.8)	(267.4)	(213.5)	(245.3)	(147.7)		(3.4)	(7.2)	(3.8)	(1.0)	(0.0)	
4	1821.9	2054.2	1500.8	1783.7			4.2	5.1	4.3	2.2		
	(149.7)	(261.4)	(205.6)	(251.1)			(1.6)	(2.6)	(3.3)	(0.8)		
5	1819.3	2037.0	1484.1				5.3	1.8	10.8			
	(175.2)	(253.8)	(210.5)				(2.6)	(0.9)	(6.4)			

Notes: This table reports means and their standard errors in agency fee payers in states affected by RTW reforms from 2007-2019. *Sources:* DOL

Table A.6 – Trends in Membership Outside of Treatment Groups (Reported AFP)

	Members			AFP		
Year	Total	RTW Never	RTW Pre-2007	Total	RTW Never	RTW Pre-2007
2007	6370.2	6714.0	2720.0	587.6	628.8	150.5
	(600.5)	(653.4)	(489.1)	(152.0)	(166.1)	(45.9)
2008	6699.5	7092.0	2765.6	525.8	560.9	174.2
	(670.0)	(732.9)	(482.0)	(126.6)	(139.0)	(58.8)
2009	6753.9	7042.2	3527.3	734.6	785.9	160.2
	(660.4)	(716.4)	(548.7)	(235.7)	(256.6)	(36.2)
2010	7138.0	7325.0	4613.2	886.6	932.4	268.0
	(754.5)	(804.7)	(1222.4)	(258.1)	(277.1)	(57.3)
2011	6145.5	6442.2	2544.7	758.4	774.9	558.1
	(621.6)	(670.5)	(377.6)	(210.3)	(226.3)	(308.7)
2012	6428.5	6685.2	2954.4	721.8	761.3	187.0
	(642.2)	(687.4)	(516.9)	(202.8)	(217.7)	(36.9)
2013	6267.6	6511.9	3188.2	725.5	755.8	344.0
	(623.0)	(670.2)	(487.3)	(183.3)	(197.5)	(130.9)
2014	6130.2	6317.5	3672.9	402.3	418.6	188.5
	(609.9)	(653.8)	(664.5)	(73.7)	(79.2)	(41.1)
2015	6253.9	6433.8	3881.2	373.4	385.9	208.5
	(592.5)	(634.7)	(690.8)	(62.7)	(67.3)	(47.0)
2016	6535.6	6829.1	3197.0	401.5	411.8	283.7
	(603.0)	(653.2)	(471.5)	(64.0)	(69.3)	(72.2)
2017	6663.9	6894.5	3964.9	395.4	395.2	398.4
	(591.1)	(638.1)	(680.9)	(63.4)	(67.9)	(132.5)
2018	6885.3	7169.7	3971.9	203.3	191.8	320.9
	(619.3)	(675.1)	(699.7)	(34.8)	(36.5)	(114.0)
2019	6981.0	7363.3	3645.8	164.9	150.2	293.0
	(652.4)	(721.4)	(655.7)	(24.0)	(24.4)	(94.7)

Notes: This table reports means and their standard errors in agency fee payers in local unions located in states which were unaffected by RTW reforms from 2007-2019.
Sources: DOL

Table A.7 – Trends in Membership Relative to the Enactment of RTW (Reported AFP)

	Members						AFP					
h	Total	MI	IN	WI	WV	KY	Total	MI	IN	WI	WV	KY
-5	4719.8	5543.3	4571.2	4409.4	987.0	3443.6	46.8	58.3	16.9	29.3	163.0	47.0
	(1024.3)	(2090.1)	(1544.7)	(992.1)	(460.0)	(2150.4)	(18.8)	(38.7)	(9.8)	(13.9)	(162.0)	(38.8)
-4	4794.6	5815.0	4788.4	4023.4	1004.0	3492.6	50.6	52.1	27.7	50.1	148.0	50.6
	(1087.7)	(2245.3)	(1612.7)	(853.7)	(499.0)	(2208.5)	(17.9)	(33.8)	(13.4)	(32.2)	(147.0)	(37.8)
-3	4948.9	6172.4	4571.0	3955.1	940.5	4135.3	49.9	66.3	22.4	38.1	71.5	47.7
	(1147.3)	(2432.0)	(1590.2)	(839.1)	(471.5)	(2629.8)	(17.3)	(35.0)	(11.4)	(27.2)	(70.5)	(39.4)
-2	4753.0	5649.3	5109.8	3619.4	893.5	4215.2	45.8	50.6	44.4	42.9	24.5	40.0
	(1091.5)	(2136.3)	(1963.3)	(734.9)	(422.5)	(2722.7)	(16.3)	(29.3)	(24.3)	(29.6)	(23.5)	(38.6)
-1	4613.1	5840.9	4384.2	3554.9	874.0	3572.6	66.2	92.0	95.8	16.1	15.5	41.9
	(1042.2)	(2208.4)	(1537.8)	(730.8)	(404.0)	(2277.2)	(23.5)	(49.9)	(45.5)	(5.7)	(14.5)	(24.1)
0	3710.3	4052.2	4331.8	3141.8	844.0	3517.0	87.1	51.8	158.0	132.5	15.5	32.0
	(624.2)	(1144.8)	(1507.3)	(658.4)	(402.0)	(2232.0)	(31.2)	(17.2)	(83.2)	(105.1)	(14.5)	(18.7)
1	3863.5	4231.6	4314.9	3329.6	851.0	3807.2	32.5	13.8	113.0	22.4	0.0	6.8
	(640.2)	(1200.1)	(1510.2)	(649.6)	(406.0)	(2295.0)	(12.3)	(5.2)	(63.6)	(9.3)	(0.0)	(3.8)
2	3835.8	4324.4	4222.7	3155.8	816.0	3715.5	20.7	13.7	51.6	18.3	0.0	7.7
	(649.8)	(1200.6)	(1477.2)	(688.2)	(368.0)	(2483.8)	(5.5)	(5.6)	(23.9)	(6.2)	(0.0)	(4.9)
3	3874.8	4306.1	4714.6	3079.9	826.5		22.5	16.8	63.8	9.6	0.0	
	(660.0)	(1187.3)	(1612.2)	(613.0)	(319.5)		(8.0)	(6.3)	(39.3)	(3.2)	(0.0)	
4	4148.6	4355.1	4589.2	3483.1			18.3	12.5	49.0	8.2		
	(673.2)	(1109.1)	(1563.5)	(718.8)			(7.6)	(5.4)	(36.1)	(3.0)		
5	4329.7	4255.5	4530.0				24.2	9.7	63.4			
	(907.8)	(1104.8)	(1634.2)				(12.0)	(5.2)	(41.2)			

Notes: This table reports means and their standard errors in agency fee payers in states affected by RTW reforms from 2007-2019. *Sources:* DOL

APPENDIX B

Appendix to Chapter 2

B.1 Data Appendix

B.1.1 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.

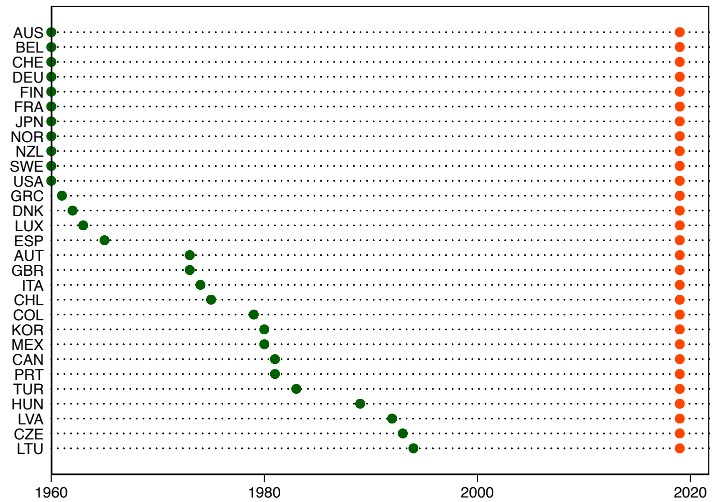
Source Selection There exists more than one candidate series for some countries and variables. At this point, we need to select our sources at the country-level for the following series: real output, policy rate, and unemployment. The source selection process occurs in three steps. First, we assign all 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.

Tax data.

Figure B.1 – Sample Countries and Period



Notes: This figure plots the sample size of each country included in the data set.

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-9052-63EC199F5E63, accessed: February 24, 2022
- All Indexes, Consumer Price Index, IMF, https://data.imf.org/?sk=4FFB52B2-365\3-409A-B471-D47B46D904B53, accessed: February 24, 2022
- All items, World Revenue Longitudinal Data Set, IMF, https://data.imf.org/?sk=77413F1D-1525-450A-A23A-47AEED40FE78, 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.

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

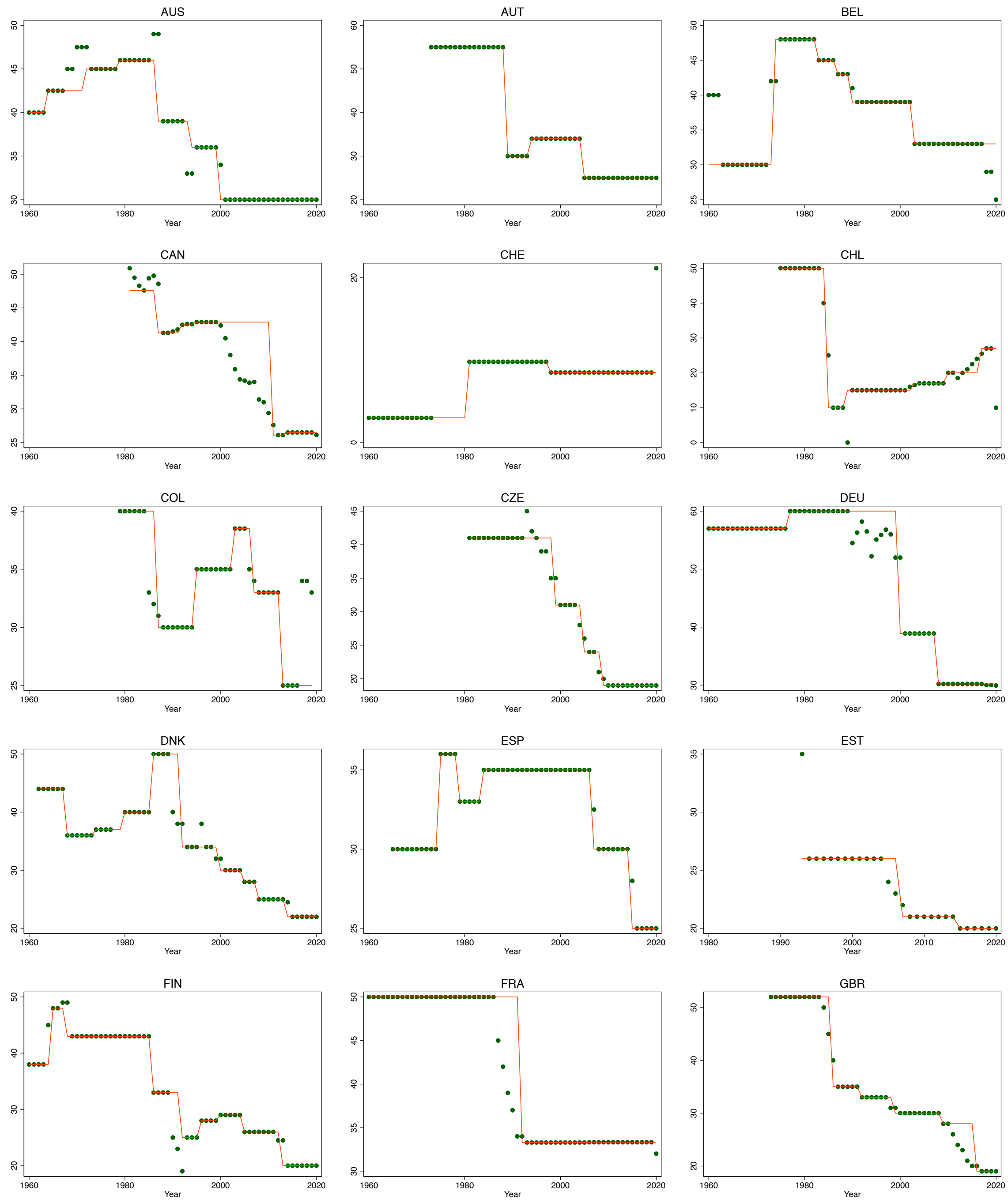
Notes: Access Dates: BIS: October 17, 2021, IMF:October 17, 2021, PWT: Dec 15, 2021

When aggregating corporate tax rates within a country, we always keep the top marginal rate to ensure consistency. Figure B.1 depicts the coverage of our sample. Table A.I reports our data sources and when they were accessed.

B.2 Permanent Tax Reforms by Country

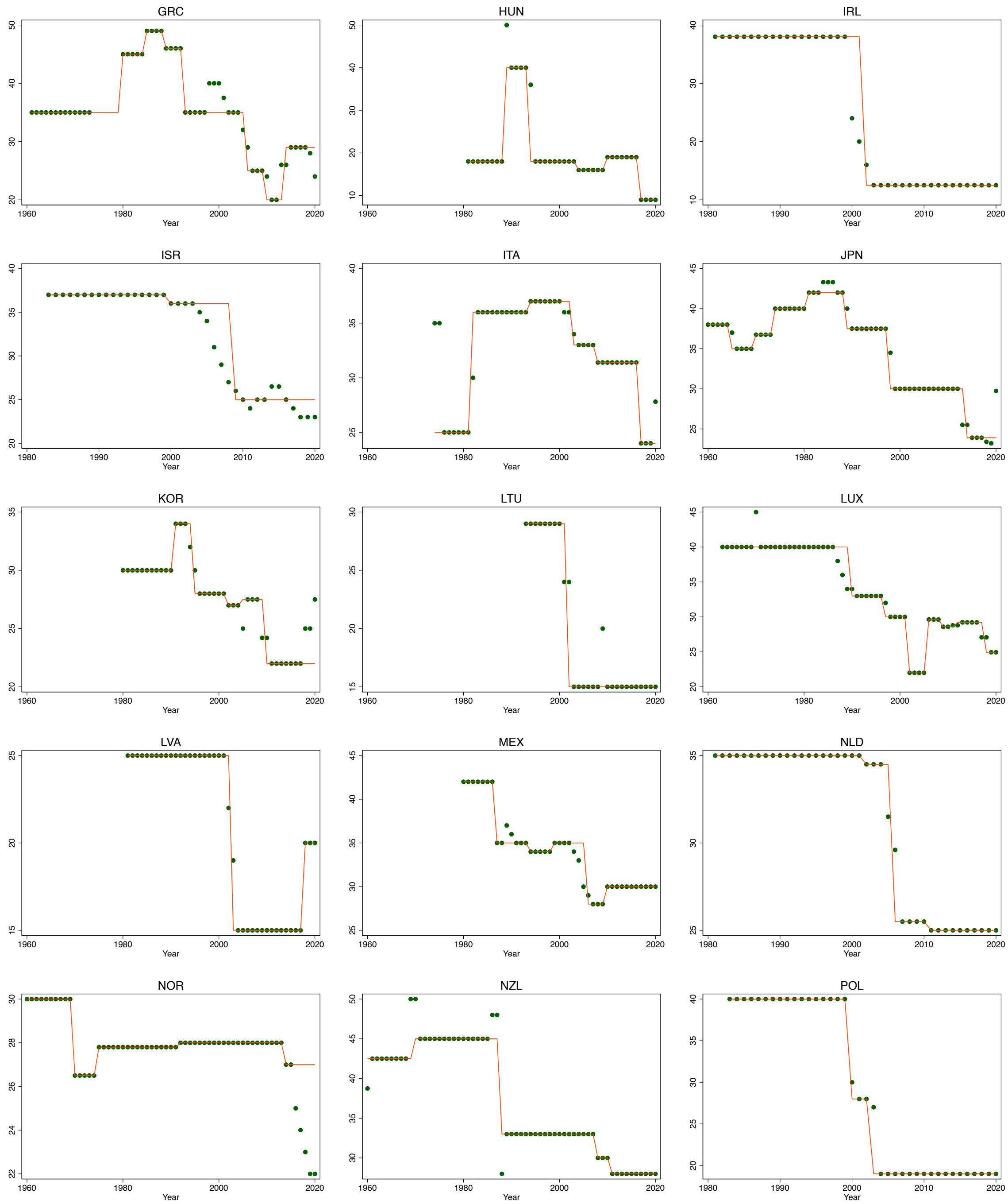
In this section, we present the result of the filtering procedure by country. Each figure plots the permanent corporate income tax series, along with the raw, unfiltered corporate income tax data.

Figure B.2 – Permanent Tax Reforms by Country (A)



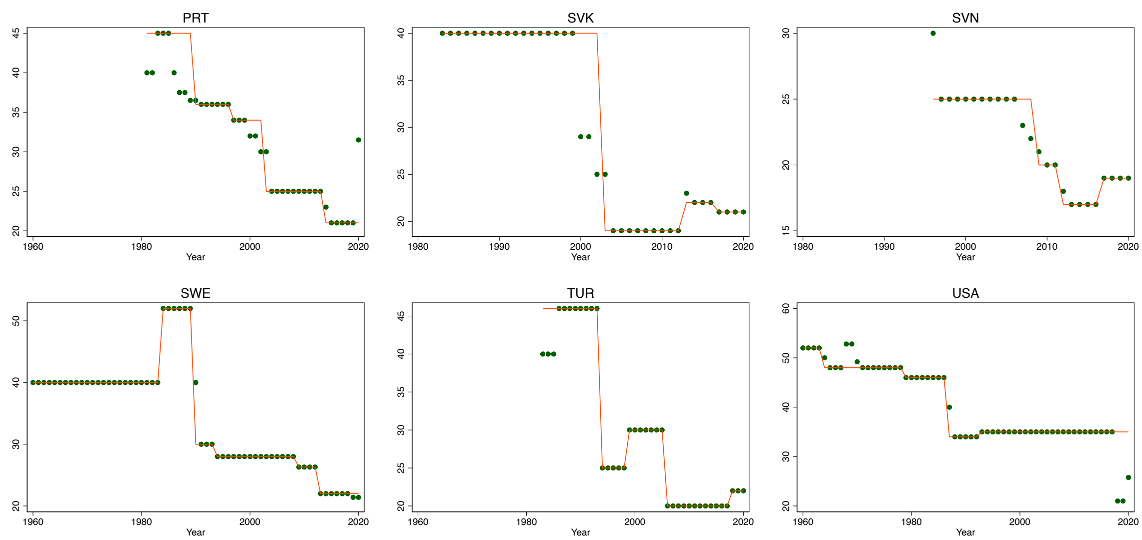
Notes: This figure plots raw and filtered top statutory corporate tax rate series.

Figure B.3 – Permanent Tax Reforms by Country (B)



Notes: This figure plots raw and filtered top statutory corporate tax rate series.

Figure B.4 – Permanent Tax Reforms by Country (C)

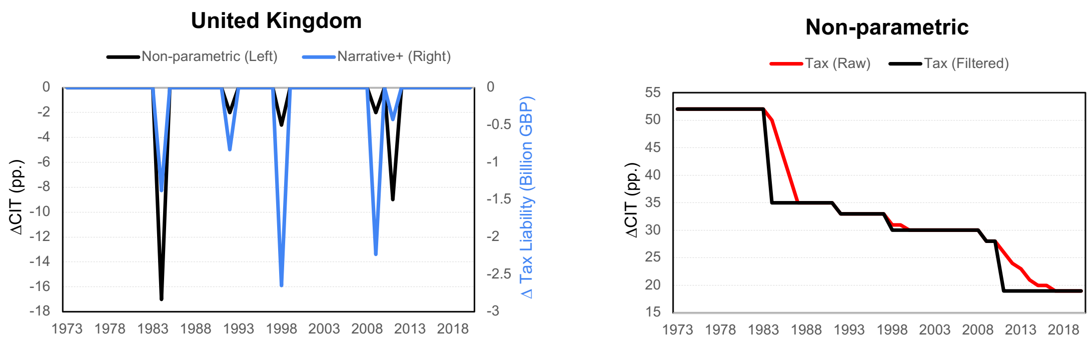


Notes: This figure plots raw and filtered top statutory corporate tax rate series.

B.3 Comparison with Narrative Measures

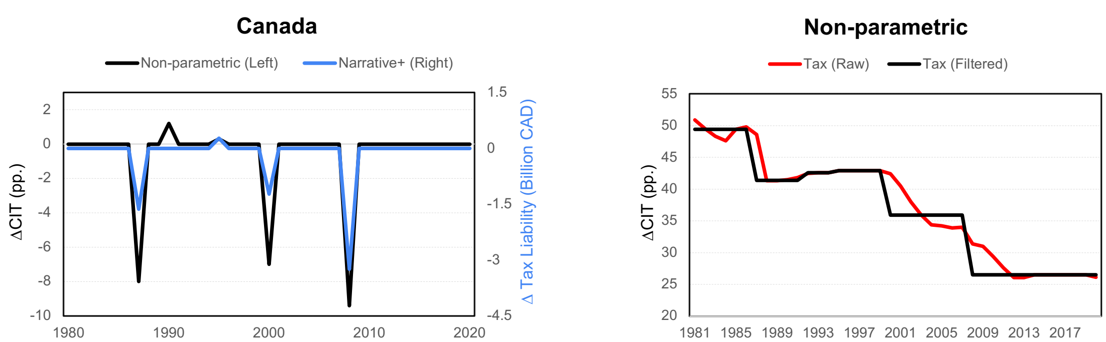
In this section, we present the comparison of the nonparametric filter approach with narrative approaches for specific countries. We find that our nonparametric measure largely coincides with the narrative approach and is less correlated with cyclical variation in aggregates relative to the narrative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

Figure B.5 – UK Tax Reforms: Nonparametric and Narrative Approaches



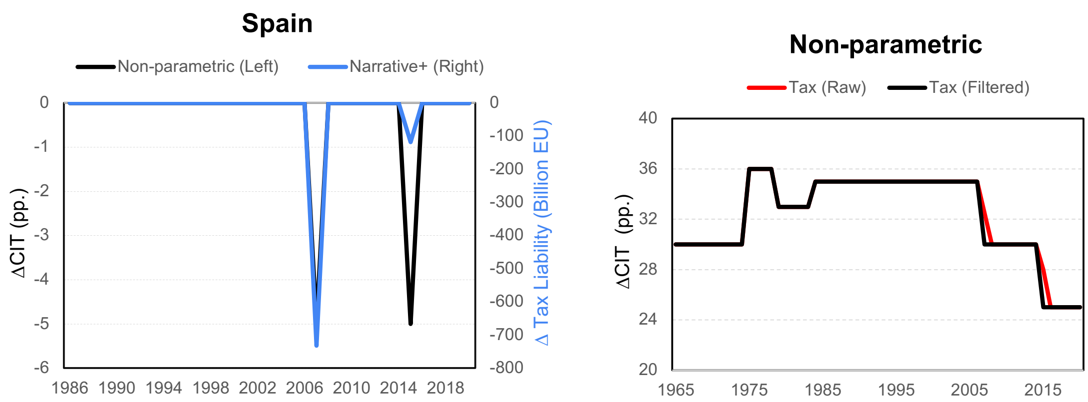
Notes: Source: Authors' calculations and Cloyne (2013). see Appendix B.1 for details.

Figure B.6 – Canada Tax Reforms: Nonparametric and Narrative Approaches



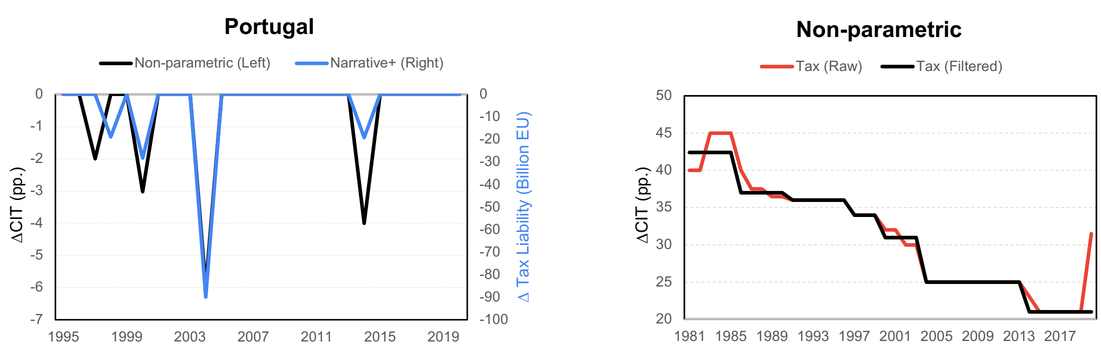
Notes: Source: Authors' calculations and Hussain and Liu (2019). see Appendix B.1 for details.

Figure B.7 – Spain Tax Reforms: Nonparametric and Narrative Approaches



Notes: Source: Authors' calculations and Gil et al. (2019), see Appendix B.1 for details.

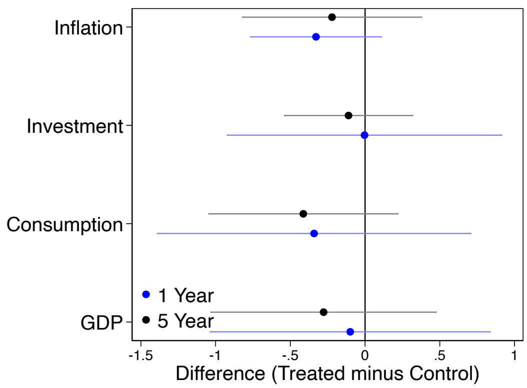
Figure B.8 – Portugal Tax Reforms: Nonparametric and Narrative Approaches



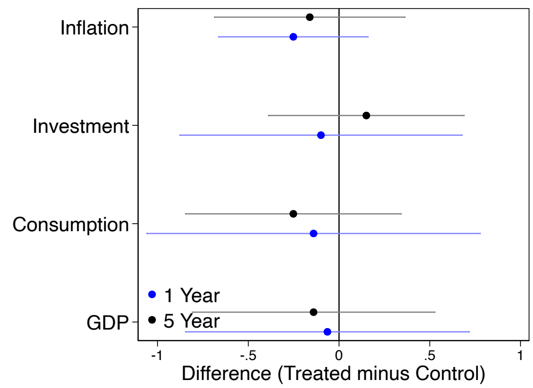
Notes: Source: Authors' calculations and Pereira and Wemans (2015). see Appendix B.1 for details.

Figure B.9 – Covariate Balance across Negative Tax Reforms, UK

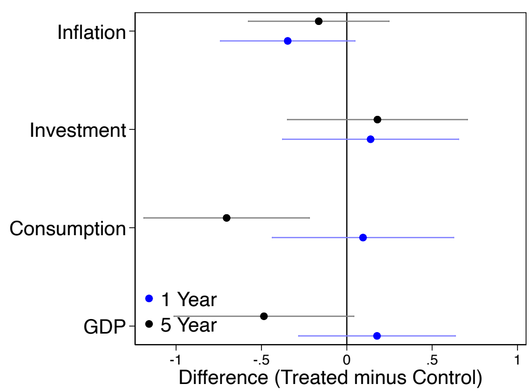
(A) Narrative



(B) Nonparametric

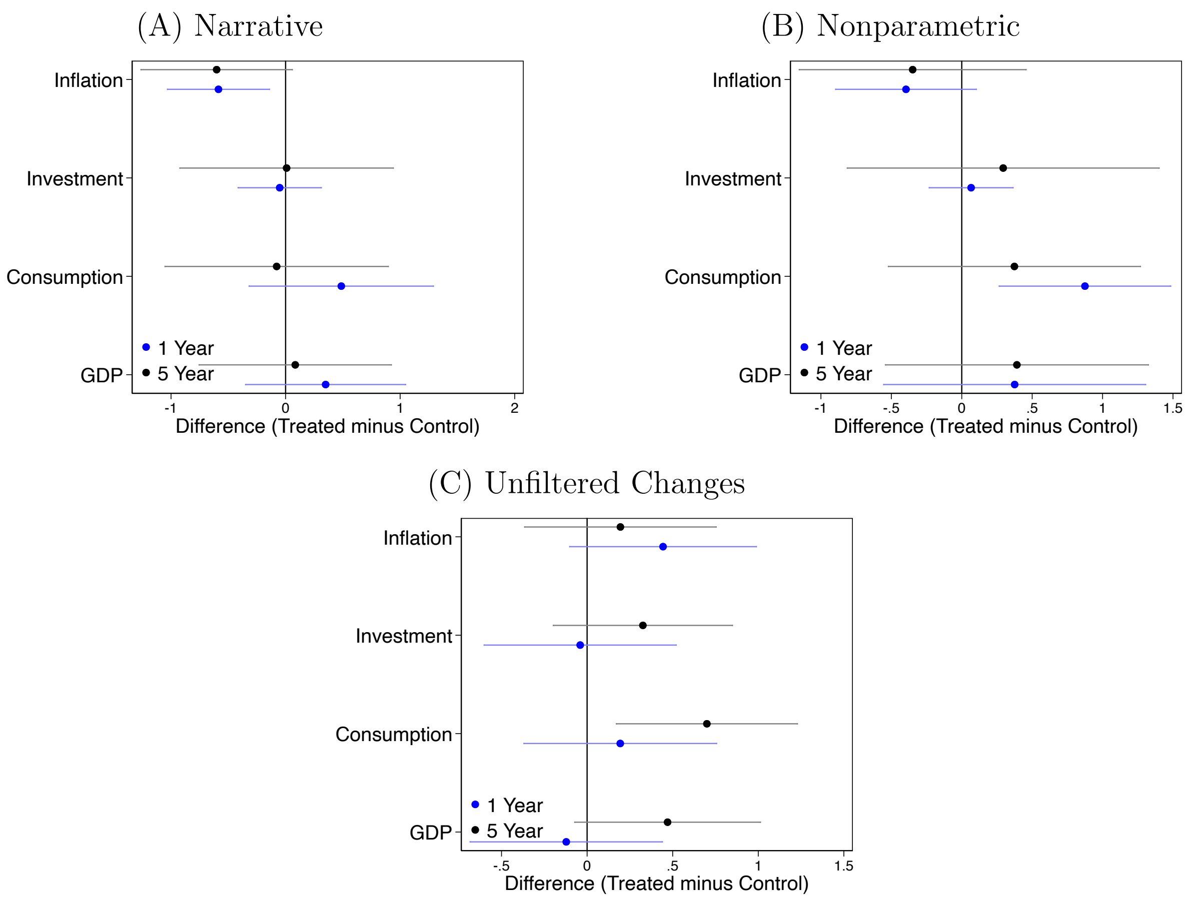


(C) Unfiltered Changes



Notes: These figures 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.

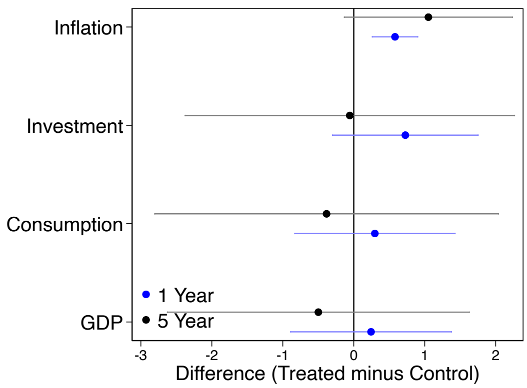
Figure B.10 – Covariate Balance across Negative Tax Reforms, Canada



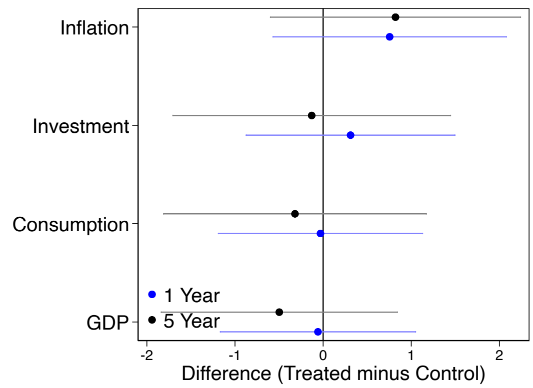
Notes: These figures show balance between treatment and control groups are balanced for contemporaneous variables and the 5-year lagged averages of the covariates. 95% confidence intervals reported. Output, consumption, and investment are in growth rates. Differences are scaled by standard deviation. 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 B.11 – Covariate Balance across Negative Tax Reforms, Spain

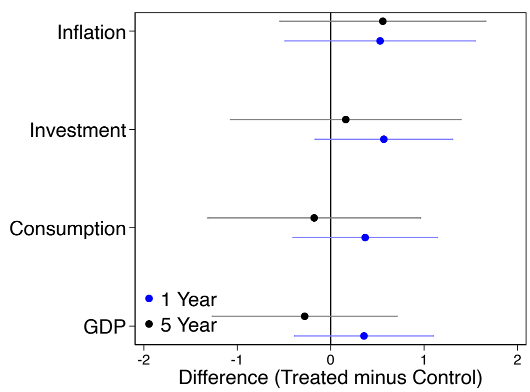
(A) Narrative



(B) Nonparametric

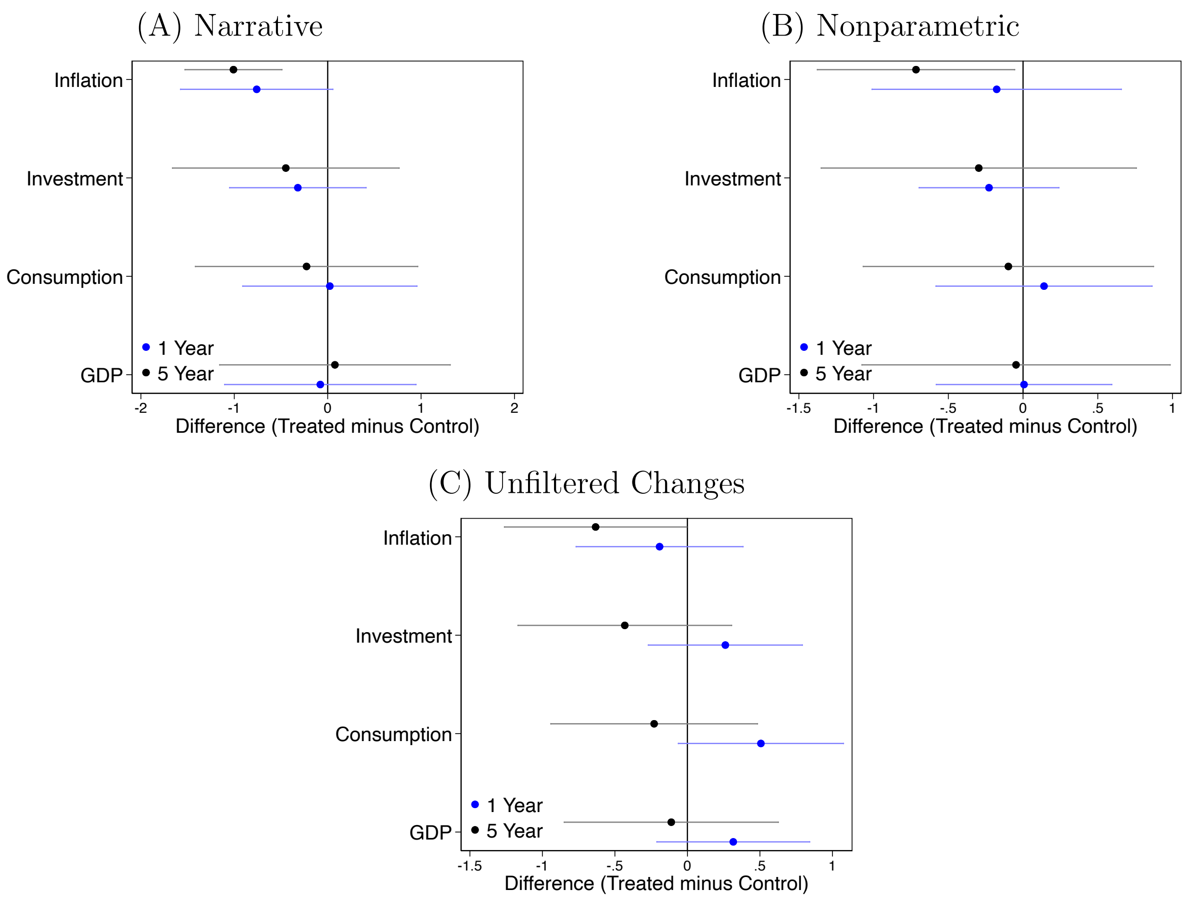


(C) Unfiltered Changes



Notes: These figures 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 B.**12** – Covariate Balance across Negative Tax Reforms, Portugal



Notes: These figures 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.

APPENDIX C

Appendix to Chapter 3

C.1 Data and BCE Replication

In this appendix, I discuss my replication of time series exercise featured in BCE and how I construct quarterly aggregate investment rate series using data from the Bureau of Economic Analysis (BEA). I follow the procedure outlined in BCE. This procedure incorporates data on total, equipment, and structure investment and capital from the following national account and fixed asset tables: Table 1.1.5 Gross Domestic Product lines 9-11 provides nominal annual private fixed nonresidential investment; Table 1.1. Fixed Assets and Consumer Durable Goods lines 4-6 provides annual capital stock at year-end prices; Tables 1.3 Fixed Assets and Consumer Durable Goods lines 4-6 provides nominal annual private nonresidential depreciation; Table 1.1.5 Gross Domestic Product lines 9-11 provides quarterly nominal fixed nonresidential investment; And, Table 1.1.9 Gross Domestic Product lines 9-11 quarterly implicit price deflators. BCE explains their data-cleaning procedure in detail in their appendix.

In 2013, the BEA announced a comprehensive revision of their national account and fixed asset data tables. As part of this revision, the BEA changed their treatment of intellectual property, equipment, and private nonresidential investment. Specifically, the BEA changed the name of Table 1.1.5 Gross Domestic Product line 11 from "equipment" to "equipment and software," separating equipment and software investment. Accompanying this change, they included intellectual property investment as a component of private nonresidential fixed investment. After 2013, Table 1.1.5 Gross Domestic Product line 12 reports annual private nonresidential fixed investment, defined as "investment in software, in research and development (R&D), and in entertainment, literary, and artistic originals by private business."

While this revision also included minor changes in the accounting of transactions costs and depreciation, the two revisions discussed above in detail are the revisions most pertinent to this analysis. Figure C.1 plots the extended sample against the replication data provided by BCE. The structure investment rate series closely tracks the series used in BCE, as the 2013 revision did not significantly affect the accounting of this variable. The equipment and total investment rate series are highly correlated with the series used in BCE, but diverge towards the end of the original sample, which tracks the recent upward trend in intellectual property investment in the US economy.

The time horizon in BCE is 1960Q1-2005Q4. The time horizon for my baseline specification is 1960Q1-2019Q4. To understand the relationship between the original study and this project, I replicate their baseline empirical specifications below using the data provided in their replication file. Their baseline empirical specifications take the follow form:

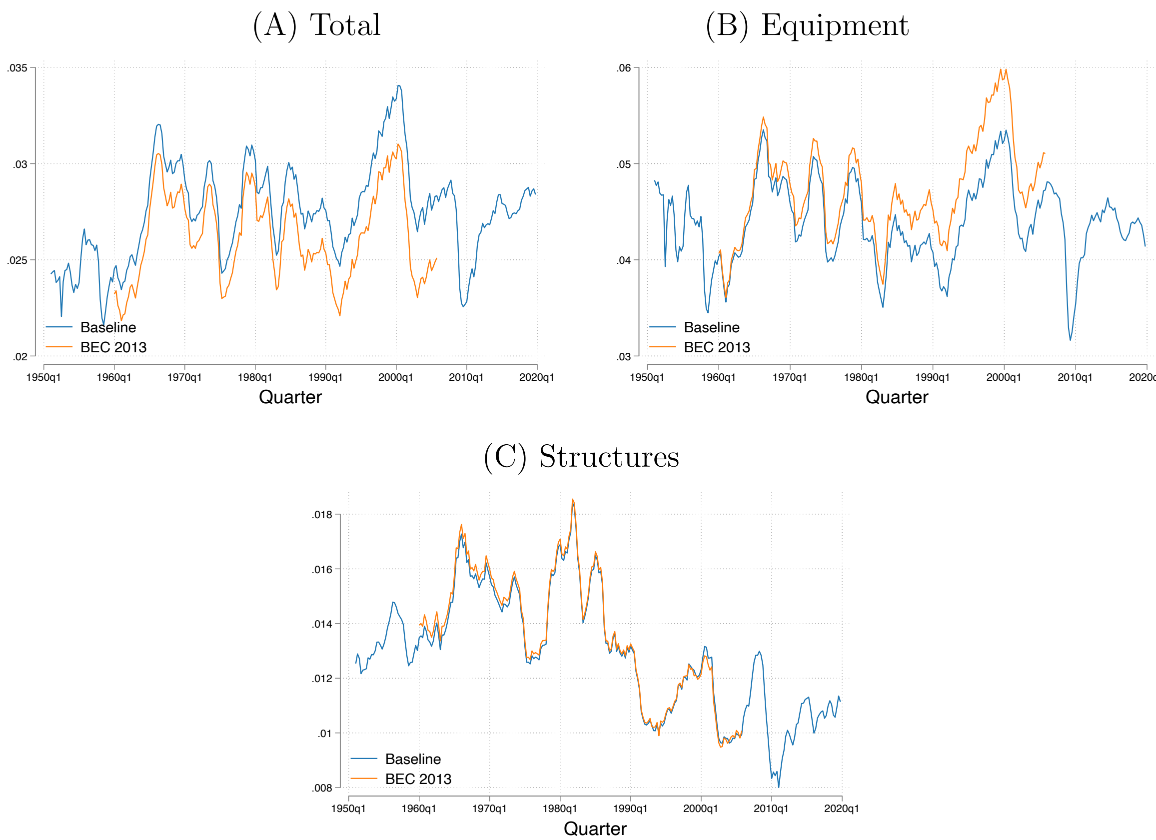
$$\left(\sigma_t^{BCE}\right)^2 = \alpha + \eta \bar{x}_{t-1}^k \tag{C.1}$$

$$|\sigma_t^{BCE}| = \alpha + \eta \bar{x}_{t-1}^k \tag{C.2}$$

I am able to replicate their baseline results in Stata 14. Table C.1 reports the original empirical results of BCE and correspond to Table 3 in their original text. Table C.2 presents the replication of their baseline results. In general the estimates are very close, varying only marginally. As an exercise in data validation, I reproduce their baseline specification for my extended sample. Table C.3 presents the results of this exercise. In general, the estimates are close.

Departing from these results, I use their second model, which regresses squared residuals onto lagged investment, for the rest of the replication exercises, as the regressor matches the regressor used in the standard GARCH(p,q) specification. Since I employ a Quasi-Maximum-Likelihood estimator (QMLE) to estimate the GARCH(1,1) specification, I replicate the BCE specification using QMLE and recover comparable estimates for the original and extended sample. Table C.4 presents the estimates recovered from the BCE replication data. Table C.6 presents the results of this exercise for the full sample. The coefficient of interest in the BCE estimated using QMLE is well within the 95% confidence interval of the original estimate recovered through OLS. As discussed in the main text of the paper, given the impersistence of conditional heteroskedasticity in the total and equipment investment series, the GARCH(1,1) series does not converge for total and equipment investment in the replication sample. In the extended sample, the autoregressive coefficient in the GARCH equation is statistically insignificant and negative, suggesting that those specifications are misspecified for total and equipment investment.

Figure C.**1** – Comparison of Baseline Sample and BCE



Notes: This plot contains three subplots, which compare the baseline sample used for empirical analysis in this paper against the replication data provided by Bachmann et al. (2013). Panel A plots the U.S. aggregate total investment rate series used in the baseline analysis against U.S. total aggregate investment rate series used in Bachmann et al. (2013). Panel A plots the U.S. aggregate equipment investment rate series used in the baseline analysis against U.S. equipment aggregate investment rate series used in Bachmann et al. (2013). Panel A plots the U.S. aggregate structure investment rate series used in the baseline analysis against U.S. structure aggregate investment rate series used in Bachmann et al. (2013).
Sources: BEA, Bachmann et al. (2013), and author's calculations.

Table C.1 – Table 3 from BCE

	TOT - model 1	TOT - model 2	EQ - model 1	EQ - model 2	ST - model 1	ST - model 2
p	6	6	7	7	6	6
k	6	6	8	8	2	2
$1000 * \eta$	45.93	.03731	30.62	.053780	39.96	.02581
t - η	3.121	2.496	2.089	1.724	4.097	3.246
p-value ($\eta > 0$) - bootstrap	.0088	.0236	.0375	.0742	.0033	.0094
$\log(\sigma_{max}/\sigma_{min})$	.7367	.5933	.5521	.4395	1.1167	1.1169
$\log(\sigma_{95}/\sigma_5)$	.6118	.4816	.4520	.3547	.9194	.8994
$\log(\sigma_{90}/\sigma_{10})$	.51203	.4082	.3355	.2646	.8003	.7403
Skewness	.1574	.1574	.3759	.3759	-.1051	-.1051
Excess Kurtosis	-.9803	-.9803	-.1401	-.1401	-.9864	-.9864
Autocorr. e_t	-.0452	-.0412	-.0151	-.0131	-.0823	-.0826
N	172	172	172	172	172	172

*Notes:*This table reports a replication Table 3 in Bachmann et al. (2013). The table features 6 GARCH-X regression results and summary statistics using aggregate investment rate data from 1960-2005. The first two columns report summary statistics and regression results for two specifications estimated on total US aggregate investment. The third and fourth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The fifth and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Model 1 estimates the volatility as a function of the absolute value of the lagged average of aggregate investment. Model 2 estimates squared volatility as a function of the lagged average of aggregate investment. The first row reports the lag order of the univariate autoregression that estimates the residuals. The second row reports the lag order of the lagged average of aggregate investment used in the second stage of the estimation. The third row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage. The fourth reports the t-statistic of η. The fifth row reports the p-value of a bootstrap of 20,000 simulations. The sixth through ninth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The tenth and eleventh rows report the skewness and excess kurtosis of the underlying series, respectively. The twelfth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA, Bachmann et al. (2013).

Table C.**2** – Replication of BCE, Replication Sample

	TOT - model 1	TOT - model 2	EQ - model 1	EQ - model 2	ST - model 1	ST - model 2
p	6	6	7	7	6	6
k	6	6	8	8	2	2
$1000 * \eta$	45.94	.037315	30.63	.053796	39.96	.025806
t - η	3.122	2.496	2.089	1.724	4.097	3.246
p-value ($\eta > 0$) - bootstrap	.0021	.0211	.0182	.0686	.0008	.0061
$\log(\sigma_{max}/\sigma_{min})$	.7368	.5933	.5521	.4396	1.1168	1.117
$\log(\sigma_{95}/\sigma_5)$	.6121	.4819	.4534	.3558	.9198	.89
$\log(\sigma_{90}/\sigma_{10})$	.5186	.407	.3355	.2646	.7944	.7329
Skewness	.1574	.1574	.376	.376	-.1051	-.1051
Excess Kurtosis	-.9803	-.9803	-.14	-.14	-.9863	-.9863
Autocorr. e_t	-.0452	-.0411	-.0151	-.0131	-.0822	-.0826
N	172	172	172	172	172	172

Notes: This table reports a replication of the original empirical analysis in Table 3 of Bachmann et al. (2013), using the replication data provided by the authors. The sample spans 1960-2005. This replication uses OLS to estimate the first and second stage of the GARCH process. The table features 6 GARCH-X regression results and summary statistics using aggregate investment rate data. The first two columns report summary statistics and regression results for two specifications estimated on total US aggregate investment. The third and fourth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The fifth and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Model 1 estimates the volatility as a function of the absolute value of the lagged average of aggregate investment. Model 2 estimates squared volatility as a function of the lagged average of aggregate investment. The first row reports the lag order of the univariate autoregression that estimates the residuals. The second row reports the lag order of the lagged average of aggregate investment used in the second stage of the estimation. The third row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage. The fourth reports the t-statistic of η. The fifth row reports the p-value of a bootstrap of 20,000 simulations. The sixth through ninth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The tenth and eleventh rows report the skewness and excess kurtosis of the underlying series, respectively. The twelfth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA, Bachmann et al. (2013), and author's calculations.

Table C.3 – Replication of BCE with Extended Sample

	TOT - model 1	TOT - model 2	EQ - model 1	EQ - model 2	ST - model 1	ST - model 2
p	6	6	7	7	6	6
k	6	6	8	8	2	2
$1000 * \eta$	37.95	.028454	28.87	.062561	24.87	.01867
t - η	3.031	2.23	1.944	1.796	3.218	3.156
p-value ($\eta > 0$) - bootstrap	.0052	.0517	.0286	.0556	.0098	.0193
$\log(\sigma_{max}/\sigma_{min})$	.9515	.7612	.5145	.5453	.7398	.858
$\log(\sigma_{95}/\sigma_5)$	.6075	.4535	.3821	.3849	.5078	.5415
$\log(\sigma_{90}/\sigma_{10})$	.4676	.3494	.2991	.2999	.4555	.4843
Skewness	.1475	.1475	-.0678	-.0678	.1261	.1261
Excess Kurtosis	-.198	-.198	-.1155	-.1155	-.7475	-.7475
Autocorr. e_t	-.0344	-.0308	-.0017	-.0022	-.0238	-.0346
N	228	228	228	228	228	228

Notes: This table reports the results of a replication of the original empirical analysis in Table 3 of Bachmann et al. (2013), using an extended sample from 1960-2019. The aggregate investment rate series is constructed following the process applied in Bachmann et al. (2013). This replication uses OLS to estimate the first and second stage of the GARCH process. The table features 6 GARCH-X regression results and summary statistics using aggregate investment rate data. The first two columns report summary statistics and regression results for two specifications estimated on total US aggregate investment. The third and fourth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The fifth and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Model 1 estimates the volatility as a function of the absolute value of the lagged average of aggregate investment. Model 2 estimates squared volatility as a function of the lagged average of aggregate investment. The first row reports the lag order of the univariate autoregression that estimates the residuals. The second row reports the lag order of the lagged average of aggregate investment used in the second stage of the estimation. The third row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage. The fourth reports the t-statistic of η. The fifth row reports the p-value of a bootstrap of 20,000 simulations. The sixth through ninth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The tenth and eleventh rows report the skewness and excess kurtosis of the underlying series, respectively. The twelfth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA, Bachmann et al. (2013), and author's calculations.

Table C.4 – Comparison of Baseline Specifications and BCE, Replication Sample

	T - BCE	T - G0,1)	T - G(1,1)	E - BCE	E - G(0,1)	E - G(1,1)	S - BCE	S - G(0,1)	S - G(1,1)
$1000 * \eta$	.039297	.	.	.042259	.	.	.024307	.	.
t - η	2.977	.	.	1.604	.	.	4.772	.	.
β^a	.	-.083	.014	.	-.061	.	.	.042	.039
t - β^a	.	-6.166	.274	.	-.685	.	.	.509	.912
β^g	.	.	-.915	.	.	.	.	.	.888
t - β^g	.	.	-1.895	.	.	.	.	.	7.524
$\log(\sigma_{max}/\sigma_{min})$	.634	1.788	.132	.344	.365	.	1.003	.209	.355
$\log(\sigma_{95}/\sigma_5)$	.512	.146	.066	.281	.139	.	.809	.093	.267
$\log(\sigma_{90}/\sigma_{10})$	.432	.118	.055	.209	.083	.	.674	.051	.215
Autocorr e_t	-.042	-.006	-.015	-.011	-.013	.	-.075	-.037	-.023
p-value ($e_t > 0$)	.579	.935	.836	.884	.862	.	.324	.628	.756
N	172	172	172	172	172	172	172	172	172

Notes: This table reports a replication of the original empirical analysis in Table 3 of Bachmann et al. (2013), using the replication data provided by the authors. The sample spans 1960-2005. The aggregate investment rate series is constructed following the process applied in Bachmann et al. (2013). This replication uses OLS to estimate the first stage and MLE to estimate the second stage of the GARCH process. The table features 9 GARCH regression results and summary statistics using aggregate investment rate data. The first three columns report summary statistics and regression results for three specifications estimated on total US aggregate investment. The fourth, fifth, and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The seventh, eighth, and ninth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Columns labeled BCE estimates squared volatility as a function of the lagged average of aggregate investment. Columns labeled G(.,.) estimates standard GARCH(a, b) estimations, where a is the autoregressive lag order and b is the moving average lag order. The first row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage for BCE specifications. The second row reports the t-statistic of η for BCE specifications. The third row presents the regression coefficient for the moving average component of GARCH specifications. The fourth row reports the t-statistic of the moving average component of GARCH specifications. The fifth row presents the regression coefficient for the autoregressive component of GARCH(1,1) specifications. The sixth row reports the t-statistic of the autoregressive component of GARCH(1,1) specifications. The seventh through tenth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The eleventh and twelfth rows report the skewness and excess kurtosis of the underlying series, respectively. The thirteenth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA, Bachmann et al. (2013), and author's calculations.

Table C.5 – Comparison of Baseline Specifications and BCE, Full Sample

	T - BCE	T - G(0,1)	T - G(1,1)	E - BCE	E - G(0,1)	E - G(1,1)	S - BCE	S - G(0,1)	S - G(1,1)
$1000 * \eta$	.024554	.	.	.046526	.	.	.012395	.	.
t - η	2.9721	.	.	2.0265	.	.	3.3554	.	.
β^a	.	.156	.1687	.	.0802	.1032	.	.0192	.027
t - β^a	.	1.7982	1.9923	.	.9969	1.1597	.	.27	.6635
β^g	.	.	-.2994	.	.	-.4573	.	.	.8855
t - β^g	.	.	-1.1143	.	.	-.8819	.	.	5.1541
$\log(\sigma_{max}/\sigma_{min})$	.6323	.668	1.634	.3882	.4706	.7775	.5227	.1061	.2403
$\log(\sigma_{95}/\sigma_5)$	.3899	.2552	.3358	.2825	.1337	.2175	.3524	.041	.1746
$\log(\sigma_{90}/\sigma_{10})$	.3005	.2044	.2415	.2209	.1055	.1662	.3162	.024	.1449
Autocorr. e_t	-.0301	-.0336	-.0292	-.0008	-.0052	-.0057	-.0213	-.0183	-.0033
p-value ($e_t > 0$)	.6519	.6144	.6606	.9904	.9371	.9308	.7489	.7839	.96
N	228	228	228	228	228	228	228	228	228

Notes: This table reports a replication of the original empirical analysis in Table 3 of Bachmann et al. (2013), using an extended sample from 1960-2019. This replication uses OLS to estimate the first stage and MLE to estimate the second stage of the GARCH process. The table features 9 GARCH regression results and summary statistics using aggregate investment rate data. The first three columns report summary statistics and regression results for three specifications estimated on total US aggregate investment. The fourth, fifth, and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The seventh, eighth, and ninth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Columns labeled BCE estimates squared volatility as a function of the lagged average of aggregate investment. Columns labeled G(.,.) estimates standard GARCH(a, b) estimations, where a is the autoregressive lag order and b is the moving average lag order. The first row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage for BCE specifications. The second row reports the t-statistic of η for BCE specifications. The third row presents the regression coefficient for the moving average component of GARCH specifications. The fourth row reports the t-statistic of the moving average component of GARCH specifications. The fifth row presents the regression coefficient for the autoregressive component of GARCH(1,1) specifications. The sixth row reports the t-statistic of the autoregressive component of GARCH(1,1) specifications. The seventh through tenth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The eleventh and twelfth rows report the skewness and excess kurtosis of the underlying series, respectively. The thirteenth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA and author's calculations.

Table C.6 – Comparison of Baseline Specifications and BCE, Full Sample

	T - BCE	T - G(0,1)	T - G(1,1)	E - BCE	E - G(0,1)	E - G(1,1)	S - BCE	S - G(0,1)	S - G(1,1)
$1000 * \eta$	.024554	.	.	.046526	.	.	.012395	.	.
t - η	2.9721	.	.	2.0265	.	.	3.3554	.	.
β^a	.	.156	.1687	.	.0802	.1032	.	.0192	.027
t - β^a	.	1.7982	1.9923	.	.9969	1.1597	.	.27	.6635
β^g	.	.	-.2994	.	.	-.4573	.	.	.8855
t - β^g	.	.	-1.1143	.	.	-.8819	.	.	5.1541
$\log(\sigma_{max}/\sigma_{min})$	.6323	.668	1.634	.3882	.4706	.7775	.5227	.1061	.2403
$\log(\sigma_{95}/\sigma_5)$	.3899	.2552	.3358	.2825	.1337	.2175	.3524	.041	.1746
$\log(\sigma_{90}/\sigma_{10})$	.3005	.2044	.2415	.2209	.1055	.1662	.3162	.024	.1449
Autocorr. e_t	-.0301	-.0336	-.0292	-.0008	-.0052	-.0057	-.0213	-.0183	-.0033
p-value ($e_t > 0$)	.6519	.6144	.6606	.9904	.9371	.9308	.7489	.7839	.96
N	228	228	228	228	228	228	228	228	228

Notes: This table reports a replication of the original empirical analysis in Table 3 of Bachmann et al. (2013), using an extended sample from 1960-2019. This replication uses OLS to estimate the first stage and MLE to estimate the second stage of the GARCH process. The table features 9 GARCH regression results and summary statistics using aggregate investment rate data. The first three columns report summary statistics and regression results for three specifications estimated on total US aggregate investment. The fourth, fifth, and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The seventh, eighth, and ninth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Columns labeled BCE estimates squared volatility as a function of the lagged average of aggregate investment. Columns labeled G(.,.) estimates standard GARCH(a, b) estimations, where a is the autoregressive lag order and b is the moving average lag order. The first row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage for BCE specifications. The second row reports the t-statistic of η for BCE specifications. The third row presents the regression coefficient for the moving average component of GARCH specifications. The fourth row reports the t-statistic of the moving average component of GARCH specifications. The fifth row presents the regression coefficient for the autoregressive component of GARCH(1,1) specifications. The sixth row reports the t-statistic of the autoregressive component of GARCH(1,1) specifications. The seventh through tenth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The eleventh and twelfth rows report the skewness and excess kurtosis of the underlying series, respectively. The thirteenth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA and author's calculations.

Table C.7 – Comparison of Baseline Specifications and BCE, Full Sample, HP-Filtered

	T - BCE	T - G(0,1)	T - G(1,1)	E - BCE	E - G(0,1)	E - G(1,1)	S - BCE	S - G(0,1)	S - G(1,1)
$1000 * \eta$	.023094	.	.	.066422	.	.	.019054	.	.
t - η	1.6877	.	.	2.0839	.	.	2.0276	.	.
β^a	.	.1163	.128	.	.0729	.1227	.	-.0648	.0408
t - β^a	.	1.5597	1.6302	.	1.0308	1.2436	.	-1.4834	.8824
β^g	.	.	-.3111	.	.	-.5063	.	.	.8847
t - β^g	.	.	-.6755	.	.	-1.0023	.	.	7.8317
$\log(\sigma_{max}/\sigma_{min})$	.2863	.5288	.9712	.3541	.4185	.9994	.3862	.778	.35
$\log(\sigma_{95}/\sigma_5)$	.2348	.1952	.2604	.2445	.1228	.2495	.235	.1122	.2475
$\log(\sigma_{90}/\sigma_{10})$	.1791	.1442	.1865	.1928	.088	.1871	.1947	.081	.1919
Autocorr. e_t	-.0192	-.0263	-.0181	-.0085	-.015	-.0134	-.0227	-.0322	.011
p-value ($e_t > 0$)	.7732	.6934	.7857	.8985	.8214	.8403	.7336	.6285	.8691
N	228	228	228	228	228	228	228	228	228

Notes: This table reports a replication of the original empirical analysis in Table 3 of Bachmann et al. (2013), using an extended sample from 1960-2019. All investment series are HP-filtered prior to estimation using a smoothing parameter of 1600. This replication uses OLS to estimate the first stage and MLE to estimate the second stage of the GARCH process. The table features 9 GARCH regression results and summary statistics using aggregate investment rate data. The first three columns report summary statistics and regression results for three specifications estimated on total US aggregate investment. The fourth, fifth, and sixth columns report summary statistics and regression results for two specifications estimated on US aggregate equipment investment. The seventh, eighth, and ninth columns report summary statistics and regression results for two specifications estimated on US aggregate structure investment. Columns labeled BCE estimates squared volatility as a function of the lagged average of aggregate investment. Columns labeled G(.,.) estimates standard GARCH(a, b) estimations, where a is the autoregressive lag order and b is the moving average lag order. The first row presents the rescaled regression coefficient for the lagged average of aggregate investment in the second stage for BCE specifications. The second row reports the t-statistic of η for BCE specifications. The third row presents the regression coefficient for the moving average component of GARCH specifications. The fourth row reports the t-statistic of the moving average component of GARCH specifications. The fifth row presents the regression coefficient for the autoregressive component of GARCH(1,1) specifications. The sixth row reports the t-statistic of the autoregressive component of GARCH(1,1) specifications. The seventh through tenth rows report different measures of quantile distance for the predicted volatility of aggregate investment. The eleventh and twelfth rows report the skewness and excess kurtosis of the underlying series, respectively. The thirteenth row reports the first order autocorrelation of the latent structural shock. The last row reports the sample size.
Sources: BEA and author's calculations.

C.2 Monte Carlo Experiments

In this appendix, I run two Monte Carlo experiments to document the performance of BCE specifications on data generated using a GARCH process. This experiment seeks to understand how the BCE would perform when the underlying data generating process is symmetric. It is not intended to assume that the underlying data generating process of aggregate investment is symmetric. The data generating process in this experiment takes the following form

$$x_{1,t} = \rho x_{t-j} + \varepsilon_{1,t}, \tag{C.1}$$

$$\varepsilon_{1,t} = e_t \sigma_{1,t},\ e_t \sim N(0,1), \tag{C.2}$$

$$\sigma^2_{1,t} = \alpha + \beta_g \sigma^2_{1,t-1} + \beta_a \varepsilon^2_{1,t-1}, \tag{C.3}$$

where $\rho = 0.95$, $\alpha = 6e^{-4}$ coincide with the unconditional volatility and autoregressive consistent for aggregate investment. I report the average η and corresponding t-statistic for different values of β_g and β_a. Each Monte Carlo experiment consists of 10,000 simulations with 172 observations each, which is the sample length of the original BCE empirical analysis. Table C.8 reports the average η of the simulations, and Table C.8 reports probability that η is significant at a 5% level. There is no standard systematic between the size of the GARCH coefficinets and the average value of η. That said, the probability that the null hypothesis of $\eta = 0$ is rejected increases as the autoregressive and moving average coefficients increase. For US aggregate investment, the moving average component of the baseline specification is below 0.2. This analysis provides two insights. First, the BCE specification may provide imprecise intuition for the evolution of the conditional volatility of aggregate investment when the underlying data is generated by GARCH(1,1) specification, when the moving average and autoregressive components are high. Second, it is unlikely that an econometrician would observe a relationship between the conditional volatility of aggregate investment and the lagged average of aggregate investment if the underlying data is generated by GARCH(1,1) specification.

Table C.8 – Average η, Monte Carlo Simulation

$\beta_a \setminus \beta_g$	0.2	0.4	0.6	0.8
0.2	-0.0005	0.0008	-0.0011	-0.0018
0.4	-3.420E-05	-0.0012	-0.0004	0.0023
0.6	-4.820E-05	-0.0001	-0.0026	0.0038
0.8	-0.0005	0.0055	0.0041	-0.0016

Notes: This table reports the average η in a Monte Carlo experiment consists of 10,000 simulations with 172 observations each, where the underlying data generating process is a GARCH(1,1). The rows in the table report the coefficient on the moving average component.
Source: Author's calculations.

Table C.9 – Probability $t_\eta > 1.96$, Monte Carlo Simulation

$\beta_a \setminus \beta_g$	0.2	0.4	0.6	0.8
0.2	0.0484	0.102	0.1509	0.2117
0.4	0.0661	0.1372	0.2378	0.3156
0.6	0.0992	0.2364	0.3317	0.4092
0.8	0.1899	0.4103	0.4349	0.4525

Notes: This table reports the probability that the t_η statistic is greater than 1.96 in a Monte Carlo experiment consists of 10,000 simulations with 172 observations each, where the underlying data generating process is a GARCH(1,1). The rows in the table report the coefficient on the moving average component.
Source: Author's calculations.

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