Fiscal Policy in Monetary Unions: State Partisanship and its Macroeconomic Effects

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– PRELIMINARY –

Abstract

States have become an increasingly important agent of fiscal policy in the U.S. Motivated by the large literature that finds increases in partisanship among policymakers, we analyze whether partisanship affects state fiscal policy and what its macroeconomic effects are. Using data from close elections, we find strong partisanship effects in the passthrough of federal transfers to state: Republican governors spend less of federal funds and, instead, cut distortionary taxes. Transfers are an important vehicle of federal policies, with a share of 40% in the 2009 stimulus bill and funding the 2014 Medicaid expansion. We provide causal evidence that the passthrough of federal transfers by state governments varies between Republican and Democratic governors, using a regression-discontinuity design. To analyze the macroeconomic effects of this partisan behavior, we use a structural model of Republican and Democratic regions in a monetary union. The model delivers an aggregate transfer multiplier that is significantly lower with partisan differences. This is due to distortionary tax cuts that lower the initial aggregate demand effects, but make Republican states more competitive with a delay. Our model implies that the transfer multiplier varies over time with the partisan affiliation of governors and we find empirical support for this prediction using local-projection methods.

Keywords: partisanship, Republican, Democratic, states, fiscal policy, distortionary taxes, monetary union, fiscal multiplier, regression discontinuity.

JEL codes: C24, E62, F45, H72, H77.

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1 Introduction

Partisanship of policymakers is at historically high levels in the U.S., both when measured according to roll-call votes in the U.S. Congress (McCarty et al., 2016; Andris et al., 2015) and when measured based on news sources (Azzimonti, 2017).[1] However, the economic consequences of partisanship are unclear, a priori. According to the press, partisanship has affected the implementation of major policy initiatives, such as the Medicaid expansion by U.S. states, an integral part of the Obama-era healthcare reform. Ongoing partisan conflicts over the federal budget are also said to have led to government shutdowns.[2] Contrary to such anecdotal reports of partisan policy bias, Ferreira and Gyourko (2009) show that municipal governments in the U.S. do not exhibit partisan behavior.

More broadly, the literature on the consequences of partisanship is inconclusive. Empirically, increased partisanship may simply lead to inaction due to gridlock in the U.S. (e.g., Binder, 1999). Mian et al. (2014) show that polarization reduces the likelihood of political reform across countries. Theoretical predictions are also unclear. In a simple median voter model, increased partisanship of legislators need not lead to increased divergence in legislative outcomes (McCarty, 2007, p. 232f.).[3] In simple probabilistic voting models with ideological preferences independent of incomes, increased ideological polarization leaves the economic policy outcome unchanged (Krasa and Polborn, 2014b). In Krasa and Polborn's model of differentiated candidates, polarization of candidates' social ideology leads to a uniform shift in fiscal policy, rather than increased divergence.

In this paper, we analyze the degree of partisanship in the fiscal policies of U.S. states and quantify how it affects fiscal policy at both the state and the federal level. Specifically, our paper sets out to answer three questions: First, how partisan are fiscal policies in U.S. states? Second, has the degree of partisanship varied over time? Third, what, if any, are the aggregate effects of partisanship in the states?

To address the first question, we focus on the partisanship of state governors, because U.S. states have become increasingly important for fiscal policy. From 1950 to 1999, state incomes taxes and state spending have increased almost twice as fast as state incomes (Besley and Case, 2003, p. 19). Since the 1980s, consumption and investment spending by state and local governments has been, on average, 45% higher than the corresponding federal spending. At the state-level, governors are powerful: They propose budgets and hold line-item vetoes in all but seven states (Bohn and Inman,

[1] Co-sponsorship coalitions in the U.S. Congress did not suggest increased partisanship before 2007, but data from 2007 to 2012 also points to increased partisanship (Harbridge, 2015).

[2] A New York Times op-ed talks about "incentive[s] ... to use the budget process as a tool of partisan skirmishing" and relates it to the government shutdowns since the mid-1990s ("The Shutdown Shows the Twisted Rules of a Broken Congress", 1/20/2018). The Washington Post Editorial Board argues that Medicaid expansion "was a bargain that no state leader should have passed up. Yet Republican politicians have blocked Medicaid expansion in half the states." (in: "Millions will remain uninsured because of blocked Medicaid expansion in states", 11/15/2013).

[3] In their one-period example, Alesina and Rosenthal (1996) give a sufficient condition for a sufficiently polarized presidential candidate to be moderated by a divided government when legislative positions differ from executive positions. Drazen (2000, p. 257) also discusses how increased polarization need not translate to larger differences in policies, due to divided government or endogenous adjustment of policymakers' positions. Consistent with this view, polarization measured with roll-call votes positively correlated with measures of inaction based, for example, on the passage of major laws and the confirmation time of judges in the U.S. (McCarty, 2007).

1996, Table 2). And since, in the average year, 11 out of 50 governors are elected with a margin of less than 5 percentage points, looking across U.S. states gives us sufficient variation to estimate the causal effect of gubernatorial partisanship on states' fiscal policies. While we find no unconditional differences between parties, we do find partisan effects in the passthrough of federal transfers in the U.S. This finding matters because federal transfers have become an important part of federal policies. They have grown six times faster than GDP since the end of WWII and now account for about 3% of GDP and 13% of federal expenditures. Transfers, largely to states, amounted to about 40% of the 2009 stimulus bill (Carlino and Inman, 2013). Also the 2014 Medicaid expansion relied on federal transfers to the states, initially covering all and eventually covering 90% of the costs to states. Uptake of the Medicaid expansion largely followed partisan lines, consistent with partisan differences in states policies.[4]

We measure the degree of partisanship in state fiscal policy as the difference in policy rules under Democratic and Republican governors. To estimate these rules, we use an unbalanced panel of states with close gubernatorial elections. Conditioning on close election is a natural approach to quantify the effect of governors on policy, because this conditioning should average over omitted variables. The discontinuity in the fiscal policy rules caused by the narrow election outcome gives the estimated partisan differences a causal interpretation. While we do not find robust unconditional differences between governors, we find robust differences in the use of federal transfers since the Reagan era. In response to increased transfers, Republican governors pass less of the transfers through to spending than Democrats, about one dollar for each dollar received. Instead, they lower the growth rate of taxes and of income and sales taxes in particular. In response to cuts in transfers, Republican governors cut more expenditures and use the difference again to lower taxes relative to their Democratic counterparts. We also have some evidence that, initially, the different use of transfers inflows increases economic activity more in Democratic states. But, with a delay, the policy differences lead to relatively higher economic activity in Republican-run states.

As to the second question, we find that the degree of partisanship in some, but not all fiscal policies has varied over time. Specifically, we re-estimate the fiscal reaction functions for rolling 20-year windows. For expenditure growth, the data are consistent with a lower Republican pass-through of federal transfers to spending throughout the sample period. In contrast, we find that the lower tax revenue growth under Republican governors in response to inflows of federal funds has emerged in the Reagan era. The increased partisan policy difference since the 1980s are qualitatively in line with time variation in measured partisanship in McCarty et al. (2016) and Azzimonti (2017).

To adress the final question of the aggregate effects of state partisanship, we use the estimated partisan fiscal policy rules to calibrate a general equilibrium model with heterogeneous regions. The model features two representative regions whose expenditure policies correspond to the estimated rules for Democratic and Republican governors. Other policies follow from tax adjustment rules and budget balance, but are consistent with our estimates. Our model is an extension of

[4] According to https://www.advisory.com/daily-briefing/resources/primers/medicaidmap (accessed 3/14/2018), all 18 states that have not expanded Medicaid coverage as of November 2017 did so because of Republican governors or legislatures.

the model of fiscal policy in a monetary union in Nakamura and Steinsson (2014). The model features representative households and firms in two representative regions. Households are subject to labor income taxes, provide labor elastically, and accumulate private capital. Firms operate in monopolistic competition subject to price-setting frictions. Varieties are priced in a common currency. A central bank conducts monetary policy. Our main departure is to include states as fiscal policy makers. The two regions in the model differ in their response to federal transfers, in line with our empirical estimates, and possibly in their size. We allow for the possibility that public consumption is a complement or a substitute for private consumption and we introduce productive state infrastructure, following Barro and Sala-I-Martin (1992) and Drautzburg and Uhlig (2015).[5] States finance their expenditures with federal transfers, distortionary income taxes, and lump-sum taxes. In our baseline calibration, where state government consumption complements private consumption as in Bermperoglou et al. (2017), the lower Republican pass-through cuts the short-run federal transfer multiplier in half. Republican states have smaller output in the short-run due to lower demand, but higher outputs down the road due to tax cuts, consistent with our estimates of the effects on economic activity.

Our model implies that the passthrough of federal transfers to government spending and, ultimately, to output depends on the partisan makeup of states. Over our sample period beginning with Reagan, we find that the short-run multiplier varies as much as 15 cent on the dollar, or by roughly one-third, driven by the changing partisan affiliation of governors. Moreover, our empirical estimates suggest that this phenomenon was not present before the 1980s. Together, these findings imply a source of time-variation in fiscal multipliers in addition to the economic state dependence of fiscal multipliers in Auerbach and Gorodnichenko (2012b) and Ramey and Zubairy (2014).

To validate our model, we turn to time-series data. We use local projection methods to assess the model prediction that aggregate multipliers depend on the partisan affiliation of governors. The share of Republican governors has varied between 30% and 68% of all states since 1980. Using two different approaches to allow time variation in local projection – a two-step approach based on time-varying windows and a direct approach using interaction terms – we confirm the model prediction that the impact response of aggregate GDP to an innovation in intergovernmental transfers falls with the share of Republican governors.

Our findings imply that federal policy makers concerned with stimulating the economy should consider the state of politics. When many Republican governors are governing the states, the short-term stimulative of transferring money to the states is low. In contrast, it may have beneficial supply-side effects with a delay. While our estimates focus on the U.S., understanding the economics of federal transfers is also relevant for other federal states and international bodies such as the European Union. For example, a recent report by the European Court of Auditors on transfers as part of the EU Youth Employment Guarantee notes a risk that "resources will not lead to a net increase in the level of available funding" (Ivanova et al., 2017, § 113).

[5] In ongoing work, we follow Farhi and Werning (2016) in examining the role of risk-sharing in our model. We also plan on including borrowing constrained agents following Galí et al. (2007).

After discussing the literature in section 2, we continue with the empirics. Section 3 describes the dataset and provides details on intergovernmental transfers. Section 4 provides our empirical analysis of fiscal policies for the post-Reagan era. Section 5 analyzes time-variation in the degree of partisanship. Section 6 introduces our empirical estimates in a model of a monetary union. Finally, Section 7 uses time series data to test the model prediction that transfer multipliers depend on the partisan affiliation of governors.

2 Literature

Our work builds on and contributes to three strands of literature: Partisanship in political economy, intergovernmental transfers in public finance, and the macroeconomic analysis of fiscal policy.

Within the large literature on political economy, our work is related to the work on partisan politicians, as opposed to office-motivated politicians, in the taxonomy of Alesina (1988). Broadly, we build on the literature on partisan political cycles. Alesina (1987) provides a theoretical foundation. We do not model how partisan differences arise in equilibrium, but rather quantify their consequences, if any, in the spirit of the initial study by Hibbs (1977).[6] Our work also relates to Alesina (1988): We quantify the macroeconomic effects of partisan fiscal politics. As opposed to the work on the political budget cycle that analyzes opportunistic fiscal policies around elections (e.g., Alesina and Paradisi, 2017), we measure partisan differences over the full term.

Specifically, we analyze U.S. partisan differences at the state level. Besley and Case (2003) summarize a first wave of studies from which they conclude that "party control and identify matter" (p. 67) for outcomes at the U.S. state level. Most studies surveyed in their paper proceed by OLS and are, however, potentially subject to simultaneity bias, particularly through omitted variables bias in light of the interplay of economics, voter preferences, and partisanship. Regression Discontinuity (RD) is a natural approach to quantify the effect of elected officials on policy outcomes: If vote shares reflect underlying and potentially omitted fundamentals, conditioning on close elections selects a sample with roughly equal values of the omitted variables. A number of studies have used this technique since Besley and Case (2003). Lee et al. (2004) show that marginally elected congressmen have partisan voting patterns at the U.S. federal level. At the U.S. state level, Beland (2015) uses a RDD to show that Democratic governors increase the employment of blacks relative to whites. Pettersson-Lidbom (2008) and Cervellati et al. (2017) find partisan effects in Swedish and Italian municipalities, respectively. In contrast, Ferreira and Gyourko (2009) show that at the city-level, polarization does not seem to be an issue in the U.S. Consistent with the above literature, Westwood (2017) shows that marginally elected legislators have more bipartisan rhetoric, but not votes. Together, these studies reject the notion (e.g., Frey and Schneider, 1978) that policymakers are partisan only as long as elections are not too close. Similarly, we find that full-sample estimates actually attenuate the estimated partisan differences. Unlike the studies cited above, our discontinuity is in the slope of policy rules – and not in the intercept as in standard

[6] Alesina and Sachs (1988) provide an early analysis that empirically tests a version of Alesina (1987) for the U.S. and finds support for partisanship in monetary policy.

RDD or the slope in terms of the running variables as in regression kink design (Card et al., 2016). It thereby resembles methodological work by Caetano et al. (2017) on conditional RDD. Unlike Caetano et al. (2017), our focus is not on the average conditional treatment effect, but on a slope coefficient, which here represents the partisan pass-through.

Within public finance, we build on the literature on federal transfers, surveyed in Hines and Thaler (1995) and Inman (2016). Our empirical estimates imply that policy makers have discretion over the use of federal transfers, even when they are targeted for specific purposes. With perfect compliance, one might expect a passthrough of unity. In contrast, absent frictions, policy-makers should just spend out of the annuity value of transfers and in the same proportion as they would if private incomes rose. Passthroughs differ across programs, but Hines and Thaler (1995) find a range of 25 cents to 1.06 dollars per federal dollar received in the U.S. Our point estimates for the passthrough of transfer increases under Democrats and the passthrough of IG cuts under Republicans are above unity in dollar-terms, but we cannot reject that they include unity. Averaged across both parties, our estimates are similar to the range in the literature. Recently, Leduc and Wilson (2017) estimated passthroughs in excess of two for highway spending, driven by lobbying. In a similar spirit, we relate passthroughs to politics, also addressing a short-coming of the earlier literature highlighted by Inman (2016).[7]

A large literature measures partisanship and polarization of politicians in the U.S., e.g., Andris et al. (2015), McCarty et al. (2016), and Azzimonti (2017). These studies and McCarty (2007) also correlate their measures with political or economic outcomes. However, potential endogeneity remains a concern (Gomes, 2018), acknowledged also by McCarty et al. (2016) through their emphasis on the "dance" between polarization and the economy. Mian et al. (2014) also find that polarization rises after economic crises. Krasa and Polborn (2014a) find that polarization of presidential candidates increased, based on voter surveys and a structural model. We do not provide annual measures of polarization, but we also find qualitatively that no significant differences in tax policies between parties prior to the Reagan era. Throughout, we refer with partisanship and polarization to partisanship and polarization of politicians, as opposed to that of voters or campaign contributors (see McCarty et al., 2016).

Within macroeconomics, our paper adds a new angle to the literature on time variation in fiscal multipliers. There is a debate in the literature (e.g., Auerbach and Gorodnichenko, 2012a,b; Ramey and Zubairy, 2014) whether fiscal (purchase) multipliers vary with economic conditions. Complementary to that analysis, our model implies that the (transfer) multiplier varies with two political conditions: First, the degree of partisanship matters and differences in tax policy have become significant only since the Reagan era. Second, when partisanship matters, the party affiliation of governors matters.

[7] Canova and Pappa (2006) discuss the importance of allowing dynamic heterogeneity by accounting for heterogeneous slopes across states and they estimate expenditure rules at the state and local level, focusing on the role of fiscal institutions. While they find no significant effects of fiscal institutions, they suggest that this is evidence of the ability of states to work around budgetary requirements. This also backs our notion that states have flexibility in their spending behavior.

Our finding that Republican governors tend to stimulate the state economy more by cutting taxes than their Democratic counterparts by increasing spending is in line with papers finding that tax cuts have large(r) stimulative effects. For example, Mountford and Uhlig (2009) find that tax cuts have larger multipliers than government spending increases when both are deficit-financed. Mertens and Ravn (2014) show estimate tax multipliers of two and higher and argue that studies such as Blanchard and Perotti (2002), who found roughly equal spending and tax multipliers, underestimated tax multipliers.

We also contribute to a number of papers analyzing different government spending components. Bermperoglou et al. (2017), for example, analyze the government wage bill. Oh and Reis (2012) analyze government transfers. Drautzburg and Uhlig (2015) provide separate multiplier estimates for government consumption, government investment, and government transfers to households.

3 Data

States have become increasingly important in the U.S., almost doubling the income shares of both state income taxes and state spending from 1950 to 1999 (Besley and Case, 2003). From 1980 to 2016, state and local consumption and investment spending account for about 12% of GDP, compared to about 8% for the federal government. States finance about a quarter of their expenditures with intergovernmental transfers, largely from the federal government.

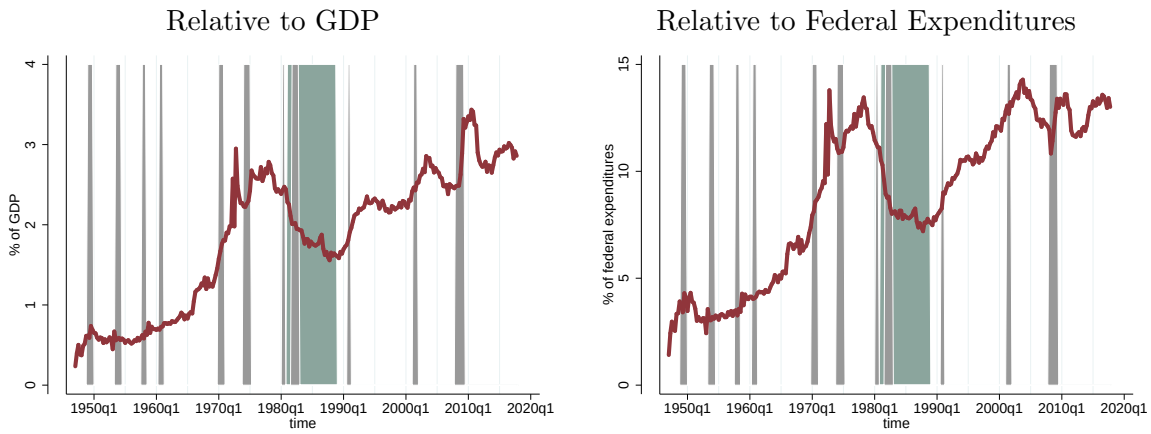
3.1 Federal Intergovernmental Transfers

Intergovernmental transfers (IG) from the federal government to state and local governments have grown in importance throughout most of the post WWII era in the U.S.[8] As Figure 1 shows, the secular upward trend was interrupted only by the policies enacted under President Reagan in the early 1980s. Federal transfers accounted for almost 3% of GDP in 2017, up from less than 0.5% in 1947. Relative to federal expenditures, the increase is five-fold, from less than 2.5% in 1947 to above 13% in 2017.

Movements in IG reflect both cyclical and discretionary policies. Data from 1980 onward, which are the main focus of our analysis, show this. The drop in IG in the early 1980s coincided with Reagan-era policies, shaded in green in the figure. The Reagan-era aside, however, IG tends to increase in recessions. Regression analysis confirms the significance of these correlations:

$$\Delta \log \frac{IG_t}{GDP_t} = \underset{[0.79]}{0.002} + \underset{[2.04]}{0.025} \times \mathbf{1}\{Recession\}_t - \underset{[2.57]}{0.019} \times \mathbf{1}\{Reagan\}_t, \qquad N = 152. \tag{3.1}$$

[8] Overall federal transfers to states differ from the grants-in-aid to state and local governments in Figure 1: (1) They exclude local governments, and (2) they encompass more than grants-in-aid. The census manual lists as examples "the reimbursement of one government by another for tuition costs, hospital care, boarding prisoners, construction of public improvements, etc.; grants in aid; payments-in-lieu-of-taxes, and the like." (U.S. Census Bureau, 2006, page 3-5f). Figure A.6 shows the annual measure for transfers to state governments over the available sample period. Except for the 1970s, federal transfers to states are higher than the overall grants-in-aid. Importantly, capital expenditures and state-run unemployment insurance numbers are excluded from NIPA. See Rider and Holdren (2005, p. III-9).



Gray shaded areas indicate NBER recession dates. The green shaded area indicates the Reagan presidency.

Figure 1: Intergovernmental transfers (Grants-in-aid to state and local governments) since 1947

Heteroskedasticity and autocorrelation robust standard errors t-statistics based on three lags in brackets. In recession quarters, IG tends to increase 2.5% relative to GDP, whereas during the Reagan era it fell by 1.9% relative to GDP. See Figure A.7 for a close-up and an alternative regression specification.

3.2 Description of main dataset

We construct a panel data set encompassing fiscal and political outcomes in U.S. States from 1963 to 2013. We collect comprehensive data on revenues and expenditures for all states from the U.S. Census Bureau's State and Local Government Finance historical database for 1958 to 2006 by fiscal year. For both expenditures and revenues, the State and Local Government Finance database provides detailed accounts for both the end use and source of financing, including purpose of intergovernmental transfers as well as type of spending. The more recent data comes from the Census' Annual Surveys of State and Local Government Finances. We do not use the preliminary estimate for 2014 because we found that preliminary estimates can be off substantially in 2007 and 2008, when the historical and contemporaneous sources overlap. We also collect GDP deflator and State GDP data from the U.S. Bureau of Economic Analysis's Regional Economic Accounts by calendar year. To merge the dataset, we line up fiscal years with the calendar years straddling the end of the previous fiscal year and the beginning of the current fiscal year, to best reflect states' contemporaneous information. Fiscal years begin in the calendar year before with the state budget allocation being set in advance for all U.S. states, despite difference in the timing of fiscal years for four states. We assign the political status of the state to be that in the first quarter of the calendar year preceding the fiscal year as it is in the middle of the budget process. We use the GDP deflator to deflate all quantities to real dollar values. Appendix A provides detailed variable definitions.

We assemble a political database including state legislature partisan affiliation, governor party and marginal victory, and state presidential vote. The state legislature data comes from Klarner

(2015). Klarner assembles this open source data set from primary sources. This database also includes a variety of budget power variables assembled by Klarner's study of legal fiscal rules. Using text recognition software, we assembled a database of gubernatorial outcomes from the Council of State Government's Book of States, which provides margin of victory and party affiliation from 1933 to date. Since the vote share can lead to ambiguous outcomes when other parties won the most vote, we manually check the election results whenever third parties are shown as having the most votes. In addition, we check all governors elected within a 5pp. margin of victory (MOV). We also collect non-electoral gubernatorial change outcomes from the National Governors Association.[9] Finally, we take state-level presidential voting records from the University of California Santa Barbara's American Presidency Project. Our final political database extends from 1958 to 2014. Our final data set spans 1963 to 2008 with full fiscal and political data. Tables A.1 and A.2 in the Appendix list the marginally elected Republican and Democratic governors by state, year, and margin of victory, along with the control of the state legislature. It is notable from the table that most states switch, as Figure A.1 in the Appendix also illustrates. For example, even states that produce landslides in some elections, such as California or Texas, had marginally elected governors from both parties.

3.3 Fiscal variable definitions

Our fiscal variables follow the definitions found in U.S. Census Bureau (2006). Our measure of government expenditures is called "Total Expenditure". The Census defines it as "includ[ing] all amounts of money paid out by a government during its fiscal year – net of recoveries and other correcting transactions – other than for retirement of debt, purchase of investment securities, extension of loans, and agency or private trust transactions." (U.S. Census Bureau, 2006, p. 5-1.) This measure is the sum of current operating expenditures, total capital outlays, total spending on assistance and subsidies, total insurance trust benefits, total interest on debt, and total intergovernmental expenditures.

We use "General Revenue" net of federal intergovernmental transfers as the main measure of revenue for our analysis. General Revenue is defined by the Census as "compris[ing] all revenue except that classified as liquor store, utility, or insurance trust revenue. The basis for this distinction is the nature of the revenue source involved, not the fund or administrative unit established to account for and control a particular activity." (U.S. Census Bureau, 2006, p. 4-3) General revenue is the sum of tax revenue, intergovernmental revenue, current charges, and miscellaneous charges. While the Census provides an alternative and larger measure called "Total Revenue" that also includes social insurance trust revenue, the Census requires unrealized gains or losses to be booked in the fiscal year that they occur, which skews the data during recessions.

To measure the constraints on fiscal policy, we also use "total debt" from the census data set.

[9] In years with a change in governor party, we assign the governor's political party to the party during the budget process in the first quarter of the previous calendar year. Unless otherwise noted, we drop state-years with independent governors – a rare occurrence, as Figure A.1 shows.

The weakness of this measure is that it is based on the face value of outstanding debt, rather than its market value. However, by focusing on the change in total debt we should limit the importance of the composition problem of debt. We also focus on debt with a maturity of at least one year which accounts for almost all debt. Our results are, however, robust to using all debt outstanding. The Census discourages using alternative measures, such as the past surplus.[10]

3.4 Sample selection

We organize our analysis according to the predominant state fiscal year definition and begin our estimation sample in the (state) fiscal year of 1983. This fiscal year is the first fiscal year that states knew the Reagan policies: Reagan assumed office in 1981 and the first new federal fiscal year in his presidency begins in September 1981. Fiscal years begin in July in most states, whereas the federal fiscal year begins in September. States could react to the 1981 federal budget during their budget deliberations for FY 1983 that take place in the first half of 1982. In our analysis, we relate the expenses in a given fiscal year to the political majorities in the previous fiscal year because of the implementation lag. Some of our results are sensitive to including the pre-Reagan years, and we analyze this time-dependence in Section 5.

We define the cutoff for a close election in terms of the percentage point difference between Republican and Democratic votes. I.e., if no votes were cast for independent candidates, a MOV of 3pp. would correspond to a 51.5% Republican vote share with the remaining 48.5% going to the Democratic candidate. This definition follows Ferreira and Gyourko (2009). Only half as many voters (plus one) have to switch to reverse the election outcome. Figure 2 shows the corresponding number of marginally elected governors by year for our baseline cutoff of a 4pp. MOV.

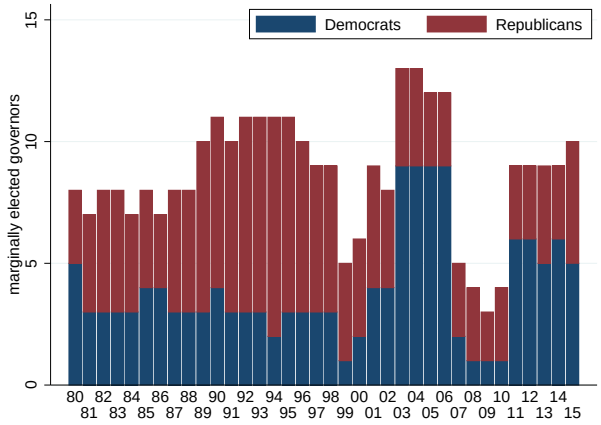


Figure 2: Democratic and Republican governors elected within a 4pp. margin of victory from calendar year 1980 to 2015.

[10] "[...] the Census Bureau statistics on government finance cannot be used as financial statements, or to measure a government's fiscal condition. For instance, the difference between a government's total revenue and total expenditure cannot be construed to be a 'surplus' or 'deficit.'" See U.S. Census Bureau (2006, p. 3-13.).

We drop states that have large sovereign wealth funds financed through severance taxes. In the literature (e.g., Conley and Dupor, 2013), it is common to remove the four smallest U.S. states, which include three of the most oil dependent states, or to control for oil prices. Instead, we focus on states that have sovereign wealth funds with explicit requirements on revenues and expenditures. For example, the Alaska Constitution mandates that at least 25% of oil revenue is deposited in its wealth fund. Such fiscal rules and the potential to use these funds to smooth expenditures or taxes may create problems for our model. We thus drop the states starting in the the year that they instituted their wealth fund: Wyoming (since 1975), Alaska (1976), and North Dakota (2009).[11]

		Main sample	Main sample with close elections			Dem=Rep
	1963-2014	1983-2014	Within 5pp.	Dem<5pp.	Rep<5pp.	p-val
Debt per capita	2121.8	2811.6	3090.5	2825.8	3290.0	1.0
Debt growth	-0.8	0.0	-0.3	-0.3	-0.3	0.9
Population	5177.1	5777.4	6284.7	5768.7	6673.8	0.7
Population growth	1.1	1.0	0.9	1.0	0.8	0.9
Expenditure growth	3.2	2.6	2.5	2.6	2.5	0.1
Income sales tax rev growth	2.9	2.1	2.5	2.4	2.6	1.0
Net general rev growth	3.0	2.2	2.6	2.7	2.6	0.4
Tax rev growth	2.6	2.0	2.5	2.4	2.5	0.8
Overall GDP growth	2.1	1.9	2.0	1.8	2.1	0.8
Private GDP growth	2.1	1.9	2.0	1.8	2.2	0.8
IG increases	5.5	5.0	4.8	4.5	5.0	0.6
IG decreases	-1.9	-1.6	-1.4	-1.5	-1.4	1.0
Observations	2439.0	1508.0	321.0	138.0	183.0	.

Population in 1,000s. Debt per capita in 2012 dollars. All other variables, except for population growth, also in real per capita terms. p-values based on standard errors clustered by state and year after removing state and year fixed effects. The 5 pp. MOV includes two observations that drop out in the presence of these fixed effects.

Table 1: Descriptive statistics: Means

Table 1 summarizes the mean of the levels and growth rates of our main economic variables. It also report p-values for tests of the Republican and Democratic difference in our main sample of close elections, after removing state and year fixed effects and clustering by state and year. While Democrats tend to govern in state-years with somewhat higher GDP, transfer, expenditure, and net general revenue levels, these differences are not statistically significant. Differences in the growth rates of the same variables are insignificant throughout. For example, overall real GDP growth is 1.8% under closely elected Democratic governors and 2.1% under closely elected Republican governors. The 0.3pp. difference is, however, insignificant with a p-value of 0.8 that takes out fixed effects and uses clustered standard errors.

[11] They are the only states to receive 20% of their revenue from severance taxes. Our main results seem robust to including these states.

4 Partisan policies post-Reagan: Empirics

Before we begin our main empirical analysis, it is worth to consider Table 1 more closely. In particular, the table shows that the average partisan differences for fiscal variables are insignificant. We therefore now analyze the potential for partisan differences conditional on covariates – and federal transfers in particular.

4.1 Empirical specification

We estimate the relationship between various outcome variables and IG transfer receipts, along with political determinants. Because the data is trending, we analyze the data in growth rates. To control for state economic and fiscal conditions we include the lagged change in long-term debt, and GDP growth. Following Besley and Case (2003), we use state and year fixed effects. This isolates the within-state variation in political outcomes and the between-state variation in intergovernmental transfers and business cycles.

Our focus is on close elections because full-sample regressions may not reveal the actual partisan behavior. Voters might "select" one party during certain economic conditions or parties might cater to voters' political preferences. In both cases, policy choices would not reflect the partisan preferences of policymakers. Policy choice could also be obscured by rent-seeking motives of politicians in non-competitive races that could lead to higher spending. To account for this potential endogeneity, we estimate budget rules for marginally-elected governors. This resembles regression discontinuity designs, which compare two groups near an arbitrary cutoff where selection into a "treatment" group is essentially random and then makes inference based on differences in outcomes for the two groups. Inference on the impact of the treatment group is then as good as using randomly assigning treatments (Lee, 2008). Similarly, we argue here that conditioning on close elections allows us to give the estimated differences between political parties a causal interpretation.

Specifically, we estimate regressions of the following form:

$$\begin{aligned}\Delta Y_{s,t} = {} & \mu_s + \nu_{r(s),t} + \alpha_r \mathbf{1}_{\{Gov_{s,t-1}=rep\}} \\ & + (\beta_{0,debt} + \beta_{r,debt}\mathbf{1}_{\{Gov_{s,t-1}=rep\}})\frac{\Delta Debt_{s,t-1}}{GDP_{s,t-1}} \\ & + (\beta_{0,gdp} + \beta_{r,gdp}\mathbf{1}_{\{Gov_{s,t-1}=rep\}})\Delta \ln(GDP_{s,t-1}) \\ & + (\gamma_{0,+} + \gamma_{r,+}\mathbf{1}_{\{Gov_{s,t-1}=rep\}})\Delta \ln IG^{+}_{s,t} + (\gamma_{0,-} + \gamma_{r,-}\mathbf{1}_{\{Gov_{s,t-1}=rep\}})\Delta \ln IG^{-}_{s,t} \end{aligned} \tag{4.1}$$

where $Y_{s,t}$ is (log) expenditures in our main results and a measure of (log) revenue or economic activity in our extensions. $r(s)$ denotes the Census region, so that ν allows for Region×year fixed effects. Here, $\Delta \ln IG^{+}_{s,t} \equiv \max\{0, \Delta \ln IG_{s,t}\}$ and $\Delta \ln IG^{-}_{s,t} \equiv \min\{0, \Delta \ln IG_{s,t}\}$.

Underlying our specification is the idea that if governors were randomly assigned any errors from potentially endogenous transfers then any bias would average to zero across Democratic and Republicans. Formally, let $Y = XD\beta + \epsilon$, where all variables are zero mean. Moreover, X is

correlated with ϵ, $\mathbb{E}[X\epsilon] \neq 0$, while $D \perp\!\!\!\perp (\epsilon, X)$. Hence, $\text{Cov}[\epsilon, XD] = \mathbb{E}[\epsilon XD] = \mathbb{E}[\mathbb{E}[\epsilon X|D]D] = \mathbb{E}[\mathbb{E}[\epsilon X]D] = \mathbb{E}[\epsilon X] \times \mathbb{E}[D] = 0$, given independence. Then $\frac{\text{Cov}[Y,XD]}{\text{Var}[XD]} = \beta + \frac{\text{Cov}[\epsilon,XD]}{\text{Var}[XD]} = \beta$. This argument relies on the sample restriction. Away from the threshold, the assumption that $D \perp\!\!\!\perp (\epsilon, X)$ is violated. In a multivariate setting with $Y = XD\beta + \mathbf{W}'\gamma + \epsilon$, the corresponding assumption is that $D \perp\!\!\!\perp (\epsilon, X, \mathbf{W})$. While we cannot test our assumption in terms of ϵ, we can test the unconditional correlations of X and D. Indeed, as our discussion of Table 1 highlights, there are no significant partisan differences in our main model variables.

We address additional concerns about our identification scheme by using a different set of fixed effects. Specifically, one concern for identification is that even though the governor dummy is independent, the federal government could channel transfers as a function of the governors' party and the conditions in the state. To mitigate these concerns we also estimate versions of our regression that have party×state and party×year fixed effects. Intuitively, allowing for party×year fixed effects allays concerns that marginally elected governors of one party might have systematically different IG flows. For example, Republican governors could all decline to participate in a new federal program, such as Medicaid expansion, and the independence assumption would then be violated. Party×year fixed effects would account for that and only use residual variation for identification. In this case our regression setup is that of Caetano et al. (2017). They formally develop a RDD when the average effect across the threshold – α_r in (4.1) absent controls – is zero.

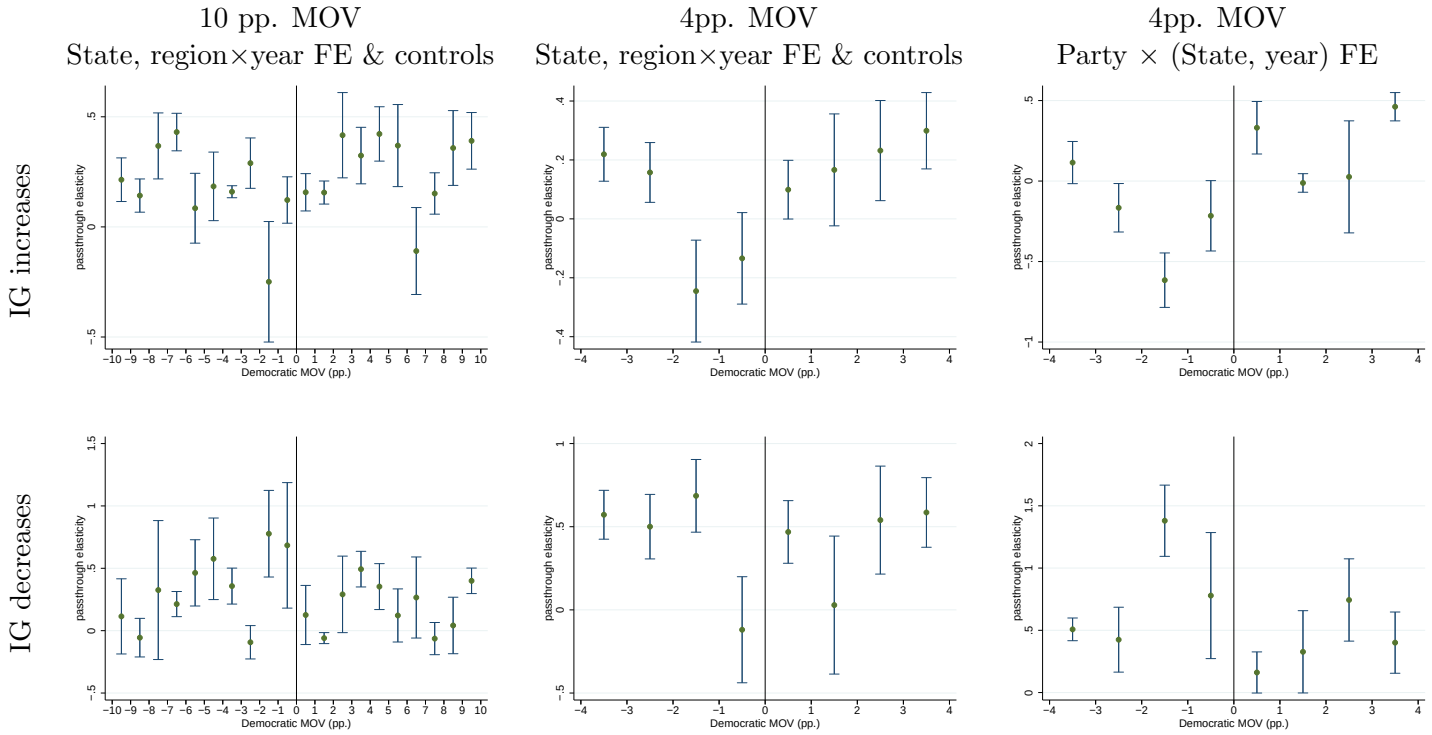
4.2 Expenditure side

We begin our analysis with the expenditure side by illustrating our identification scheme. Specifically, we first estimate (4.1) in two steps: (1) We remove fixed effects and other controls from total expenditure growth and IG up to a MOV of either four or ten percentage points, and (2) we estimate the slopes of the residual expenditure growth on the residual of IG_+ and IG_- for each one percentage point MOV bin. Figure 3 shows the resulting slope estimates.

Marginally elected Democratic governors have a higher pass-through of intergovernmental transfer increases to spending than marginally elected Republicans. In contrast, for large margins of victory, the pass-through is not systematically different. The differences are most pronounced within margins of victory of two percentage points or less, but vary somewhat with the regression specification. For example, with a 4pp. MOV cutoff, region-year fixed effects, and control variables, the pass-through elasticity averages -0.19 for Republican governors elected with a MOV no larger than 2pp., but averages about 0.13 for Democrats elected within the same margin. Given the ratio of IG to expenditures of about 0.25, this 0.32 difference in these naive elasticity estimates corresponds roughly to a difference in dollar-to-dollar pass-through of 1.28 (0.32×4, 4 being the ratio of total expenditures to federal IG revenue): If a Democrat were to spend, say 1.3 for each dollar transferred from the Federal government, this estimate would imply that the Republican counterpart spends virtually none of it. When we use the same specification, but estimate the controls in a 10pp MOV sample, the difference in elasticities is 0.22, while it rises to 0.58 when

using a 4pp. MOV to estimate party-year and party-state fixed effects.[12]

For cuts to intergovernmental transfers we have suggestive evidence that the pass-through to spending (cuts) is higher for Republican governors. Focusing on governors elected within two percentage points, we find an elasticity that is 0.70 higher for Republicans (0.73 vs 0.03) when we estimate the controls in a 10pp MOV sample, but a difference of only 0.03 when estimated in the 4pp. MOV sample with the same controls. With the same 4pp MOV sample, but after removing (only) party-year and party-state fixed effects, we find an elasticity that is 0.84 higher for Republican governors. While the magnitude is thus uncertain, the results suggest, qualitatively, that Republican governors cut state expenditures relatively more than Democrats do in response to cuts in federal transfers.



We construct the plots by first removing fixed effects and, if applicable, controls in the full sample 10pp and 4pp samples. We then estimate slopes for 1pp. bins. The figures show the estimated slopes and heteroskedasticity-robust ± one standard error. The standard errors are meant to be suggestive only. When we report direct estimates of (4.1), we quantify the uncertainty coming from the controls and fixed effects and clusters standard errors.

Figure 3: Illustrating our regression discontinuity in slopes: Republican Governors pass less of IG increases on to spending and pass more of IG decreases on to spending cuts.

While suggestive, the results in Figure 3 do not show proper standard errors and our heuristic discussion neglects that the slopes in some bins are more precisely estimated than in others. To aggregate slopes and properly compute standard errors, we now estimate (4.1) directly. Our inference

[12]Here, we find no unconditional difference on average between Republican and Democratic governors regarding expenditure growth. Figure B.10 shows the analogous figure for the intercept term, replacing only the third panel with one without party interactions since party interactions render the intercept unidentified.

uses standard errors clustered by state and year, implemented using `reghdfe` in `Stata` (Correia, 2016).

	(1) 100 pp.	(2) 6 pp.	(3) 5 pp.	(4) 4 pp.	(5) 3 pp.
Debt	0.005	-0.031	-0.026	-0.058	0.007
	(0.30)	(-1.20)	(-0.64)	(-0.96)	(0.14)
GDP growth	0.173***	0.069	0.169	0.169	0.151
	(3.34)	(0.47)	(0.83)	(0.79)	(0.46)
Rep x Debt	0.011	0.031	0.038	0.042	0.044
	(0.54)	(0.72)	(0.96)	(0.63)	(0.82)
Rep x Growth	0.008	-0.334**	-0.261	-0.001	0.405
	(0.13)	(-2.54)	(-1.22)	(-0.00)	(1.20)
Republican Gov.	0.002	0.015**	0.015**	0.025**	0.000
	(0.70)	(2.17)	(2.13)	(2.15)	(.)
IG incr.	0.268***	0.378***	0.339***	0.301***	0.316**
	(6.92)	(7.58)	(11.68)	(5.35)	(2.16)
IG decr.	0.086***	0.032	0.201	0.209	0.024
	(3.04)	(0.37)	(1.61)	(1.57)	(0.12)
Rep x IG incr.	-0.084**	-0.223***	-0.182**	-0.275**	-0.405**
	(-2.36)	(-3.40)	(-2.14)	(-2.67)	(-2.41)
Rep x IG decr.	0.197***	0.314**	0.298**	0.263**	0.231
	(5.12)	(2.47)	(2.55)	(2.20)	(1.43)
Expenditure/IG Rev.	4.01	4.11	4.12	4.08	4.15
R-squared	0.51	0.67	0.75	0.79	0.79
R-sq, within	0.19	0.24	0.30	0.25	0.20
Observations	1499	374	300	239	119
States	48	45	43	40	28
Years	32	32	31	31	27

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$. To compute a dollar-to-dollar pass-through, multiply the elasticity by the Expenditure/IG revenue ratio.

Table 2: Expenditure growth: State FE, Region x Year FE, with controls

Estimating the expenditure policy rule directly using data from closely elected governors confirms the takeaway from the graphical illustration: Democratic governors have a higher pass-through of transfer increases to spending, while Republican governors have a higher pass-through of transfer cuts to spending cuts. Table 2 contains the results for the model in (4.1) estimated from 1983 to 2014. The different columns represent results for MOV varying cutoffs. Column (1), the 100pp. MOV, corresponds to the full sample. Before turning to the partisan differences, we discuss the baseline coefficients first, starting with the full sample. In the full sample we find that governors of either party have higher expenditure growth when lagged GDP growth is higher, but lagged debt growth is not a significant determinant of spending. Transfer increases are associated with significantly higher spending: The pass-through elasticity is 0.27, corresponding to a dollar pass-through of 1.08 (0.27×4), at the high end of the results surveyed by Hines and Thaler (1995), but significantly below the 2.40 pass-through for highway spending in Leduc and Wilson (2017). The

	(1)	(2)	(3)	(4)	(5)	(6)
IG incr.	0.381***	0.337***	0.307***	0.367***	0.322***	0.301***
	(7.57)	(4.85)	(4.91)	(7.58)	(4.58)	(5.35)
IG decr.	0.090	0.089	0.226*	0.092	0.056	0.209
	(0.93)	(0.54)	(1.80)	(0.94)	(0.34)	(1.57)
Republican Gov.	0.016*		0.028***	0.016		0.025**
	(1.76)		(2.89)	(1.46)		(2.15)
Rep x IG incr.	-0.449***	-0.428***	-0.291**	-0.434***	-0.407***	-0.275**
	(-5.40)	(-3.34)	(-2.72)	(-4.69)	(-3.33)	(-2.67)
Rep x IG decr.	0.362***	0.461**	0.263**	0.357***	0.499**	0.263**
	(3.38)	(2.27)	(2.21)	(3.18)	(2.50)	(2.20)
R-squared	0.64	0.71	0.78	0.65	0.72	0.79
R-sq, within	0.24	0.20	0.24	0.26	0.22	0.25
Observations	266	259	239	266	259	239
States	41	41	40	41	41	40
Years	32	32	31	32	32	31
StateFE	Yes	By party	Yes	Yes	By party	Yes
YearFE	Yes	By party	By region	Yes	By party	By region
Controls	No	No	No	Yes	Yes	Yes

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$. To compute a dollar-to-dollar pass-through, multiply the elasticity by the Expenditure/IG revenue ratio.

Table 3: Expenditure growth: Various specifications, 4pp MOV

pass-through for transfer cuts is lower, with an elasticity of only 0.09. This lower pass-through is consistent with states smoothing through transfer cuts that are more transient than transfer increases when transfer rise secularly. The full-sample estimates also show that Republican governors have a lower pass-through of spending increases and a much higher pass-through of transfer cuts. For tighter margins of victory, we find, however, that the difference in the pass-through elasticity increases almost monotonely from -0.08 in the full sample to -0.41 for a 3pp. MOV for transfer increases. For transfer cuts, we find a roughly stable difference, with Republican governors having a pass-through elasticity to spending cuts that is 0.20 higher in the full sample, 0.23 in the 3pp. MOV sample (albeit insignificantly), and somewhat higher elasticities for intermediate cutoffs that are all statistically significant.

Different channels could bias the transfer pass-through down in the full sample and explain why conditioning on close elections increases the pass-through difference. The downward bias could arise if higher spending causes higher transfers, for example through matching grants, this would bias up the full sample estimation, but not the coefficient estimate interacted with the quasi-randomly assigned gubernatorial variable.[13] A bias could also arise if governors are more partisan in contested elections, for example to turn out the vote, while governors with a large margin of victory are opportunistic and engage in rent-seeking by spending less carefully independent of their party

[13]Indeed, unconditionally, the expenditure growth and IG growth have a positive and significant correlation of 0.40 (t=8.3), after removing state and region-year fixed effects.

affiliation. Together, we find it plausible that the estimated differences in the spending elasticities are attenuated in the full sample.

Unlike in the binned regressions underlying the graphical illustration, we do find significantly higher spending growth intercepts for Republican governors at intermediate margins of victory. For example, at the 4pp. MOV, it implies that a Republican governor has, unconditionally, a 2.5% higher expenditure growth than a Democratic governor. This effect is zero at the 3pp. MOV and 1.5% at the 5pp. MOV. We can interpret this estimate as Republican governors smoothing transfer growth by not responding much to federal transfers, while Democrats do. To see this, note that at the 4pp. MOV the combined pass-through elasticity for a Republican governor for transfer increases is only 0.026 ($= 0.301 - 0.275$), or a dollar pass-through of 10 cents on the dollar. In contrast, for Democrats the corresponding estimate is of 1.20 for each dollar, given the elasticity of 0.301. This smoothing of expenditure growth is in line with the somewhat smaller volatility of Republican expenditure growth once we account for the same fixed effects (3.4% compared to 3.9% for Democrats). However, since the intercept is insignificant in other specifications and cutoffs (see Tables B.5 and B.6), we do not pursue this aspect of our estimates further.

Our conclusions about the different spending elasticities are robust across specifications. Table 3 shows that at the 4pp. MOV our findings hold whether or not we include controls, or whether we allow state and year fixed effects to vary by state, or whether we only have state and year fixed effects. In addition, the result also holds for transfer increases when we relate the change to spending to the change in transfers over longer horizons; see Table 4. For transfer cuts, which are less common at longer horizons, the results hold up to the three-year horizon, but are insignificant at the two-year horizon.

Looking across various types of expenditures, our results suggest that most types of expenditures are adjusted. Tables B.7 to B.10 report estimates at the 4pp. MOV for various specifications for capital expenditures, transfers to local governments, transfers to households, and other expenditures, such as operating expenditures. Except for transfers to households, which comprise about one eights of the total (Figure A.5), some regression specification indicates that each expenditure category is adjusted, with the strongest results for the adjustment of transfers to local governments. We conclude that the expenditure cuts can largely be viewed as across the board cuts.

4.3 Revenue side

To understand what Republican governors do with the federal transfers, we now turn to the revenue side. We first estimate the overall revenue response and then look into various components of general revenue. General revenue excludes insurance trust revenue that reflects valuation effects in insurance trust funds. We also subtract federal intergovernmental transfers from general revenue to focus on policy choices. Still, general revenue is endogenous and plausibly less directly influenced by policy choices so that we quickly turn to tax revenue growth. Figures A.2 to A.4 break down the components of total revenue, general revenue shares, and tax revenue.

	(1) 1-year	(2) 2-year	(3) 3-year	(4) 4-year
Republican Gov.	0.028***	0.032**	0.052***	0.045**
	(2.89)	(2.18)	(2.91)	(2.68)
IG incr.	0.307***	0.265***	0.278***	0.330***
	(4.91)	(5.77)	(3.43)	(5.10)
IG decr.	0.226*	0.165	-0.016	0.000
	(1.80)	(1.50)	(-0.22)	(0.01)
Rep x IG incr.	-0.291**	-0.215	-0.277**	-0.199**
	(-2.72)	(-1.65)	(-2.07)	(-2.12)
Rep x IG decr.	0.263**	0.164	0.625***	-0.046
	(2.21)	(1.26)	(3.70)	(-0.25)
R-squared	0.78	0.84	0.87	0.88
R-sq, within	0.24	0.18	0.18	0.21
Observations	239	239	239	239
States	40	40	40	40
Years	31	31	31	31
StateFE	Yes	Yes	Yes	Yes
YearFE	By region	By region	By region	By region
Controls	No	No	No	No

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$. To compute a dollar-to-dollar pass-through, multiply the elasticity by the Expenditure/IG revenue ratio.

Table 4: Expenditure growth: State FE, Region x Year FE, no controls. 4 pp MOV.

	Net general revenue		Tax revenue		Income & sales tax	
	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.
Debt	0.029	0.047	0.015	0.090	0.034	0.094
	(1.46)	(0.85)	(0.68)	(1.26)	(1.42)	(1.38)
GDP growth	0.364***	-0.016	0.510***	-0.153	0.371***	-0.197
	(4.78)	(-0.07)	(5.69)	(-0.70)	(4.90)	(-0.92)
Rep x Debt	0.018	-0.063	0.015	-0.095	-0.001	-0.063
	(0.59)	(-1.05)	(0.47)	(-1.32)	(-0.03)	(-0.87)
Rep x Growth	-0.031	0.333	-0.007	0.539*	0.024	0.565**
	(-0.55)	(1.27)	(-0.09)	(2.00)	(0.26)	(2.40)
Republican Gov.	0.003	0.005	-0.002	0.013	-0.003	0.017
	(0.80)	(0.42)	(-0.26)	(0.93)	(-0.49)	(1.03)
IG incr.	0.074	0.169**	0.047	0.201***	0.039	0.224***
	(1.63)	(2.50)	(1.01)	(3.40)	(0.74)	(3.22)
IG decr.	-0.030	-0.189	-0.080*	-0.276*	-0.101**	-0.282
	(-1.02)	(-1.09)	(-1.86)	(-1.75)	(-2.27)	(-1.64)
Rep x IG incr.	-0.026	-0.174	0.059	-0.220*	0.088	-0.234*
	(-0.52)	(-1.63)	(0.80)	(-1.73)	(1.07)	(-1.74)
Rep x IG decr.	0.018	0.293*	0.045	0.414**	0.053	0.490**
	(0.38)	(1.94)	(0.54)	(2.17)	(0.58)	(2.41)
R-squared	0.48	0.77	0.49	0.79	0.51	0.79
R-sq, within	0.05	0.09	0.07	0.13	0.05	0.15
Observations	1499	239	1499	239	1499	239
States	48	40	48	40	48	40
Years	32	31	32	31	32	31

All regressions include state and year fixed effects. *t*-statistics based on standard errors clustered by state and year.

Table 5: Growth of general revenue components: State FE, Region x Year FE, with controls

Table 5 shows that tax revenue grows less rapidly under Republican governors in the presence of transfer increases. While in the full sample we find no significant determinant of any of the three revenue components here, at the 4pp. margin we find that revenue grows more quickly after transfer increases. For example, the elasticity of tax revenue with respect to transfer increases is 0.201. This could be due to more economic activity when expenditures grow faster, but also due to the need to raise revenue to accommodate the baseline expenditure pass-through above one, which we also estimate. The partisan difference in elasticities is, however, about the same size as the elasticity itself, with -0.220. The relatively lower tax revenue growth is consistent with the relatively lower expenditure growth. While the effect is insignificant for net general revenue, it also holds for the growth of income and sales tax revenue, which together account for about 80% of tax revenue.

	Average individual income tax rate		Current marginal tax rate		Future marginal tax rate	
	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.
Debt	-0.019	0.022	0.027	2.664**	0.235	0.297
	(-0.16)	(0.10)	(0.05)	(2.24)	(0.66)	(0.32)
GDP growth	0.446	0.679	-1.537	6.512	-2.211	6.741
	(0.73)	(0.73)	(-0.73)	(1.14)	(-1.13)	(1.09)
Rep x Debt	0.135	-0.393	0.220	-5.879***	-0.001	-3.159***
	(0.78)	(-1.55)	(0.28)	(-3.14)	(-0.00)	(-2.95)
Rep x Growth	0.311	-1.263	0.495	-22.721**	1.812	-22.800**
	(0.43)	(-0.87)	(0.19)	(-2.17)	(0.61)	(-2.28)
Republican Gov.	0.009	-0.081	0.047	-0.422	0.067	-0.209
	(0.21)	(-1.15)	(0.30)	(-0.78)	(0.42)	(-0.45)
IG incr.	-0.319	0.592	-0.183	3.115**	-0.459	2.735*
	(-1.64)	(1.54)	(-0.22)	(2.11)	(-0.54)	(1.93)
IG decr.	0.088	-0.619	-2.450*	-4.504**	-2.470*	-2.356
	(0.23)	(-0.89)	(-1.91)	(-2.31)	(-1.98)	(-1.26)
Rep x IG incr.	0.141	-1.389**	0.453	-6.269*	-0.075	-6.552*
	(0.62)	(-2.34)	(0.49)	(-1.94)	(-0.11)	(-1.99)
Rep x IG decr.	0.227	0.508	2.357	-4.362	3.754**	-0.550
	(0.46)	(0.77)	(1.66)	(-1.56)	(2.33)	(-0.19)
R-squared	0.94	0.98	0.90	0.96	0.91	0.97
R-sq, within	0.01	0.14	0.01	0.33	0.01	0.25
Observations	1499	239	1499	239	1499	239
States	48	40	48	40	48	40
Years	32	31	32	31	32	31

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table 6: Tax rates: State FE, Region x Year FE, with controls

When transfers are cut, tax revenue also grows more quickly under Democratic governors than under Republican governors. The difference is in elasticities is 0.414 and 0.490. While we only give these difference a causal interpretation, taking the baseline response to transfer cuts points to a

coherent narrative: Our expenditure growth estimates suggest that Democratic governors do not cut spending in response to transfer cuts. Consistent with this, the estimated elasticity of -0.276 in Table 5 for tax revenue growth implies Democratic governors raise overall tax revenue under tax cuts. In contrast, the combined Republican point estimate of 0.138 (-0.276+0.414) implies that Republicans may even lower tax revenue growth when transfer cuts happen.

We also find suggestive evidence of partisan difference in tax rates (Table 6). First, we calculate an average individual income tax rate as the individual income tax revenue relative to state GDP, averaged across the two calendar years straddling the fiscal year. Second, we use the maximum state marginal tax rates on wage income from the NBER TAXSIM database. In both cases, we find evidence of a partisan difference in the response to transfer increases. When transfer growth is one percent higher, Republican governors have, on average, an average personal income tax rate that is 0.13pp. lower than in the Democratic states. The corresponding number for the (statutory) top marginal tax rate is 0.63pp.

4.4 Private sector activity

We also find that the partisan differences in fiscal policy also affect the private sector, though our results are somewhat noisy. First, we look at real per capita GDP growth over the calendar year in the private sector in Table 7. GDP growth in the calendar year straddling the first half of the fiscal year is flat. With a delay, however, a partisan difference emerges that implies that the Republican use of federal transfers spurs private sector activity relatively more. Both the profit and the compensation components of GDP rise, as we document in the appendix (Table B.14). Second, we find the opposite effect for the employment relative to population in Table 8. This is consistent with Democrats stimulating short-run activity relatively more, but Republican policies stimulating private sector activity relatively more with a delay.

	Current $(t-\frac{1}{2})$ private GDP		Future $(t+\frac{1}{2})$ private GDP		Future $(t+\frac{1}{2})$ overall GDP	
	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.
Debt	-0.000	-0.044*	-0.022	-0.042	-0.017	-0.032
	(-0.02)	(-1.80)	(-1.02)	(-1.30)	(-0.92)	(-1.07)
GDP growth	0.262***	-0.180	0.174***	0.043	0.156***	0.018
	(4.39)	(-1.09)	(2.85)	(0.28)	(3.04)	(0.15)
Rep x Debt	-0.000	0.011	0.023	0.025	0.021	0.020
	(-0.02)	(0.38)	(0.80)	(0.63)	(0.87)	(0.55)
Rep x Growth	-0.034	0.301**	-0.019	-0.040	-0.011	-0.022
	(-0.58)	(2.07)	(-0.56)	(-0.26)	(-0.38)	(-0.17)
Republican Gov.	0.004	-0.005	0.000	-0.009	-0.000	-0.006
	(1.44)	(-0.55)	(0.04)	(-0.90)	(-0.04)	(-0.74)
IG incr.	-0.000	-0.061	0.020*	-0.142**	0.014	-0.123**
	(-0.00)	(-0.65)	(1.70)	(-2.66)	(1.30)	(-2.43)
IG decr.	-0.020	0.045	-0.054***	0.176***	-0.045**	0.188***
	(-0.68)	(0.57)	(-2.76)	(3.10)	(-2.67)	(3.63)
Rep x IG incr.	-0.032	0.002	-0.000	0.152**	0.001	0.122**
	(-0.96)	(0.02)	(-0.02)	(2.71)	(0.04)	(2.26)
Rep x IG decr.	0.061	0.044	0.029	-0.119**	0.026	-0.125**
	(1.56)	(0.79)	(1.33)	(-2.32)	(1.34)	(-2.36)
R-squared	0.50	0.81	0.50	0.78	0.50	0.78
R-sq, within	0.05	0.09	0.03	0.10	0.03	0.10
Observations	1499	239	1499	239	1499	239
States	48	40	48	40	48	40
Years	32	31	32	31	32	31

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table 7: Per capita real GDP growth: State FE, Region x Year FE, with controls

	Current $(t-\frac{1}{2})$ total employment		Future $(t+\frac{1}{2})$ total employment		Current $(t-\frac{1}{2})$ public employment	
	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.
Debt	-0.003	0.003	0.003	-0.003	0.024	0.025
	(-1.26)	(0.23)	(0.43)	(-0.23)	(1.21)	(0.46)
GDP growth	0.177***	0.111	0.104***	0.015	0.019	-0.074
	(7.41)	(1.52)	(4.39)	(0.25)	(0.24)	(-0.23)
Rep x Debt	0.002	0.006	0.001	-0.008	-0.015	0.023
	(0.26)	(0.42)	(0.15)	(-0.50)	(-0.58)	(0.28)
Rep x Growth	-0.037**	0.103	-0.015	0.140*	-0.049	-0.342
	(-2.11)	(1.39)	(-0.97)	(2.02)	(-0.58)	(-0.88)
Republican Gov.	0.002***	0.000	0.001	-0.006*	0.002	0.028*
	(3.12)	(0.06)	(0.53)	(-1.81)	(0.32)	(2.03)
IG incr.	0.018**	0.076	-0.004	-0.051	0.016	0.157
	(2.48)	(1.66)	(-0.74)	(-1.33)	(0.68)	(1.06)
IG decr.	-0.001	-0.015	-0.009	0.025	-0.107	-0.116
	(-0.14)	(-0.62)	(-0.84)	(1.13)	(-1.65)	(-0.80)
Rep x IG incr.	-0.025**	-0.097**	0.004	0.032	-0.009	-0.039
	(-2.35)	(-2.30)	(0.50)	(0.82)	(-0.18)	(-0.21)
Rep x IG decr.	0.004	0.018	0.010	-0.015	0.069	0.005
	(0.32)	(0.99)	(0.76)	(-0.81)	(1.18)	(0.04)
R-squared	0.84	0.89	0.80	0.90	0.43	0.74
R-sq, within	0.19	0.27	0.07	0.14	0.00	0.04
Observations	1499	239	1499	239	1499	239
States	48	40	48	40	48	40
Years	32	31	32	31	32	31

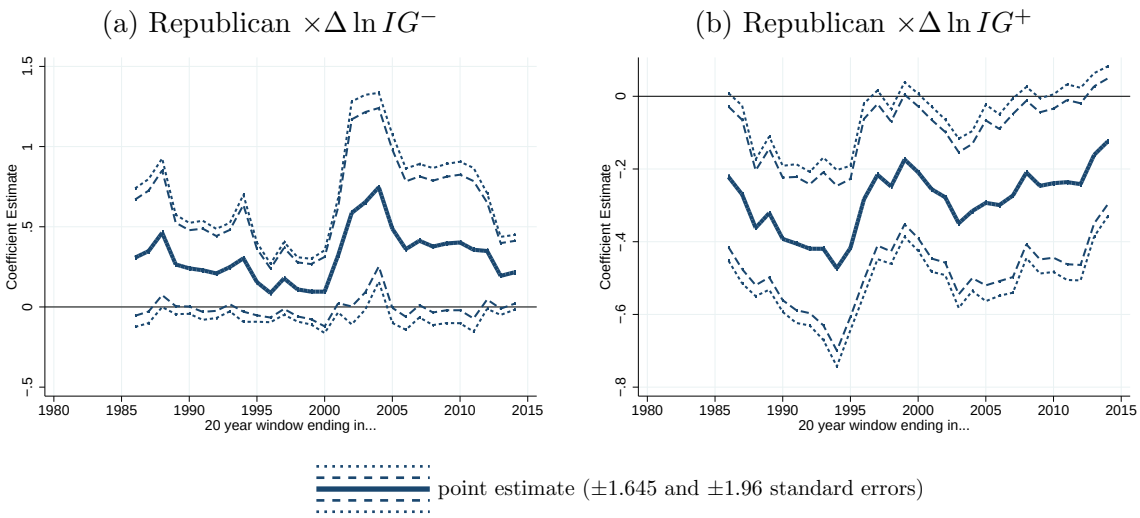
t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table 8: Employment-to-population ratio growth: State FE, Region x Year FE, with controls

5 Time-variation in partisan policies

We have now established that partisanship has significant consequences for fiscal policy in the post-Reagan era. Have the partisan effects varied over time? We deliberately chose the Reagan era as our starting point and we now discuss how our results change. In short, we find that partisan differences in tax policy were much more benign and perhaps insignificant in earlier years. This correlates with the sharp acceleration in measured partisanship, e.g., based on roll-call votes in the U.S. house (McCarty et al., 2016, Figure 1.3).

To summarize the time variation, we focus on the coefficient estimates from our baseline regression, namely the difference in the elasticity of fiscal policy outcomes with respect to changes in federal transfers. We introduce time-variation by estimating a rolling window version of (4.1) with fixed 20 year windows, ending between 1986 and 2014. Figure 4 and 5 show the estimated partisan differences in elasticities on transfer cuts and increases for expenditure growth and taxes. We show the point estimate along with confidence intervals.

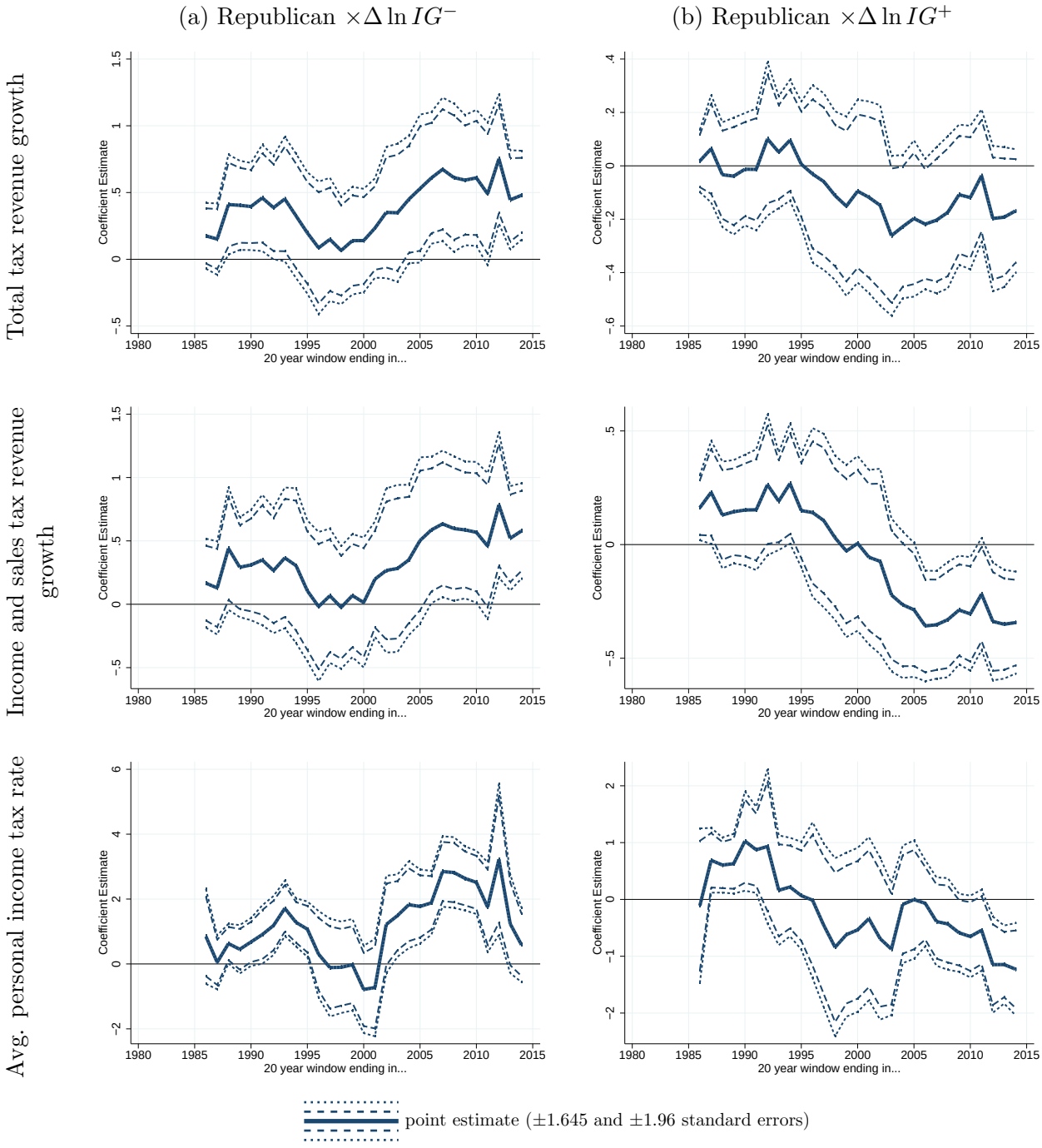


Coefficient estimates based on (4.1) estimated over a 20 year window ending in the year shown. All estimates are based on 5pp. margin of victory cutoffs . Standard errors based on standard errors clustered by state and year.

Figure 4: Time-variation in fiscal policy elasticities between Republican and Democratic governors: Total expenditure growth.

Throughout our sample period, we find evidence for a smaller pass-through of federal transfer to state spending under Republican governors. Figure 4(b) shows the corresponding pass-through elasticity for transfer increases. While the point estimates vary, qualitatively there is little change through most of our sample and there is some indication the lower spending pass-through may have eased in recent years. The point estimates suggest, in contrast, that the partisan difference in response to transfer cuts in Figure 4(a) may have risen since the Reagan era, i.e., in samples ending in 2002. Given the wide confidence intervals, we treat the partisan differences on the expenditure side as qualitatively constant over time.

Partisan differences in tax policies, in contrast, seem to have increased since the Reagan era, according to our results. Figure 5 shows the coefficient estimates for overall tax revenue growth, income and sales tax revenue growth, and our calculated average personal income tax rate. Here we find evidence that the lower tax growth under Republican governors has strengthened or emerged in recent years. This holds true both for transfer cuts and increases. Beginning with the response to transfer increases, the point estimates for all three tax measures show a decrease in magnitude relative to the early sample period. For income and sales tax growth and the calculated average personal tax rate the differences to earlier sample periods are statistically significant, as the non-overlapping (pointwise) confidence intervals indicate. While the results for transfer cuts are noisier, they also show qualitatively a significantly lower tax increase under Republican governors than under Democratic governors at the end of our sample that does not hold in samples that include the late 1970s and early 1980s or the early 1960s. We conclude that the partisan tax policy differences have varied over time and are now more pronounced.



Coefficient estimates based on (4.1) estimated over a 20 year window ending in the year shown. All estimates are based on 5pp. margin of victory cutoffs . Standard errors based on standard errors clustered by state and year.

Figure 5: Time-variation in fiscal policy elasticities between Republican and Democratic governors: Tax revenues.

6 Model

We build on the model of a monetary union with complete markets and local capital from Nakamura and Steinsson (2014). The model is a two-region version of a standard New-Keynesian model with common monetary policy and a common federal government. In ongoing work, we investigate a version of the model with perfect insurance against idiosyncratic shocks, but only a risk-free nominal one-period bond is available for trade between the regions.[14] Since our focus is on the political friction rather than the consumption response of households as in Dupor et al. (2018), we also generate a role for transfers work by modeling financially constrained households as exogenously constrained following Galí et al. (2007).

6.1 Environment

State governments. State governments provide public services to households and public infrastructure to firms.[15] State governments also levy income taxes, but are able to accumulate surpluses (or deficits) to smooth taxes.

We summarize partisanship through a single parameter: The pass-through of federal transfers to state spending. ψ_{IG} is the pass-through of the home governor, while ψ^*_{IG} is the pass-through of the representative other governor. State spending is also partly given by an exogenous process, leading us to the following representation of state spending:

$$G_{st,t} = \psi_{IG} IG_t + G^x_{st,t}$$
$$G^x_{st,t} = \mu_{G,st} + \rho_{st,g} G^x_{st,t-1} + \omega_{st,g} \epsilon^x_{st,t}$$

Motivated by our estimates that most spending components adjust to changes in transfers, we assume that states spend a fraction $1-\phi$ on public services. These may affect the households' flow utility. States invest the remaining fraction ϕ of overall spending in infrastructure:

$$K_{st,t} = (1-\delta_G) K_{st,t-1} + \phi G_{st,t}.$$

States adjust labor taxes to finance the part of the budget not covered by federal transfers and, potentially, past surpluses:

$$(1-\gamma^s)(P_t G_{st,t} - IG_t - \psi_{G,SP} R^n_{t-1} SP_{t-1}) = \tau_{st,t} W_t N_t.$$

$\psi_{G,SP} = 1$ implies that past surpluses go fully towards offsetting current expenses, whereas values

[14] Complete markets may matter in the presence of local taxes, see Farhi and Werning (2016).

[15] States can partly provide services or infrastructure through funding lower level governments such as school districts and municipal governments. In addition to services and infrastructure, states may also transfer funds to households, either directly or indirectly.

of $\psi_{G,SP} < 1$ imply some smoothing of surpluses even if otherwise labor taxes are fully adjusted period by period ($\gamma^s = 1$). The remainder of the budget is financed through changes in the surplus:

$$SP_t + \bar{T} + \gamma^s IG = \gamma^s P_t G_{st,t} + (\gamma^s \psi_{G,SP} + (1 - \psi_{G,SP}))R^n_{t-1} SP_{t-1}.$$

Federal government. The federal government levies lump-sum and distortionary taxes to finance federal government consumption and to provide intergovernmental transfers to states. Nominal per capita transfers are equal to IG_t in each region.

For simplicity, federal transfers to the states are exogenous:

$$IG_t = \rho_{IG} IG_{t-1} + \sigma_{IG} \epsilon_{IG,t}.$$

Purchases equal real per capita amounts $G^f_{Ht} = G^f_{Ft} = G^f_t$ per region (exogenous).

Similar to state governments, labor income taxes finance a fraction of the budget every period:

$$(1 - \gamma^f)(nP_{Ht}G_{Ht} + (1 - n)P_{Ft}G_{Ft} + IG_t) = \tau^f_t \int_0^1 W_t(x)L_t(x)dx.$$

The federal government finances the remaining fraction γ^f of expenditures via lump-sum taxes.

Households. Households value private consumption, state consumption, and leisure. Their labor income is subject to a linear income tax. In this version of our model, markets are complete.

Lifetime utility is given by:

$$\begin{aligned} V_t &= \mathbb{E}_t \sum_{s=0}^{\infty} \beta^s U(C_{t+s}, G_{st,t+s}, N_{t+s}) \\ C_t &= \left(\phi_H^{\frac{1}{\eta}} C_{Ht}^{1-\frac{1}{\eta}} + (1 - \phi_H)^{\frac{1}{\eta}} C_{Ft}^{1-\frac{1}{\eta}} \right)^{\frac{\eta}{\eta-1}} \\ C_{Jt} &= \left(\int_0^1 c_{jt}^{1-\frac{1}{\theta}} dj \right)^{\frac{\theta}{\theta-1}}, \quad (J, j) \in \{(H, h), (F, f)\} \end{aligned}$$

Here, C_t is a CES aggregate of consumption from the home region C_{Ht} and the foreign region C_{Ft}. These are, in turn, also CES aggregates of individual varieties.

Households' felicity function is balanced-growth consistent and implies a constant Frisch-elasticity ϵ_ν, following Trabandt and Uhlig (2011):

$$U(C, G_{st}, N) = \frac{\left(C_t^{1-\frac{1}{\lambda}} + \kappa_G ((1 - \phi) G_{st,t})^{1-\frac{1}{\lambda}} \right)^{\frac{\lambda}{\lambda-1}(1-\frac{1}{\epsilon_c})} \left(1 - \left(1 - \frac{1}{\epsilon_c} \right) \kappa_N N^{1+\frac{1}{\epsilon_\nu}} \right)^{\frac{1}{\epsilon_c}} - 1}{1 - \frac{1}{\epsilon_c}}$$

ϵ_c is the intertemporal elasticity of substitution, λ is the elasticity of substitution between private and public consumption, and $\kappa_G \geq 0$ is the (unnormalized) weight on public consumption. $1 - \phi$

is the fraction of state expenditures spent on state consumption. Note that only a fraction $1-\phi$ of state spending enters as consumption, reflecting the fact that the state spends the remainder on infrastructure.

The household's budget constraint is given by:

$$\begin{aligned} &P_t(C_t + I_t + \kappa(\nu_t)K^p_{t-1}) + \mathbb{E}_t[M_{t,t+1}B_{t+1}(\cdot)] \\ \leq &B_t + (1-\tau^f_t - \tau^s_t)W_t L_t + R^k_t K^p_{t-1}\nu_t + \int_0^1 \Xi_{ht}(z)dz - T_t \end{aligned}$$

Labor income taxes have a federal and a state component, τ^f_t and τ^s_t. The price P_t is the minimum cost of the consumption bundle:

$$P_t = \left(\phi_H P_{H,t}^{1-\eta} + (1-\phi_H)P_{F,t}^{1-\eta}\right)^{\frac{1}{1-\eta}}$$

Households also accumulate capital subject to adjustment costs in the rate of investment:

$$K^p_t = (1-\delta)K_{t-1} + I_t\left(1 - \frac{\kappa_I}{2}\left(\frac{I_t}{I_{t-1}} - 1\right)^2\right).$$

Intermediate goods producers. In each region, there is a continuum of producers $\ell \in [0,1]$ who produce using public infrastructure, private capital, and labor. They perceive constant returns to scale to private capital and labor, although there is a congestion externality for public infrastructure, following Barro and Sala-I-Martin (1992) and Drautzburg and Uhlig (2015). Intermediate goods producers set nominal prices in units of the home region. They may reset prices with an *iid* (Calvo-)probability of $1-\xi$ every period.

Each producer has access to the following constant returns to scale production technology:

$$y_{ht}(\ell) = \left(\frac{K_{st,t-1}}{\bar{y}_{H,t}}\right)^{\frac{\zeta}{1-\zeta}} K_t(\ell)^\alpha N_t(\ell)^{1-\alpha}.$$

The congestion externality on public infrastructure implies that in a symmetric equilibrium, public infrastructure has a share of ζ in aggregate intermediate production.

Each producer faces an *iid* Calvo probability ξ of being stuck with its price $p_{h,t+s}(\ell) = p_{ht}(\ell)$ for another period. Producers therefore set prices to maximize the expected discounted profit flow:

$$\mathbb{E}_t \sum_{s=0}^{\infty} M_{t,t+s}\left(p_{h,t+s}(\ell)y_{h,t+s}(\ell) - W_{t+s}N_{t+s} - R^k_{t+s}K_{t+s}\right).$$

Monetary policy. The monetary authority sets nominal interest rates following a Taylor rule.[16] Specifically, interest rates are smoothed over time and respond to aggregate inflation, and detrended

[16] In the background, we assume the presence of federal lump-sum transfers and taxes that offset the revenue (or losses) generated by monetary policy.

aggregate output:

$$\ln R_t^n = \rho_r \ln R_{t-1}^n + (1-\rho_r)\left(-\ln\beta + \phi_\pi \ln \Pi_t^{agg} + \phi_y \ln \frac{Y_t^{agg}}{\bar{Y}}\right),$$
$$\Pi_t^{agg} = n\Pi_t + (1-n)\Pi_t^*,$$
$$Y_t^{agg} = nY_t + (1-n)Y_t^*.$$

6.2 Equilibrium

We focus on a standard competitive equilibrium: Firms and households take prices, aggregate quantities, and government policies as given when they make their decisions.

To solve the model we, use perturbation methods to compute a first order approximation to the equilibrium dynamics. We analyze an economy only with shocks to intergovernmental transfers. This is possible because we focus on linear dynamics and the dynamic effect only, rather than business cycle statistics in general.

6.3 Calibration

For the common parameters, our calibration follows Nakamura and Steinsson (2014). We thus focus our discussion on the new parameters that we introduce.

To pin down the state spending rules, we draw from the asymptotically normal distribution of estimated dollar pass-throughs of IG increase to expenditures, based on the estimates in Table 2 but converted to dollar terms. The implied dollar coefficients are summarized in Table B.4. We feed the resulting draws into our structural model. The resulting distribution can also be interpreted as a posterior based given a flat prior.

Even though our underlying state panel data estimates feature asymmetric response to year-over-year transfer cuts and increases, the difference in pass-through is robust across several specifications. First, we have already shown in Table 4 that similar estimates hold when we consider multi-year changes, as we do in the policy-experiment here. Second, only the difference between pass-through coefficients is identified, while the baseline coefficient is better viewed as calibrated. The absolute difference between elasticities are of comparable magnitude when we look at increases and cuts in Table 2, so that our model applies equally to long-lasting cuts when we switch labels.

Among the remaining parameters, the parameters that govern how important state governments are for private sector consumption and investment are key. To pin them down, we assume that in steady state, state governments behave optimally: State governments equate marginal utilities to marginal costs, pinning down $\kappa_G = \left(\frac{(1-\phi)\bar{G}^s}{\bar{C}}\right)^{1/\lambda}$ as in Bachmann et al. (2017). And when government services are productive, state governments maximize production net of costs of public infrastructure, as in Drautzburg and Uhlig (2015). This implies that $\zeta = \frac{\phi\bar{G}^s}{\bar{Y}}$, where ϕ is the fraction the state spends on investment.

Parameter	Value / Distribution
Discount factor β	0.99
Frisch elasticity of labor supply ν	1
Calvo stickiness ξ	0.75
Private capital share in production α	0.33
Within-region elasticity of demand θ	6
Across-region elasticity of demand η	2
Home demand for home goods ϕ_H	0.69
Foreign demand for home goods ϕ_H^*	$\frac{n}{1-n}(1-\phi_H)$
Investment adj. cost	0.7
Utilization cost elasticity	1
Taylor rule: inflation ϕ_π	1.5
Taylor rule: output ϕ_y	0.5
Taylor rule: smoothing ρ_r	0.8
Size of home region n	0.5
Elasticity of substitution w.r.t state consumption λ	0.5
Steady state & contemporaneous labor tax fraction $1-\gamma^f = 1-\gamma^s$	0.7
Elasticity of taxes with respect to surplus $\psi_{G,SP}$	0
Federal government consumption $\bar{G}/\bar{Y}$	0.075
Federal government IG $\overline{IG}/\bar{Y}$	0.025
State government consumption $\bar{G}/\bar{Y}$	0.125
Persistence of IG ρ_{IG}	0.89
Standard deviation of IG σ_{IG}	0.10
Democratic transfer pass-through ψ_{IG}^*	Table 2, column (4)
Republican transfer pass-through ψ_{IG}	Table 2, column (4)

The pass-through coefficients are taken from column (4) of Table 2: the "IG incr." coefficient times the expenditure to IG revenue ratio yields ψ_{IG}^* for Democratic governors and ("IG incr." + "Rep x IG incr.") times the expenditure to IG revenue ratio yields ψ_{IG} for Republican governors.

Table 9: Calibrated parameters

To discipline the importance of taxes, we impose that labor income taxes contribute 70% of revenue net of intergovernmental transfers. This reflects the importance of total taxes in general revenue net og intergovernmental transfers; see Figure A.3. We use the same fraction both in steady state and over time, i.e., we set $1-\gamma^s = 0.7$. For simplicity, we also set $\gamma^f = \gamma^s$. This amounts to treating current charges and miscellaneous general revenue as lump-sum taxes in the model. We also capture the most important revenue sources as sales and personal income taxes account for 80% of tax revenue in the data (Figure A.4), and we follow Prescott (2004) in modeling both revenue sources simply as an income tax.

We calibrate the IG process to the 2009 stimulus package: We choose $\rho_=$0.89 to match a half-life of six quarters (Drautzburg and Uhlig, 2015) and a cumulative (non-discounted) value of 320 bn dollars (Carlino and Inman, 2013), or 2.22% of GDP. This yields $\omega_{IG} = 100 \times (1-\rho_{IG}) \times 0.0222$.

Here we focus on the case when private and state consumption are substitutes since Fiorito and Kollintzas (2004) argue that consumption is an (Edgeworth) complement to consumption. In

Appendix C we also discuss the case when private and state consumption are substitutes.

6.4 Dynamics following a shock to federal transfers

Figure 6 shows the exogenous shock hitting the economy and the equilibrium responses of fiscal policy and prices for two scenarios. In one scenario, labeled "all Democrats" and shown as dashed, orange lines, both regions are perfectly symmetric. We focus on the median response here.[17] As a fraction of GDP, their spending increases by the same amount and the dynamics are the same within each region. Consequently, the real exchange rate is constant. Producer prices rise in both regions as labor costs rise due to higher federal taxes and the increased hours worked. State taxes slightly increase as states increase spending by slightly more than one to one.

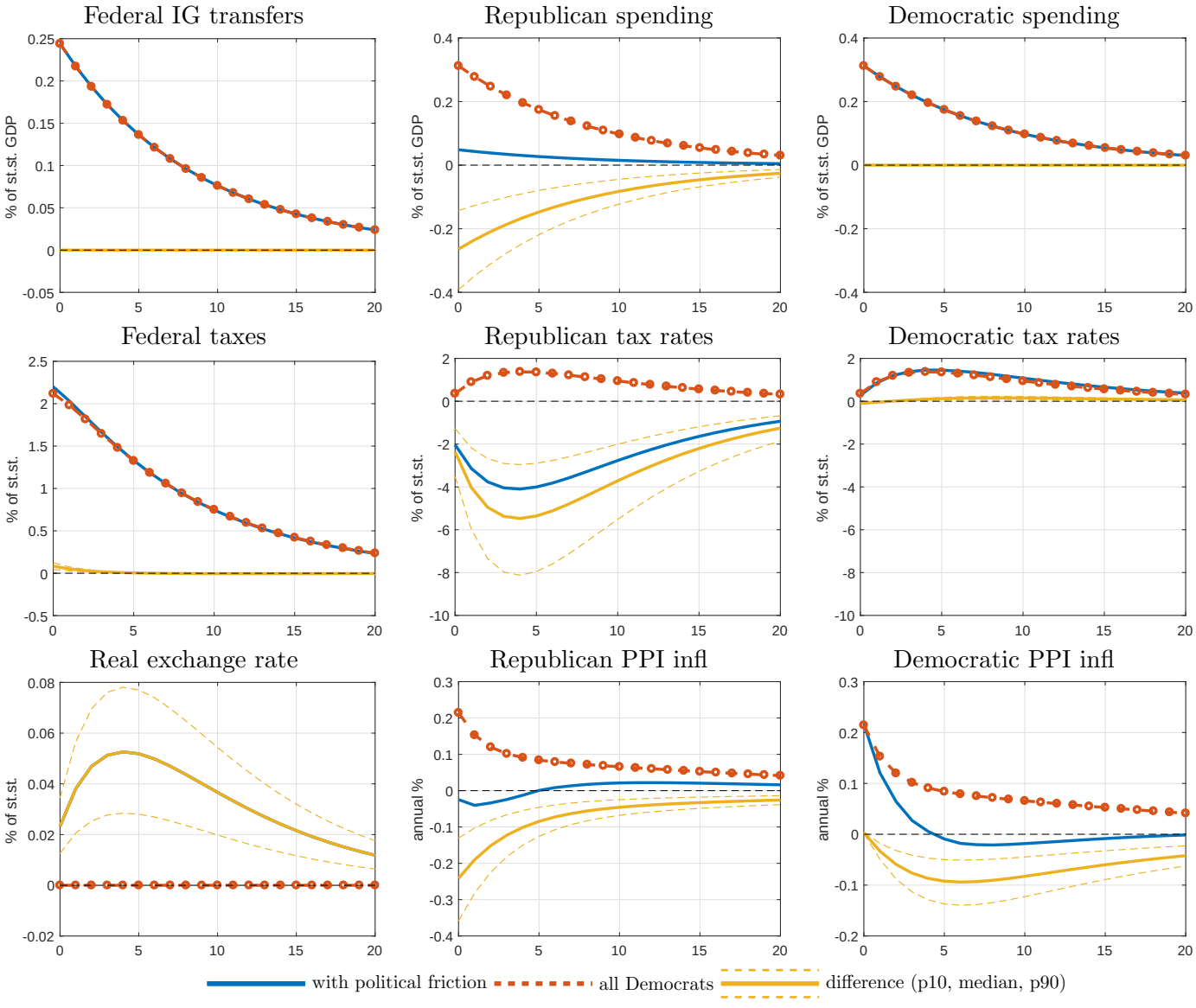


Figure 6: IRFS: Initial shock, fiscal policy responses, and price effects

[17] I.e., we use the draw from Democratic pass-through for both states and report the median across draws.

In the other scenario, labeled "with political friction" and shown as solid, blue lines, one region has the lower "Republican" pass-through. We focus again on the posterior median. This is evident in the lower Republican spending increase in this scenario. Because the regions spend asymmetrically, also the response are asymmetric. Federal taxes still rise, hardly affected by the asymmetric responses. However, the Republican region now cuts taxes, while the Democratic regions still has increasing taxes. The Republican PPI therefore remains roughly flat while the Democratic real exchange rate appreciates.

All differences between the two scenarios are statistically significant when we take the estimation uncertainty from our empirical section into account: We interpret the asymptotic distribution of ψ_{IG} and ψ^*_{IG} as their posterior for a flat prior. We then take draws from this distribution and compute the difference for each of the joint draws. The yellow line shows the median difference along with the 80% credible set.

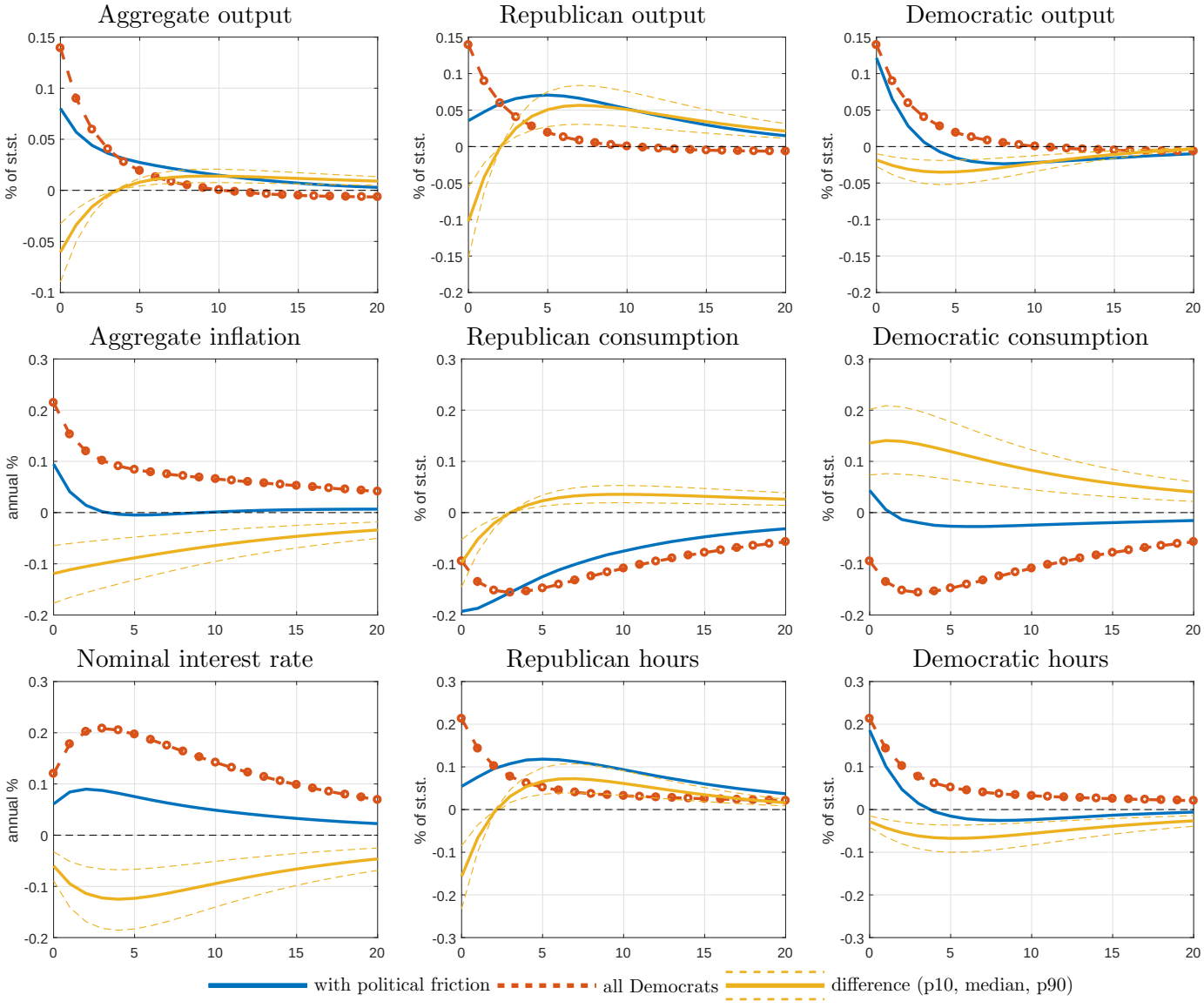


Figure 7: IRFs: output, consumption, and hours

Figure 7 shows the responses of various private sector quantities for each scenario. If both regions behave the same, aggregate output rises by about 0.14% following the 0.25% spending increase. Output reverts to zero somewhat faster than spending, dropping about 0 after ten quarters already. Because households are poorer and real interest rates rise (with a one quarter delay), private consumption falls, while hours worked increase. The results with the political friction are more nuanced: The increase in aggregate output is only 0.08% and is spread unevenly across the two regions. Initially, the Democratic region with its larger increase in demand experiences an increase almost as large as when both regions behaved the same. But whereas the Republican output rises by about 0.05% after five quarters, the Democratic region's output is slightly negative by then. Hours worked are, largely, a scaled up version of output. Again, all differences are statistically significant.

6.5 Multipliers

How much does the federal government stimulate the economy for each dollar it spends under the two scenarios? To answer this question, we follow Mountford and Uhlig (2009) and analyze present discounted value (PDV) multipliers: The ratio of the PDV of output relative to the PDV of federal transfers. Figure 8 shows these PDV multipliers over time, varying the importance of distortionary taxes. In the "all Democrats" scenario and the baseline calibration, the initial multiplier is 56 cents – the ratio of the GDP increase of a good 0.14% to the spending increase of 0.25pp. of GDP. Since output declines more quickly than spending, the multiplier subsequently declines and falls slightly below 0.2 after 20 quarters. With political frictions, the impact multiplier falls by about 40%, to 0.32. It then declines to slightly more than 0.2 after 20 quarters.

The proportional fall in the multiplier is robust for different calibrations of labor income taxes. When labor taxes account for only 10% of the financing need, the impact multiplier rises to 0.74, but the partisan difference also increases to 0.34, keeping the relative change in the impact effect roughly constant. The multipliers rise in our calibration because we simultaneously change the steady state share of labor income taxes. Thus, the 2.5% tax rate increase in the scenario in which labor income taxes account only for 10% of tax revenue applies to a lower tax rate, resulting in a smaller percentage point increase.

We now turn to analyzing how the multiplier varies with how large the Republican region is in the economy. Figure 9 shows the PDV multipliers both as a function of time and the share n of the Republican region in the economy – keeping all other parameters at their baseline value. Without partisan differences, the multiplier simply declines with the horizon, as in Figure 8 above. With partisan differences, both the impact multiplier and the time-profile of the multiplier vary with the size of the Republican region in our model.

Our results suggest that the effects of federal fiscal policy depend on who is running the states. Who has run the U.S. states, Republicans or Democrats, has varied significantly over our sample period: The left panel in Figure 10 shows the fraction of states governed by Republicans, omitting the occasional independent governor. This fraction ranges from a low of 30% after Reagan took

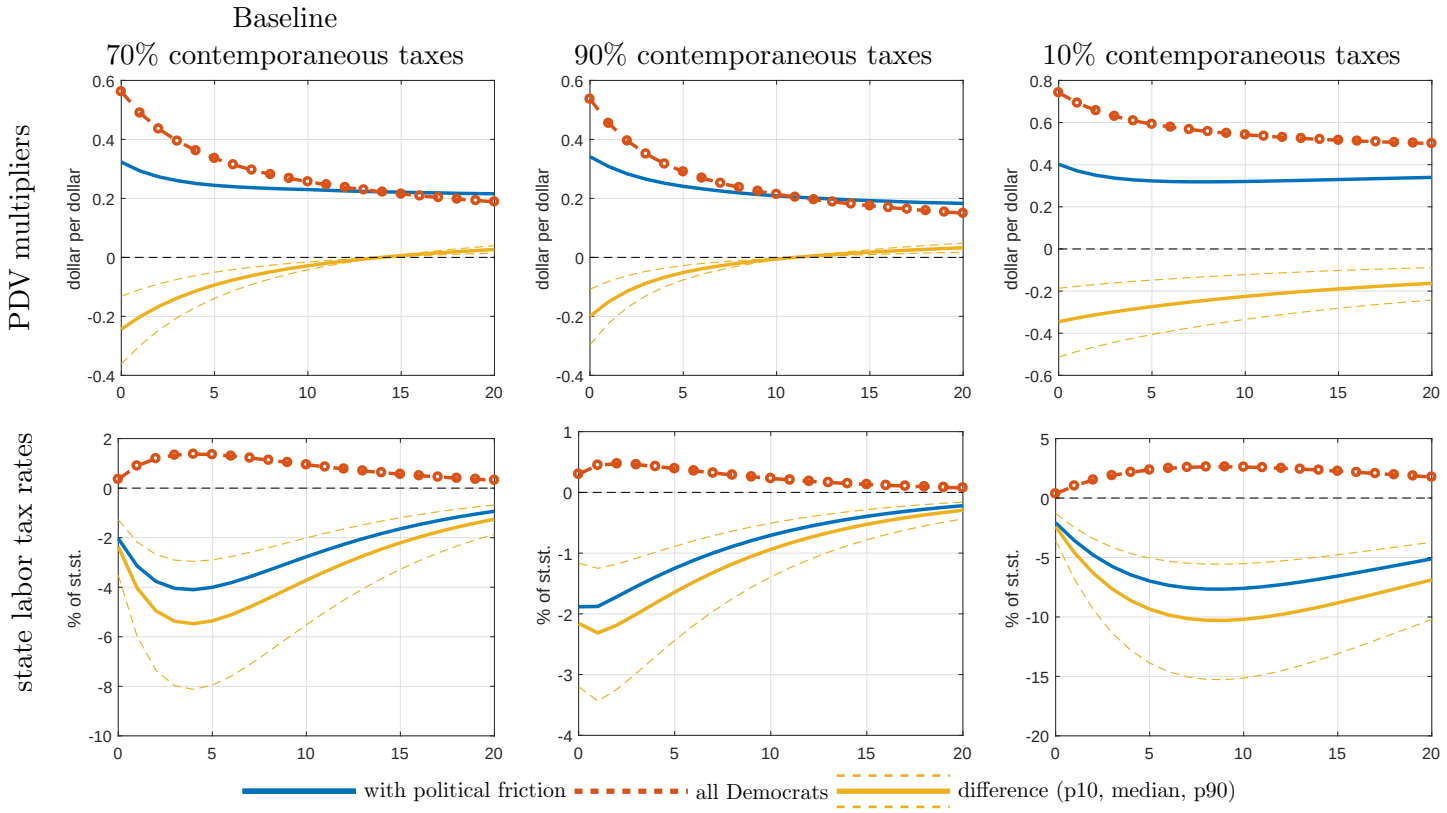


Figure 8: PDV multipliers and distortionary taxes

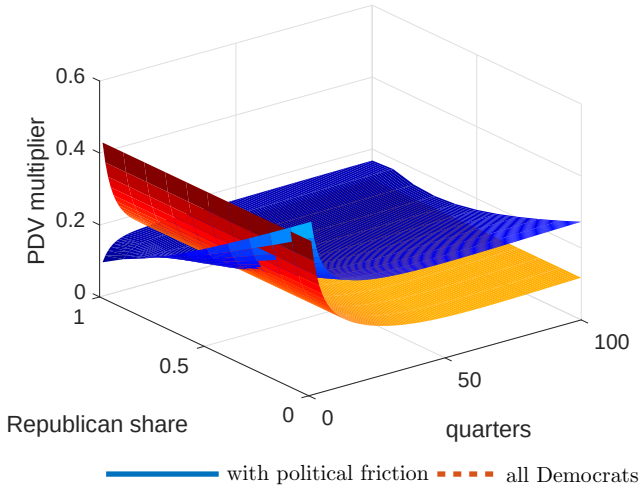


Figure 9: PDV multipliers over time and as a function of the share of Republican governors: baseline

office to a high of roughly two thirds during Clinton's second term. Using these value to calibrate n in our model, translates to sizable differences in the impact transfer multiplier, shown in the middle panel of Figure 10. The transfer multiplier peaks during the early Reagan years with values slightly above 0.4 and falls to about 0.2 during Clinton's and Obama's second terms. The long-run multiplier, in contrast, hardly depends on n and is therefore largely time-invariant (right panel). Computing the difference to the initial multiplier draw by draw, we find that the differences over time are statistically significant.

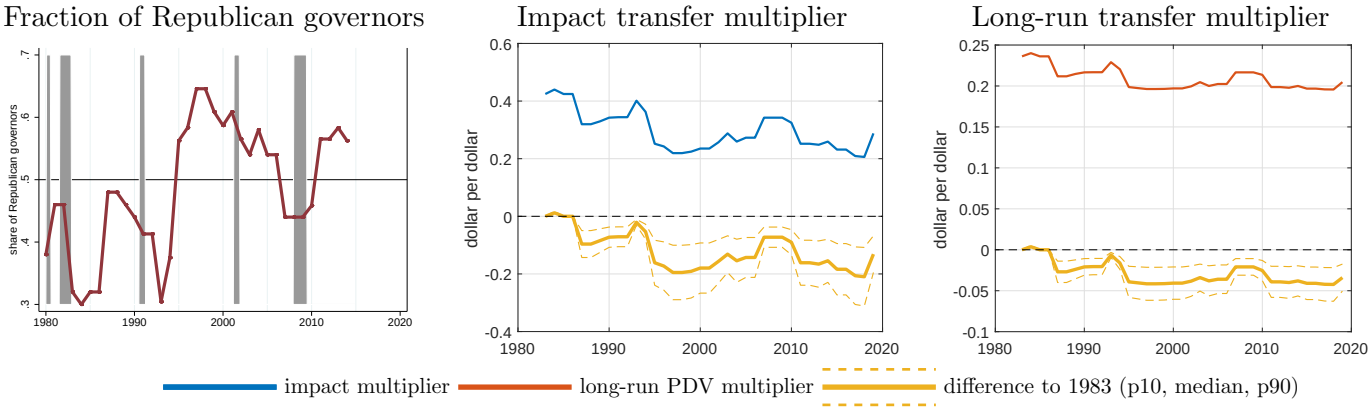


Figure 10: Party control and transfer multipliers over time: State consumption as a complement (baseline)

6.6 Robustness

Here we highlight the robustness of our conclusions about transfer multipliers with regards to three characteristics: (1) How complementary state consumption is to private consumption, (2) how easy it is to adjust capital, and (3) whether part of government consumption is productive. To that end, Figure C.14 compares the multiplier in Republican share n space and over time to the baseline and three scenarios, varying one parameter at a time.

Whether state consumption is a gross complement or a gross substitute is a crucial parameter. While Fiorito and Kollintzas (2004) argue that consumption is an (Edgeworth) complement to consumption, here we also consider what would happen if it were a gross substitute. In that case, the impact effect is largely unchanged, but in the long-run, the multiplier would be much higher when the fraction of Republicans is high, as not growing public consumption but cutting taxes stimulates private consumption.

Specifically, when states public consumption does not complement but substitute private consumption ($\lambda = 1.5$), the multipliers shrink (Figure C.15). The impact multiplier falls from an average of 0.15 to about 0.05 in 2018. The long-run multiplier increases by up to 8 cents from an initial level slightly below zero. The recent difference in the impact-multiplier shrinks in absolute terms, but accounts for a fall of the initial multiplier by two thirds. The long-run multiplier is now higher when Republicans are running more states.

Figure C.14 shows that the differences between policies also become more important if capital is harder to adjust. Intuitively, with fixed capital, output can only increase when labor input rises. However, higher labor taxes lower the incentives to work. Essentially fixed capital, the case in the bottom left panel of Figure C.14, amplifies the time-variation in multipliers. In this case the multiplier remains does not rise over time when Republicans cut taxes because accumulating capital is too costly.

Last, we consider the baseline model, but without productive government investment. This hardly changes the multiplier. Looking closely, the "all Democrats" multiplier rises, but the difference to the baseline is small. Intuitively, we infer a small share of public capital in production to rationalize the small steady state share of public investment. Changes in the public capital stock therefore matter little for private output.

7 Model validation in aggregate time series

Here we qualitatively assess the prediction of our model that the intergovernmental transfer multiplier varies with the state of politics: Does the impact GDP multiplier for a transfer shock indeed fall with the share of Republican governors? We estimate multipliers off the GDP response to a one-percent innovation in intergovernmental transfers. We allow for time-variation in two different ways. First, we estimate rolling window local-projection regressions and then correlate the estimated effects on output with the average share of Republican governors in the same window. The rolling window is a special case of the kernel-based approach to time-variation in Giraitis et al. (2014). Second, we estimate a linear projection that directly allows for a non-linear effect due to the state of politics.

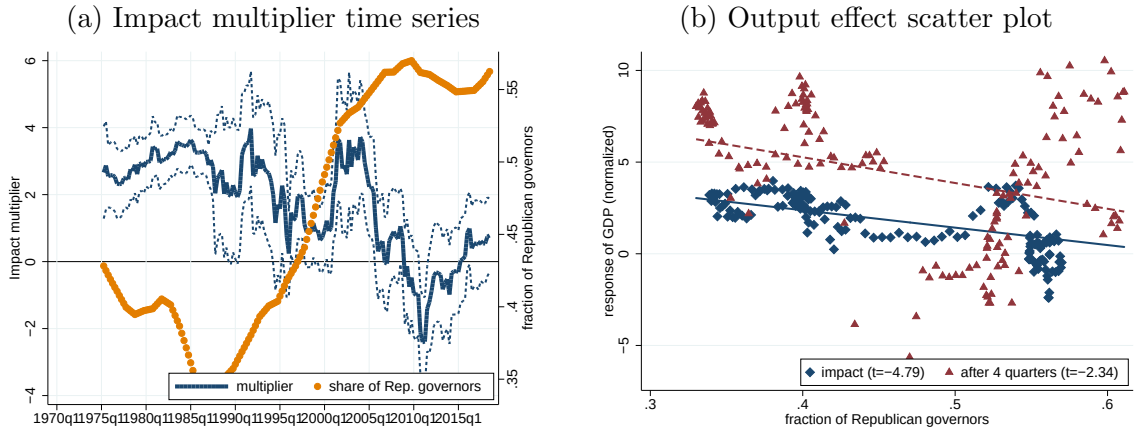
We treat the innovation to intergovernmental transfers as the transfer shock. While the institutional setting differs from the setting for government purchases for which Blanchard and Perotti (2002) defended such an identifying assumption, it is important to note that federal IG transfers exclude important countercylical transfers to states in NIPA. This treatment is different from the budgetary considerations in the Census data and excludes funds for unemployment insurance and funds destined to households. We thus view this assumption is a reasonable starting point if we also include a rich enough information set. To control for expectations, we include lags of GDP , federal expenditures, state and local expenditures, federal tax revenue net of transfers, and intergovernmental transfers, all in logs and real per capita terms.

Our rolling window approach regresses aggregate real per capita GDP (in log-levels) at horizon $t+h$ on intergovernmental transfers at t plus the controls in the four preceding quarters:

$$\ln GDP_{t+h} = \alpha_h^{(\tau)} + \beta_h^{(\tau)} \ln IG_t + \sum_{\ell=1}^{4} \mathbf{x}'_{t-\ell} \gamma_\ell^{(\tau)} + u_{t+h}, \qquad t \in \{\tau - 39, \ldots, \tau\}.$$

We allow for autocorrelation in u_{t+h} for up to $h+1$ quarters when computing standard errors. We

normalize by the average ratio of grants-in-aid to GDP. β_0 has thus the interpretation of an impact multiplier. We reestimate this regression of rolling windows of 40 or 60 quarters.



Note: Only the impact response has the interpretation of a multiplier; the four-quarter ahead result is the cumulative effect on GDP relative to the impact effect on IG.

Figure 11: Reduced-form 15-year rolling window output effects of IG transfers and share of Republican governors.

We then relate the implied output effect of the innovation in intergovernmental transfers to the fraction of Republican governors during the same regression window. Specifically, we estimate:

$$\frac{\hat{\beta}_h^{(\tau)}}{(\overline{IG/Y})_\tau} = \delta_h + \kappa_h \overline{Rep}_\tau.$$

To account for the persistence in $\frac{\hat{\beta}_h^{(\tau)}}{(\overline{IG/Y})_\tau}$, we report heteroskedasticity and autocorrelation-robust standard errors.

Figure 11(a) shows the impact multipliers, estimated over a rolling window of 15 years. It also shows the average fraction of Republican governors over the estimation sample. The graph suggests a negative relationship between the multiplier and the share of Republican governors. Figure 11(b) confirms this. It shows a scatter plot of the same relationship and reports the corresponding t-statistic. Both are highly significant. Figure D.16 in the appendix shows the analogous results with 10-year rolling windows.

An alternative way to allow for time variation in impulse-response is through interaction terms. We also pursue this route and estimate directly:

$$\ln GDP_{t+h} = \alpha_{0,h}^{(\tau)} + \alpha_{Rep,h}^{(\tau)} Rep_{t-4} + \beta_{0,h}^{(\tau)} \ln IG_t + \beta_{Rep,h}^{(\tau)} \ln IG_t \times (Rep_{t-4} - \overline{Rep}) + \sum_{\ell=1}^{4} \mathbf{x}'_{t-\ell} \gamma_{0,\ell}^{(\tau)} + \sum_{\ell=1}^{4} \mathbf{x}'_{t-\ell} \times (Rep_{t-4} - \overline{Rep}) \gamma_{Rep,\ell}^{(\tau)} + u_{t+h}.$$

Here, we lag the share of Republican governors by four quarters to account for the fact that state budgets are passed one fiscal year in advance, the same as in our panel regressions. Table 10

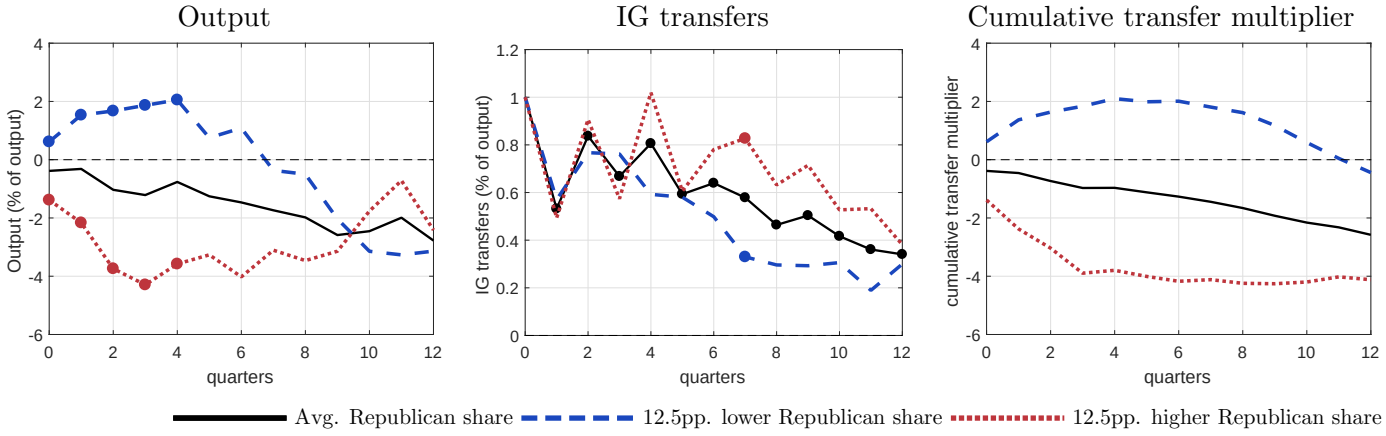
shows the corresponding estimates. Up to four quarters out, the effect of intergovernmental transfers shrinks with the (lagged) fraction of Republican governors, qualitatively the same as in our structural model.

	Impact	h=1	h=2	h=3	h=4
Intergov. Transfers (IG)	-0.008	-0.007	-0.023	-0.027	-0.017
	(-0.80)	(-0.42)	(-1.08)	(-1.29)	(-0.71)
Fraction Rep Gov x IG	-0.176**	-0.325*	-0.476**	-0.542**	-0.495*
	(-2.08)	(-1.92)	(-2.50)	(-2.33)	(-1.88)
Fraction Rep Gov.	0.892	1.709	2.745	3.347	4.202
	(1.26)	(1.22)	(1.39)	(1.38)	(1.56)
R-squared	1.00	1.00	1.00	0.99	0.99
Observations	219	218	217	216	215

Inference based on Newey-West heteroskedasticity and autocorrelation robust standard errors with six lags. Coefficients on control variables omitted.

Table 10: Reduced-form output effects of IG innovations and share of Republican governors: Direct regression with single lag for various horizons.

Figure 12 shows the implied IRFs for output and intergovernmental transfers, along with the cumulative multiplier. The partisan effects on output are significant up to four quarters out, while the baseline output effect is not significantly different from zero. Partisan effects on IG transfers itself are largely insignificant, consistent with the notion that partisan considerations do not influence federal transfers. When the Democratic share of governors is one standard deviation (12.5pp.) higher than usual, the estimates imply an impact multiplier of 0.6, which rises up to 2.1 after six quarters, before declining.



For the output and IG transfer IRF, filled markers denote significance at the 10% level or higher. Inference based on Newey-West heteroskedasticity and autocorrelation robust standard errors with two more lags than the response horizon. For the deviations from the baseline, the markers indicate significant differences from the baseline. For the cumulative multiplier, the figure shows point estimates only.

Figure 12: Responses to innovations in intergovernmental transfer: Direct regressions

Adding survey expectations as a way to control for fiscal foresight does not affect our qualitative

results. Ramey (2011) and Leeper et al. (2013) have documented the importance to account for agents' information set for estimating fiscal multipliers. In Figure D.17 we first add one-quarter ahead inflation and output growth expectations from the Survey of Professional Forecasters to our baseline model. Second, we also add one-quarter ahead expectations of both federal and state and local government purchases. Third, we also add three-quarter ahead purchase expectations. In all cases, we include their interactions with the share of Republican governors. In all three cases, we confirm that the impact output effects are lower when a higher share of states is governed by Republicans. Intriguingly, we also find that once we control for expectations that output effects at the two to three year horizon are rises with the share of Republicans.

The share of Republican governors does not affect the government purchases multiplier, consistent with our model that it only affects the economy through the use of intergovernmental transfers. When we run the same interacted regression for the government purchases multiplier, we find an insignificant effect of the interaction term; see Table D.15. This shows that our finding is not an artifact of the Republican share of governors being some proxy for some underlying determinant of federal purchases, policy, or the economy more broadly.

8 Conclusion

Partisan preferences matter for fiscal policy in the U.S. While there are no unconditional partisan differences in fiscal policy across U.S. states, regression estimates data from close elections reveals partisan differences in fiscal policy in response to inflows of federal intergovernmental transfers. For tax policies, these difference are more pronounced now than they were before the Reagan-era. Suggestive evidence points to a initially higher economic activity with the Democratic use of funds, but, with a delay, higher GDP growth in Republican-run states.

At the aggregate level, the results imply that the partisan composition of state governments matters for the efficacy of fiscal policy. Plugged into a standard model of monetary unions augmented with state governments, our model implies that the impact multiplier is lower when many Republicans are running state-governments because of Keynesian demand effects. Even though Republican states grow faster than Democratic states with a delay, as in our regressions, this keeps the multiplier down. This is a potentially novel source of time-variation in fiscal multipliers. An additional source of time-variation can arise from time-variation in the effects of partisanship itself that we also find, but have yet to explore in our model. We present suggestive time-series evidence supporting the model prediction that the state of politics causes time-variation in how effective federal transfers are in stimulating the economy.

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Appendix

A Data appendix

A.1 Political variables

A.2 Revenues

All census data come from https://www.census.gov/govs/local/ and https://www2.census.gov/pub/outgoing/govs/special60/State_Govt_Fin.zip.

$$\begin{aligned}
TotalRevenues_t &= GeneralRevenues_t + LiquorStoreRevenues_t \\
&\quad + TotalUtilityRevenues + TotalInsuranceTrustRevenues_t \\
GeneralRevenues_t &= TotalTaxesRev_t + TotalIntergovernmentalTransferRev_t \\
&\quad + TotalGeneralCharges_t + MiscGeneralRevenueRev_t \\
TotalUtilityRevenues_t &= WaterUtilityRevenue_t + ElectricUtilityRev_t \\
&\quad + GasUtilityRev_t + TransitUtilityRev_t \\
TotalInsuranceTrustRevenues_t &= TotalEmploymentRetirementRevenue_t + TotalUnemploymentRevenue_t \\
&\quad + TotalWorkerCompensationRevenue_t \\
&\quad + TotalOtherInsuranceTrustRevenue_t
\end{aligned}$$

A.2.1 Revenue Definition from Census

- General Government Sector: Within the totals of government revenue and expenditure, internal transfers (e.g., interfund transactions) are "netted out." Therefore, "general revenue" and "general expenditure" represent only revenue from external sources and expenditures to individuals or agencies outside the government, and do not directly reflect any "transfer" or "contributions" to or from the utilities, liquor stores, or insurance trust sectors. See Section 3.9 for more information on internal transactions.

- Utilities Sector: In the primary classification of government revenue and expenditure, the term "utility" is used to identify certain types of revenue and expenditure categories. Utility revenue relates only to the revenue from sales of goods or services and by-products to consumers outside the government. Revenue arising from outside other aspects of utility operations is classified as general revenue (e.g., interest earnings). Utility expenditure applies to all expenditures for financing utility facilities, for interest on utility debt, and for operation, maintenance, and other costs involved in producing and selling utility commodities and services to the public (other than noncash transactions like depreciation of assets).

- Liquor Stores Sector: Liquor stores revenue relates only to amounts received from sale of goods and associated services or products. Liquor store expenditure relates only to amounts for purchase of goods for resale and for provision, operation, and maintenance of the stores. Any associated government activity, such as licensing and enforcement of liquor laws or collection of liquor taxes, are classified under the general government sector

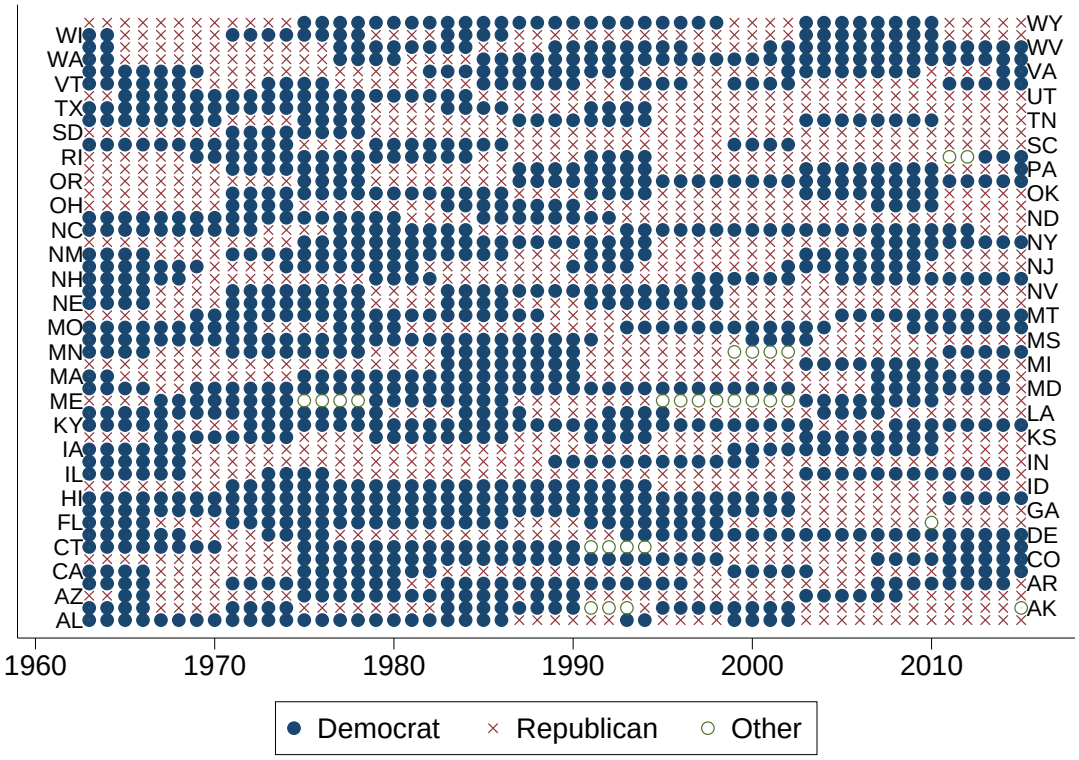
#	State	Year	MOV	in sample with following FE: State & Year	Party-State Party-Year	State & Region-Year
1	Alabama	1992	-4.2	yes	yes	yes
2	Alabama	1993	-4.2	yes	yes	yes
3	Alabama	1996	-.9	yes	yes	yes
4	Alabama	1997	-.9	yes	yes	yes
5	Alabama	1998	-.9	yes	yes	yes
6	Alabama	1999	-.9	yes	yes	yes
7	Alabama	2004	-.2	yes	yes	yes
8	Alabama	2005	-.2	yes	yes	yes
9	Alabama	2006	-.2	yes	yes	yes
10	Alabama	2007	-.2	yes	yes	yes
11	Arizona	1992	-4.7	yes	yes	yes
12	Arizona	1993	-4.7	yes	yes	yes
13	Arizona	1994	-4.7	yes	yes	yes
14	Arizona	1995	-4.7	yes	yes	yes
15	California	1984	-1.2	yes	yes	yes
16	California	1985	-1.2	yes	yes	yes
17	California	1986	-1.2	yes	yes	yes
18	California	1987	-1.2	yes	yes	yes
19	California	1992	-3.5	yes	yes	yes
20	California	1993	-3.5	yes	yes	yes
21	California	1994	-3.5	yes	yes	yes
22	California	1995	-3.5	yes	yes	yes
23	Colorado	2000	-1.1	yes	yes	yes
24	Colorado	2001	-1.1	yes	yes	yes
25	Colorado	2002	-1.1	yes	yes	yes
26	Colorado	2003	-1.1	yes	yes	yes
27	Connecticut	1996	-3.5	yes	yes	yes
28	Connecticut	1997	-3.5	yes	yes	yes
29	Connecticut	1998	-3.5	yes	yes	yes
30	Connecticut	1999	-3.5	yes	yes	yes
31	Florida	2012	-1.1	yes	yes	yes
32	Florida	2013	-1.1	yes	yes	yes
33	Florida	2014	-1.1	yes	yes	yes
34	Hawaii	2004	-4.5	yes	yes	yes
35	Hawaii	2005	-4.5	yes	yes	yes
36	Hawaii	2006	-4.5	yes	yes	yes
37	Hawaii	2007	-4.5	yes	yes	yes
38	Illinois	1984	-.1	yes	yes	yes
39	Illinois	1985	-.1	yes	yes	yes
40	Illinois	1986	-.1	yes	yes	yes
41	Illinois	1987	-.1	yes	yes	yes
42	Illinois	1992	-2.6	yes	yes	yes
43	Illinois	1993	-2.6	yes	yes	yes
44	Illinois	1994	-2.6	yes	yes	yes
45	Illinois	1995	-2.6	yes	yes	yes
46	Illinois	2000	-3.6	yes	yes	yes
47	Illinois	2001	-3.6	yes	yes	yes
48	Illinois	2002	-3.6	yes	yes	yes
49	Illinois	2003	-3.6	yes	yes	yes
50	Indiana	1986	-5	yes	yes	yes
51	Indiana	1987	-5	yes	yes	yes
52	Indiana	1988	-5	yes	yes	yes
53	Indiana	1989	-5	yes	yes	yes
54	Indiana	2014	-2.9	yes	yes	yes
55	Iowa	1988	-3.9	yes	yes	yes
56	Iowa	1989	-3.9	yes	yes	yes
57	Iowa	1990	-3.9	yes	yes	yes
58	Iowa	1991	-3.9	yes	yes	yes
59	Kansas	1988	-3.8	yes	yes	yes
60	Kansas	1989	-3.8	yes	yes	yes
61	Kansas	1990	-3.8	yes	yes	yes
62	Kansas	1991	-3.8	yes	yes	yes
63	Louisiana	1983	-.7	yes	yes	yes
64	Louisiana	1984	-.7	yes	yes	
65	Maine	1992	-2.6	yes	yes	yes
66	Maine	1993	-2.6	yes	yes	yes
67	Maine	1994	-2.6	yes	yes	yes
68	Maine	1995	-2.6	yes	yes	yes
69	Maryland	2004	-3.9	yes	yes	yes
70	Maryland	2005	-3.9	yes	yes	yes
71	Maryland	2006	-3.9	yes	yes	yes
72	Maryland	2007	-3.9	yes	yes	yes
73	Massachusetts	1992	-3.2	yes	yes	yes
74	Massachusetts	1993	-3.2	yes	yes	yes
75	Massachusetts	1994	-3.2	yes	yes	yes
76	Massachusetts	1995	-3.2	yes	yes	yes
77	Massachusetts	2000	-3.4	yes	yes	yes
78	Massachusetts	2001	-3.4	yes	yes	yes
79	Massachusetts	2002	-3.4	yes	yes	yes
80	Massachusetts	2003	-3.4	yes	yes	yes
81	Massachusetts	2004	-4.8	yes	yes	yes
82	Massachusetts	2005	-4.8	yes	yes	yes
83	Massachusetts	2006	-4.8	yes	yes	yes
84	Massachusetts	2007	-4.8	yes	yes	yes
85	Michigan	1992	-.7	yes	yes	yes
86	Michigan	1993	-.7	yes	yes	yes
87	Michigan	1994	-.7	yes	yes	yes
88	Michigan	1995	-.7	yes	yes	yes
89	Minnesota	1992	-3.3	yes	yes	yes
90	Minnesota	1993	-3.3	yes	yes	yes
91	Minnesota	1994	-3.3	yes	yes	yes
92	Minnesota	1995	-3.3	yes	yes	yes
93	Minnesota	2008	-1	yes	yes	yes
94	Minnesota	2009	-1	yes	yes	yes
95	Minnesota	2010	-1	yes	yes	
96	Minnesota	2011	-1	yes	yes	
97	Mississippi	1993	-3.2	yes	yes	yes
98	Mississippi	1994	-3.2	yes	yes	yes
99	Mississippi	1995	-3.2	yes	yes	yes
100	Mississippi	1996	-3.2	yes	yes	yes
101	Missouri	2006	-3	yes	yes	yes
102	Missouri	2007	-3	yes	yes	yes
103	Missouri	2008	-3	yes	yes	yes
104	Missouri	2009	-3	yes	yes	yes
105	Montana	1994	-2.7	yes	yes	yes
106	Montana	1995	-2.7	yes	yes	yes
107	Montana	1996	-2.7	yes	yes	yes
108	Montana	1997	-2.7	yes	yes	yes
109	Montana	2002	-3.9	yes	yes	yes
110	Montana	2003	-3.9	yes	yes	yes
111	Montana	2004	-3.9	yes	yes	yes
112	Montana	2005	-3.9	yes	yes	yes
113	Nevada	2008	-4	yes	yes	yes
114	Nevada	2009	-4	yes	yes	yes
115	Nevada	2010	-4	yes	yes	
116	Nevada	2011	-4	yes	yes	
117	New Hampshire	1984	-4.6	yes	yes	yes
118	New Hampshire	1985	-4.6	yes	yes	yes
119	New Jersey	1983	-.1	yes	yes	
120	New Jersey	1984	-.1	yes	yes	yes
121	New Jersey	1985	-.1	yes	yes	yes
122	New Jersey	1986	-.1	yes	yes	yes
123	New Jersey	1995	-1	yes	yes	yes
124	New Jersey	1996	-1	yes	yes	yes
125	New Jersey	1997	-1	yes	yes	yes
126	New Jersey	1998	-1	yes	yes	yes
127	New Jersey	1999	-1.1	yes	yes	yes
128	New Jersey	2000	-1.1	yes	yes	yes
129	New Jersey	2001	-1.1	yes	yes	yes
130	New Jersey	2002	-1.1	yes	yes	yes
131	New Jersey	2011	-3.6	yes	yes	yes
132	New Jersey	2012	-3.6	yes	yes	yes
133	New Jersey	2013	-3.6	yes	yes	yes
134	New Jersey	2014	-3.6	yes	yes	yes
135	New York	1996	-3.3	yes	yes	yes
136	New York	1997	-3.3	yes	yes	yes
137	New York	1998	-3.3	yes	yes	yes
138	New York	1999	-3.3	yes	yes	yes
139	Ohio	1983	-1.7	yes	yes	yes
140	Ohio	2012	-2	yes	yes	yes
141	Ohio	2013	-2	yes	yes	yes
142	Ohio	2014	-2	yes	yes	yes
143	Oklahoma	1988	-2.9	yes	yes	yes
144	Oklahoma	1989	-2.9	yes	yes	yes
145	Oklahoma	1990	-2.9	yes	yes	yes
146	Oklahoma	1991	-2.9	yes	yes	yes
147	Pennsylvania	1984	-2.7	yes	yes	yes
148	Pennsylvania	1985	-2.7	yes	yes	yes
149	Pennsylvania	1986	-2.7	yes	yes	yes
150	Pennsylvania	1987	-2.7	yes	yes	yes
151	Rhode Island	1990	-1.7	yes	yes	yes
152	Rhode Island	1991	-1.7	yes	yes	yes
153	Rhode Island	1996	-3.8	yes	yes	yes
154	Rhode Island	1997	-3.8	yes	yes	yes
155	Rhode Island	1998	-3.8	yes	yes	yes
156	Rhode Island	1999	-3.8	yes	yes	yes
157	Rhode Island	2008	-2	yes	yes	
158	Rhode Island	2009	-2	yes	yes	
159	Rhode Island	2010	-2	yes	yes	
160	Rhode Island	2011	-2	yes	yes	yes
161	South Carolina	1988	-3.1	yes	yes	yes
162	South Carolina	1989	-3.1	yes	yes	yes
163	South Carolina	1990	-3.1	yes	yes	yes
164	South Carolina	1991	-3.1	yes	yes	yes
165	South Carolina	1996	-2.5	yes	yes	yes
166	South Carolina	1997	-2.5	yes	yes	yes
167	South Carolina	1998	-2.5	yes	yes	yes
168	South Carolina	1999	-2.5	yes	yes	yes
169	South Carolina	2012	-4.5	yes	yes	yes
170	South Carolina	2013	-4.5	yes	yes	yes
171	South Carolina	2014	-4.5	yes	yes	yes
172	South Dakota	1988	-3.6	yes	yes	yes
173	South Dakota	1989	-3.6	yes	yes	yes
174	South Dakota	1990	-3.6	yes	yes	yes
175	South Dakota	1991	-3.6	yes	yes	yes
176	Texas	1983	-.7	yes		yes
177	Utah	1990	-1.7	yes	yes	yes
178	Utah	1991	-1.7	yes	yes	yes
179	Utah	1992	-1.7	yes	yes	yes
180	Utah	1993	-1.7	yes	yes	yes
181	Vermont	2004	-2.6	yes	yes	yes
182	Vermont	2005	-2.6	yes	yes	yes

Table A.1: Marginally elected Republican governors up to a 5pp. MOV

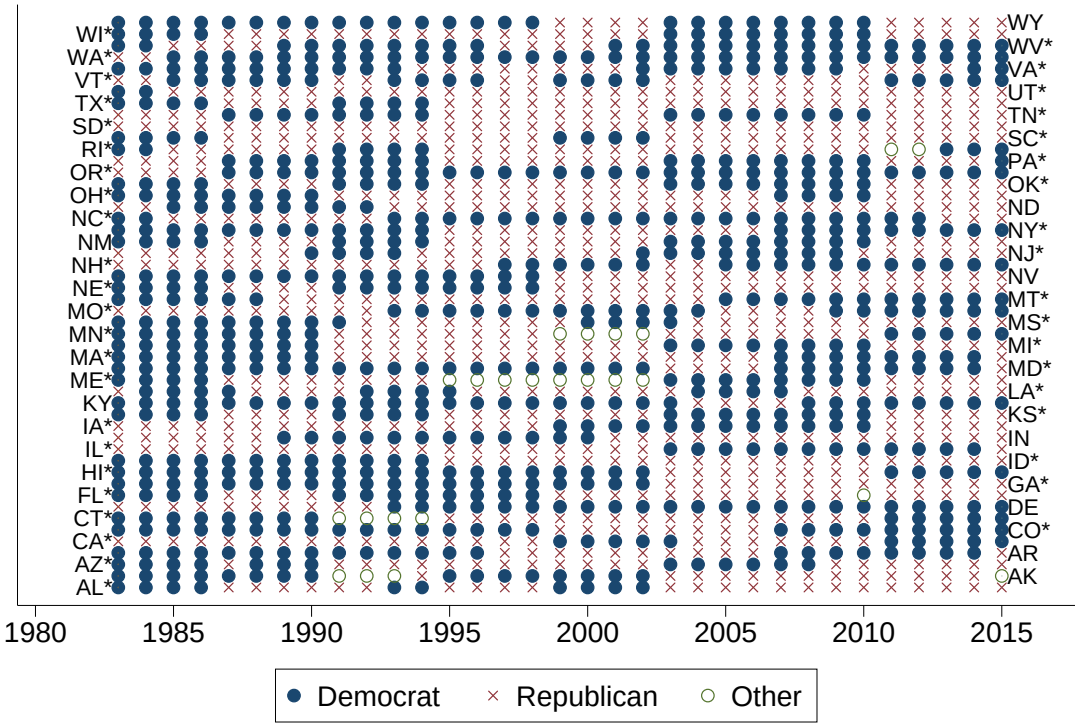
#	State	Year	MOV	in sample with following FE: State & Year	Party-State Party-Year	State & Region-Year
1	Arizona	2004	1	yes	yes	yes
2	Arizona	2005	1	yes	yes	yes
3	Arizona	2006	1	yes	yes	yes
4	Arizona	2007	1	yes	yes	yes
5	California	2004	4.9	yes		yes
6	Connecticut	2012	.6	yes	yes	yes
7	Connecticut	2013	.6	yes	yes	yes
8	Connecticut	2014	.6	yes	yes	yes
9	Florida	1996	1.5	yes	yes	yes
10	Florida	1997	1.5	yes	yes	yes
11	Florida	1998	1.5	yes	yes	yes
12	Florida	1999	1.5	yes	yes	yes
13	Georgia	1996	2.1	yes	yes	yes
14	Georgia	1997	2.1	yes	yes	yes
15	Georgia	1998	2.1	yes	yes	yes
16	Georgia	1999	2.1	yes	yes	yes
17	Hawaii	1988	3.9	yes	yes	yes
18	Hawaii	1989	3.9	yes	yes	yes
19	Hawaii	1990	3.9	yes	yes	yes
20	Hawaii	1991	3.9	yes	yes	yes
21	Hawaii	2000	1.3	yes	yes	yes
22	Hawaii	2001	1.3	yes	yes	yes
23	Hawaii	2002	1.3	yes	yes	yes
24	Hawaii	2003	1.3	yes	yes	yes
25	Idaho	1984	1.3	yes	yes	yes
26	Idaho	1985	1.3	yes	yes	yes
27	Idaho	1986	1.3	yes	yes	yes
28	Idaho	1987	1.3	yes	yes	yes
29	Idaho	1988	.9	yes	yes	yes
30	Idaho	1989	.9	yes	yes	yes
31	Idaho	1990	.9	yes	yes	yes
32	Idaho	1991	.9	yes	yes	yes
33	Illinois	2012	.9	yes	yes	yes
34	Illinois	2013	.9	yes	yes	yes
35	Illinois	2014	.9	yes	yes	yes
36	Indiana	1998	4.7	yes	yes	
37	Indiana	1999	4.7	yes	yes	
38	Indiana	2000	4.7	yes	yes	yes
39	Indiana	2001	4.7	yes	yes	yes
40	Kansas	1983	2.1	yes		yes
41	Louisiana	2005	3.9	yes	yes	yes
42	Louisiana	2006	3.9	yes	yes	yes
43	Louisiana	2007	3.9	yes	yes	yes
44	Louisiana	2008	3.9	yes	yes	
45	Michigan	2004	4	yes	yes	yes
46	Michigan	2005	4	yes	yes	yes
47	Michigan	2006	4	yes	yes	yes
48	Michigan	2007	4	yes	yes	yes
49	Minnesota	2012	.4	yes	yes	yes
50	Minnesota	2013	.4	yes	yes	yes
51	Minnesota	2014	.4	yes	yes	yes
52	Mississippi	2001	1.1	yes	yes	
53	Mississippi	2002	1.1	yes	yes	yes
54	Mississippi	2003	1.1	yes	yes	yes
55	Mississippi	2004	1.1	yes	yes	yes
56	Missouri	2002	.9	yes	yes	yes
57	Missouri	2003	.9	yes	yes	yes
58	Missouri	2004	.9	yes	yes	yes
59	Missouri	2005	.9	yes	yes	yes
60	Montana	2006	4.4	yes	yes	yes
61	Montana	2007	4.4	yes	yes	yes
62	Montana	2008	4.4	yes	yes	yes
63	Montana	2009	4.4	yes	yes	yes
64	Montana	2014	1.6	yes	yes	yes
65	Nebraska	1984	1.3	yes	yes	yes
66	Nebraska	1985	1.3	yes	yes	yes
67	Nebraska	1986	1.3	yes	yes	yes
68	Nebraska	1987	1.3	yes	yes	yes
69	Nebraska	1992	.7	yes	yes	yes
70	Nebraska	1993	.7	yes	yes	yes
71	Nebraska	1994	.7	yes	yes	yes
72	Nebraska	1995	.7	yes	yes	yes
73	New Hampshire	2002	5	yes	yes	yes
74	New Hampshire	2003	5	yes	yes	yes
75	New Hampshire	2006	2.2	yes	yes	yes
76	New Hampshire	2007	2.2	yes	yes	yes
77	New York	1984	3.4	yes	yes	yes
78	New York	1985	3.4	yes	yes	yes
79	New York	1986	3.4	yes	yes	yes
80	New York	1987	3.4	yes	yes	yes
81	North Carolina	2010	3.4	yes		
82	North Carolina	2011	3.4	yes		
83	North Carolina	2012	3.4	yes	yes	yes
84	North Carolina	2013	3.4	yes	yes	yes
85	Oklahoma	1983	4.5	yes		yes
86	Oklahoma	2004	.7	yes	yes	yes
87	Oklahoma	2005	.7	yes	yes	yes
88	Oklahoma	2006	.7	yes	yes	yes
89	Oklahoma	2007	.7	yes	yes	yes
90	Oregon	1988	4	yes	yes	yes
91	Oregon	1989	4	yes	yes	yes
92	Oregon	1990	4	yes	yes	yes
93	Oregon	1991	4	yes	yes	yes
94	Oregon	2004	1	yes	yes	yes
95	Oregon	2005	1	yes	yes	yes
96	Oregon	2006	1	yes	yes	yes
97	Oregon	2007	1	yes	yes	yes
98	Oregon	2012	1.5	yes	yes	
99	Oregon	2013	1.5	yes	yes	
100	Oregon	2014	1.5	yes	yes	yes
101	Pennsylvania	1988	2.3	yes	yes	
102	Pennsylvania	1989	2.3	yes	yes	
103	Pennsylvania	1990	2.3	yes	yes	yes
104	Pennsylvania	1991	2.3	yes	yes	yes
105	Tennessee	2004	3.1	yes	yes	yes
106	Tennessee	2005	3.1	yes	yes	yes
107	Tennessee	2006	3.1	yes	yes	yes
108	Tennessee	2007	3.1	yes	yes	yes
109	Texas	1992	2.5	yes	yes	yes
110	Texas	1993	2.5	yes	yes	yes
111	Texas	1994	2.5	yes	yes	yes
112	Texas	1995	2.5	yes	yes	yes
113	Vermont	1986	1.6	yes	yes	yes
114	Vermont	1987	1.6	yes	yes	yes
115	Vermont	2012	1.8	yes	yes	yes
116	Vermont	2013	1.8	yes	yes	yes
117	Virginia	1991	.4	yes	yes	yes
118	Virginia	1992	.4	yes	yes	yes
119	Virginia	1993	.4	yes	yes	yes
120	Virginia	1994	.4	yes	yes	yes
121	Washington	1994	4.3	yes	yes	yes
122	Washington	1995	4.3	yes	yes	yes
123	Washington	1996	4.3	yes	yes	yes
124	Washington	1997	4.3	yes	yes	yes
125	Washington	2006	0	yes	yes	yes
126	Washington	2007	0	yes	yes	yes
127	Washington	2008	0	yes	yes	yes
128	Washington	2009	0	yes	yes	yes
129	West Virginia	2002	2.9	yes	yes	yes
130	West Virginia	2003	2.9	yes	yes	yes
131	West Virginia	2004	2.9	yes	yes	yes
132	West Virginia	2005	2.9	yes	yes	yes
133	West Virginia	2014	4.8	yes	yes	yes
134	Wisconsin	2004	3.7	yes	yes	yes
135	Wisconsin	2005	3.7	yes	yes	yes
136	Wisconsin	2006	3.7	yes	yes	yes
137	Wisconsin	2007	3.7	yes	yes	yes

Table A.2: Marginally elected Democratic governors up to a 5pp. MOV

(a) Full sample: 1963–2014



(b) Baseline sample: 1983–2014



* ever in 4pp MOV sample

Figure A.1: State composition

- Social Insurance Trust Sector: Insurance trust revenue comprises only (1) retirement and social insurance contributions, including unemployment compensation "taxes" received from employees and other government or private employers, and (2) net earnings on investments set aside to provide income for insurance trusts. Transfers or contributions from other funds of the same government are not classified as insurance trust revenue but rather are reported under special exhibit categories (see Chapters 8 and 9). Insurance trust expenditure comprises only benefit payments and withdrawals of contributions made from retirement and social insurance trust funds. Costs for administering insurance trust systems are classified under the general government sector. Social Insurance Trust Sector: Insurance trust revenue comprises only (1) retirement and social insurance contributions, including unemployment compensation "taxes" received from employees and other government or private employers, and (2) net earnings on investments set aside to provide income for insurance trusts.3 Transfers or contributions from other funds of the same government are not classified as insurance trust revenue but rather are reported under special exhibit categories (see Chapters 8 and 9). Insurance trust expenditure comprises only benefit payments and withdrawals of contributions made from retirement and social insurance trust funds. Costs for administering insurance trust systems are classified under the general government sector.

A.3 Expenditures

$$
\begin{aligned}
TotalExpenditure_t &= TotalIGExpenditure_t DirectExpenditure_t \\
TotalIGExpenditure_t &= TotalIGExpenditure2Federal_t + TotalIGExpenditure2Local_t \\
DirectExpenditure_t &= TotalCurrentOperationalExpendture_t \\
&\quad + TotalCapitalOutlayExpenditure_t \\
&\quad + TotalAssistanceAndSubsidies_t + TotalInterestOnDebt_t \\
&\quad + TotalInsuranceTrustBenefits_t \\
TotalCapitalOutlayExpenditure_t &= TotalConstructions_t + TotalOtherCapitalOutlays_t
\end{aligned}
$$

A.3.1 Expenditures Definition from Census

- Current Operations: Direct expenditure for compensation of own officers and employees and for supplies, materials, and contractual services except any amounts for capital outlay (i.e., for personal services or other objects used in contract construction or government employee construction of permanent structures and for acquisition of property and equipment).
- Interest on Debt: Amounts paid for the use of borrowed money.
- Assistance and Subsidies: Direct cash assistance to foreign governments, private individuals, and nongovernmental organizations (e.g., foreign aid, agricultural supports, public welfare, veteran bonuses, and cash grants for tuition and scholarships) neither in return for goods and services nor in repayment of debt and other claims against the government.
- Capital Outlay: Direct expenditure for purchase or construction, by contract or government employee, construction of buildings and other improvements; for purchase of land, equipment, and existing structures; and for payments on capital leases.
- Intergovernmental expenditure is defined as amounts paid to other governments for performance of specific functions or for general financial support. Includes grants, shared taxes, contingent loans and advances, and any significant and identifiable amounts or reimbursement paid to other governments for performance of general government services or activities.

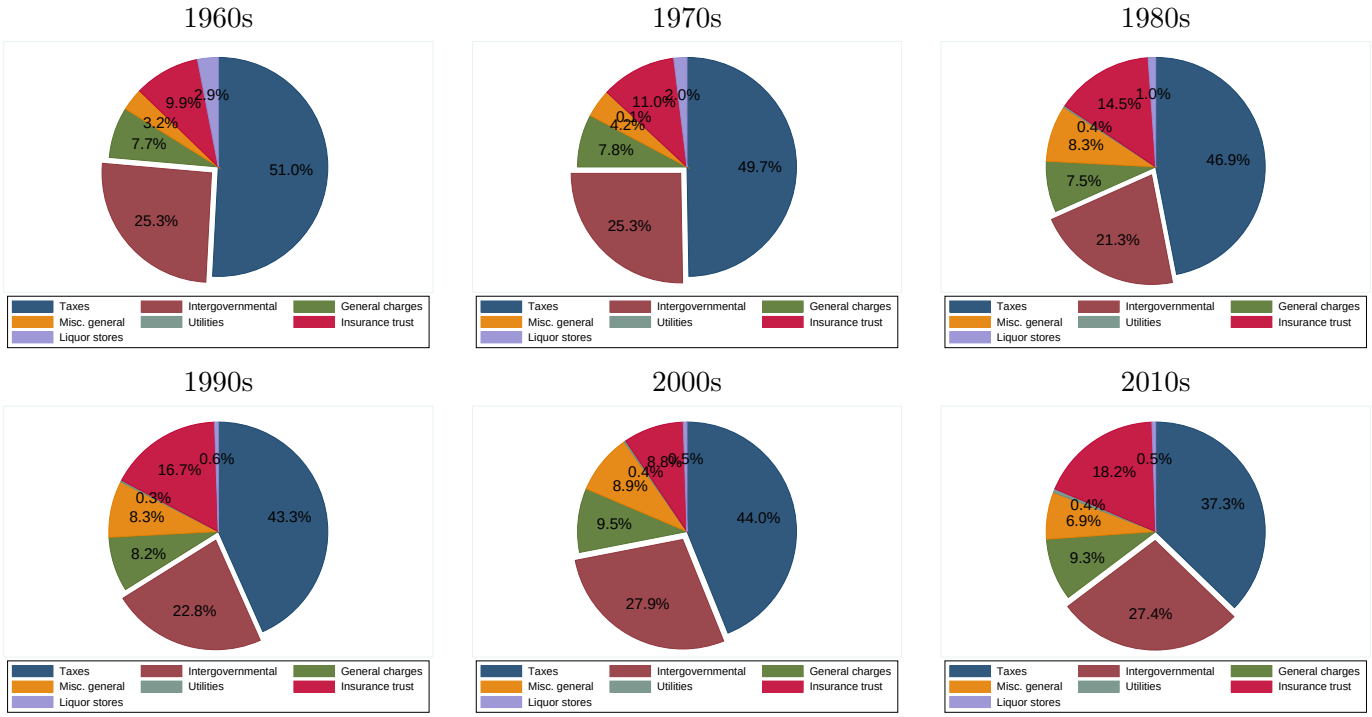


Figure A.2: Overall revenue components: Average by decade, 1963–2014

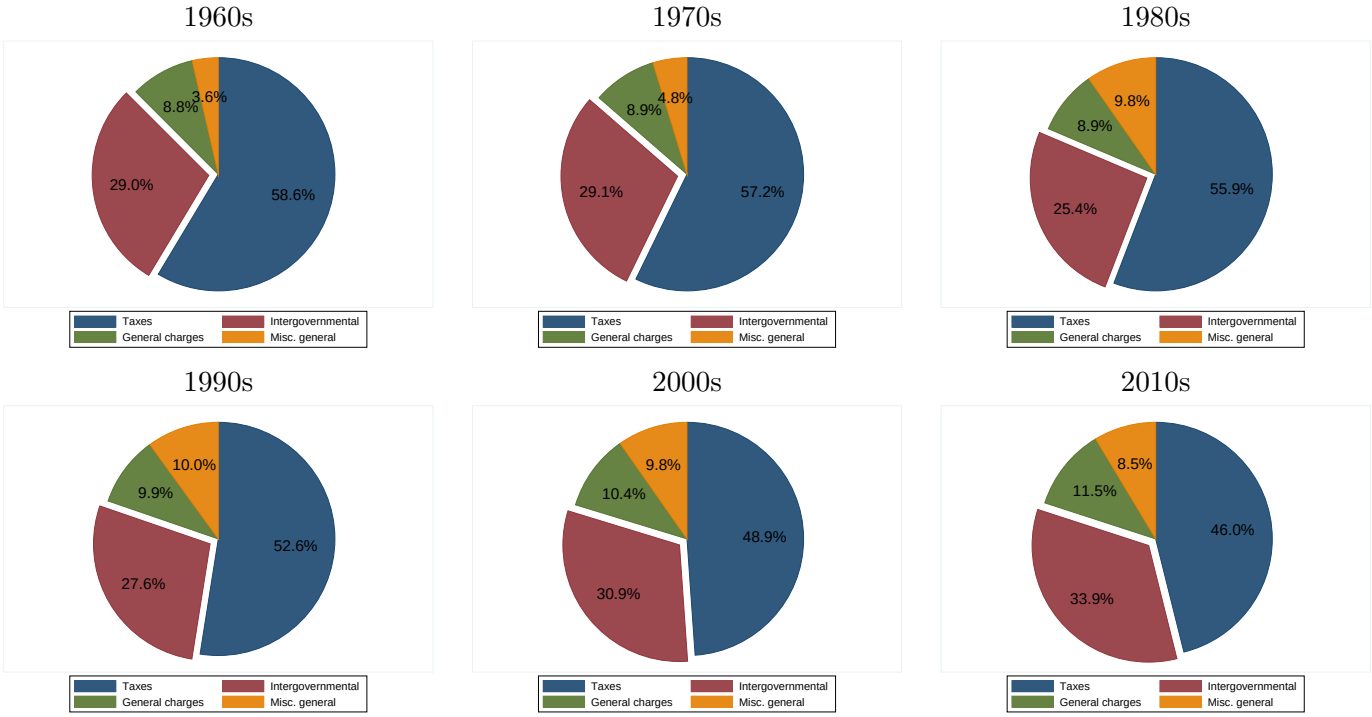


Figure A.3: General revenue components: Average by decade, 1963–2014

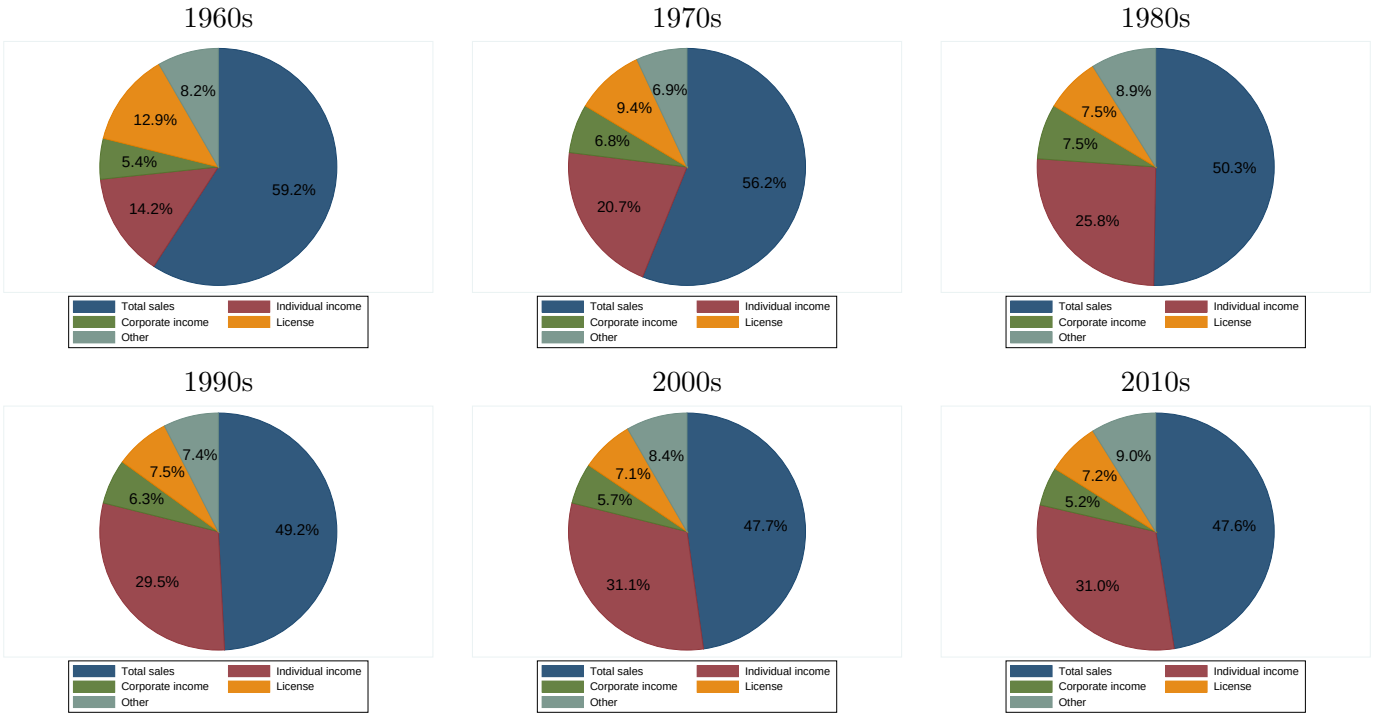


Figure A.4: Tax revenue components: Average by decade, 1963–2014

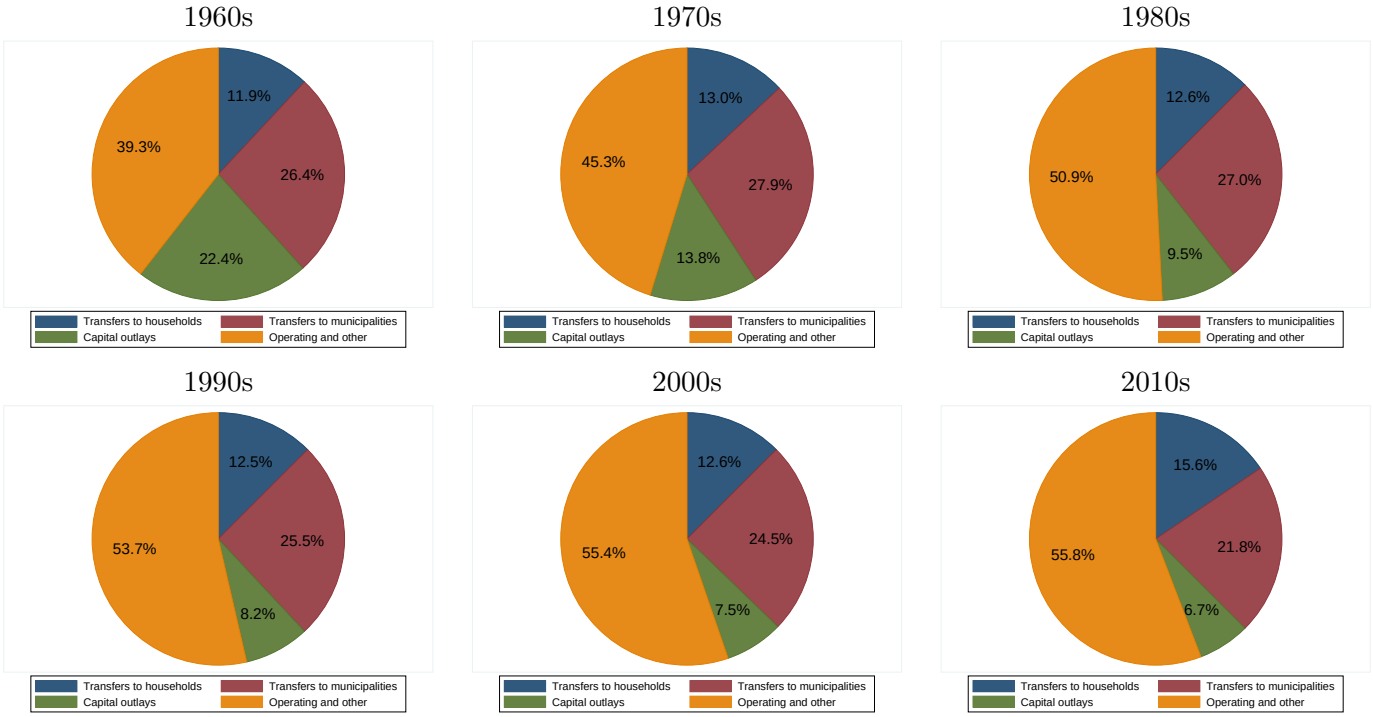


Figure A.5: Total expenditure components: Average by decade, 1963–2014

		Main sample	Main sample with close elections			Dem=Rep
	1963-2014	1983-2014	Within 5pp.	Dem<5pp.	Rep<5pp.	p-val
Debt per capita mean	2121.8	2811.6	3090.5	2825.8	3290.0	1.0
standard deviation	817.6	660.5	559.2	348.4	585.8	.
Debt growth mean	-0.8	0.0	-0.3	-0.3	-0.3	0.9
standard deviation	11.4	8.3	7.4	7.4	7.2	.
Population mean	5177.1	5777.4	6284.7	5768.7	6673.8	0.7
standard deviation	1301.4	827.7	634.8	157.8	479.0	.
Population growth mean	1.1	1.0	0.9	1.0	0.8	0.9
standard deviation	0.7	0.6	0.6	0.8	0.4	.
Expenditure growth mean	3.2	2.6	2.5	2.6	2.5	0.1
standard deviation	3.8	3.5	3.5	3.4	3.4	.
Income sales tax rev growth mean	2.9	2.1	2.5	2.4	2.6	1.0
standard deviation	5.3	4.6	4.3	3.9	3.3	.
Net general rev growth mean	3.0	2.2	2.6	2.7	2.6	0.4
standard deviation	6.8	3.9	3.6	3.3	2.8	.
Tax rev growth mean	2.6	2.0	2.5	2.4	2.5	0.8
standard deviation	4.8	4.5	4.2	3.7	3.3	.
Overall GDP growth mean	2.1	1.9	2.0	1.8	2.1	0.8
standard deviation	2.5	2.3	2.1	1.9	1.6	.
Private GDP growth mean	2.1	1.9	2.0	1.8	2.2	0.8
standard deviation	2.9	2.6	2.3	2.2	1.7	.
IG increases mean	5.5	5.0	4.8	4.5	5.0	0.6
standard deviation	5.6	4.9	5.4	4.6	5.8	.
IG decreases mean	-1.9	-1.6	-1.4	-1.5	-1.4	1.0
standard deviation	3.4	3.2	2.8	2.7	2.7	.
Observations	2439.0	1508.0	321.0	138.0	183.0	.

Population in 1,000s. Debt per capita in 2012 dollars. All other variables, except for population growth, also in real per capita terms. p-values based on standard errors clustered by state and year after removing state and year fixed effects. The 5 pp. MOV includes two observations that drop out in the presence of these fixed effects. Standard deviations are after taking out state and year fixed effects.

Table A.3: Descriptive statistics: Means and standard deviations

A.4 Additional Variable Definitions

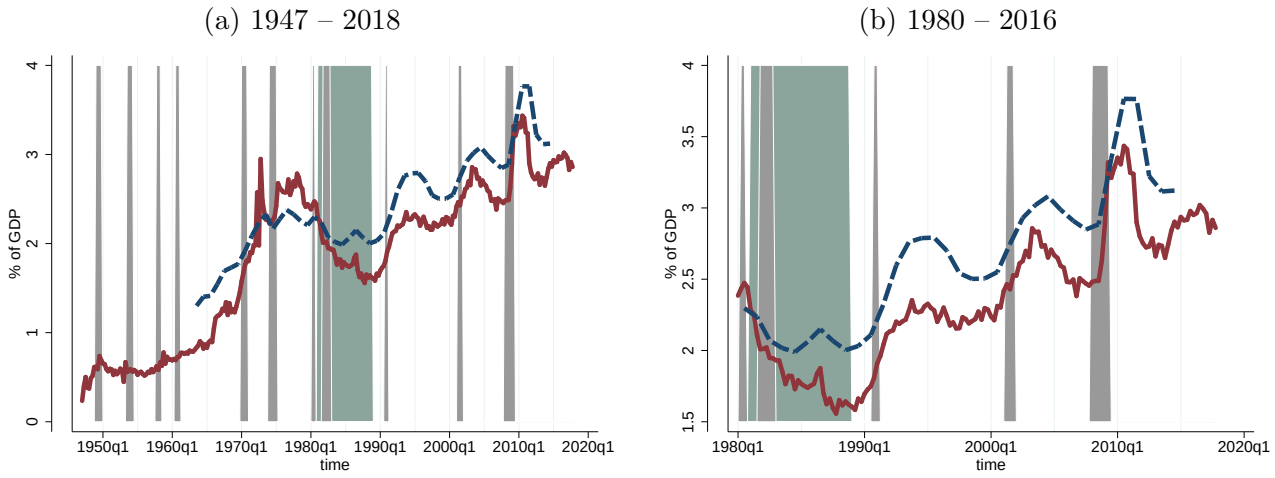
Variables used in the analysis of state level panel data:

- Annual GDP deflator: FRED label `A191RD3A086NBEA`).
- Personal Income: BEA Regional Accounts (https://apps.bea.gov/regional/downloadzip.cfm), Table CA4.
- State GDP and its components: BEA Regional Accounts, GDP by State.
- Population: BEA Regional Accounts.

Variables used in the time-series analysis:

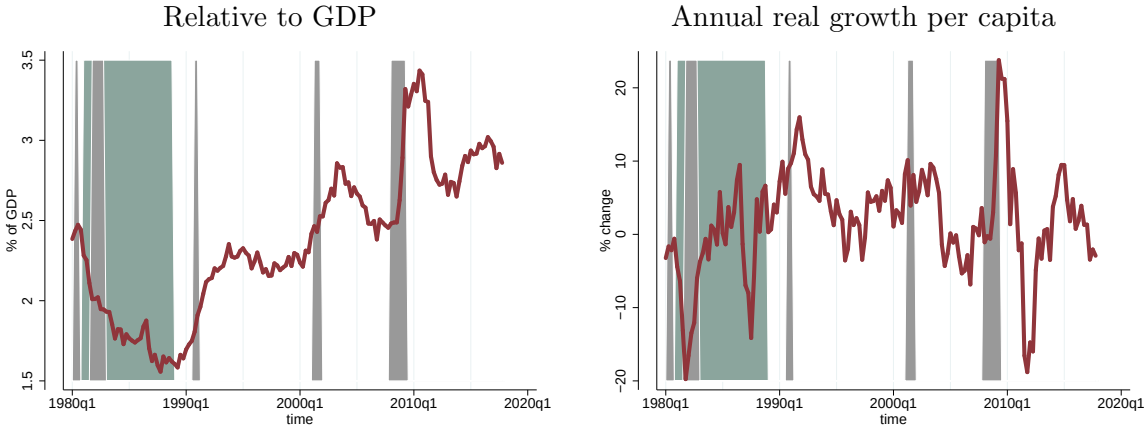
- Civilian population above 16: FRED label `CNP16OV`
- Real government consumption and investment: FRED label `GCEC1`
- Real GDP: FRED label `GDPC1`
- GDP deflator: FRED label `GDPDEF`
- State and local government expenditures: FRED label `SLEXPND`
- Federal transfers to state and local governments: FRED label `FGSL`
- Federal government current transfer receipts from persons: FRED label `B233RC1Q027SBEA`
- Federal government current transfer receipts from business: FRED label `W012RC1Q027SBEA`
- Federal government current transfer payments: FRED label `W014RC1Q027SBEA`
- Federal government current tax receipts: FRED label `W006RC1Q027SBEA`

We define taxes as current tax receipts plus transfer receipts from persons and business minus federal transfers, but plus federal transfers to state and local governments. We smooth the population estimate by initializing population to be the value in the data and then updating population as: $Pop_t = \frac{3}{4}Pop_{t-1} + \frac{1}{4}CNP16OV_t$.



Two types of coverage differences explain the discrepancies: (1) Capital expenditures and state-run unemployment insurance numbers are excluded from NIPA. (2) The Census series does not cover local governments.

Figure A.6: NIPA federal grants-in-aid to state and local governments vs Census intergovernmental transfers to states.



Note: Intergovernmental transfers from the federal government to state and local governments show both cyclical and idiosyncratic patterns. When Reagan came into office, intergovernmental transfers were cut despite the 1981–82 recession. In all other recession since 1980, intergovernmental transfers rose.

$$\Delta \log \frac{IG_t}{GDP_t} = \underset{[0.79]}{0.002} + \underset{[2.04]}{0.025} \times \mathbf{1}\{Recession\}_t - \underset{[2.57]}{0.019} \times \mathbf{1}\{Reagan\}_t, \qquad N = 152.$$

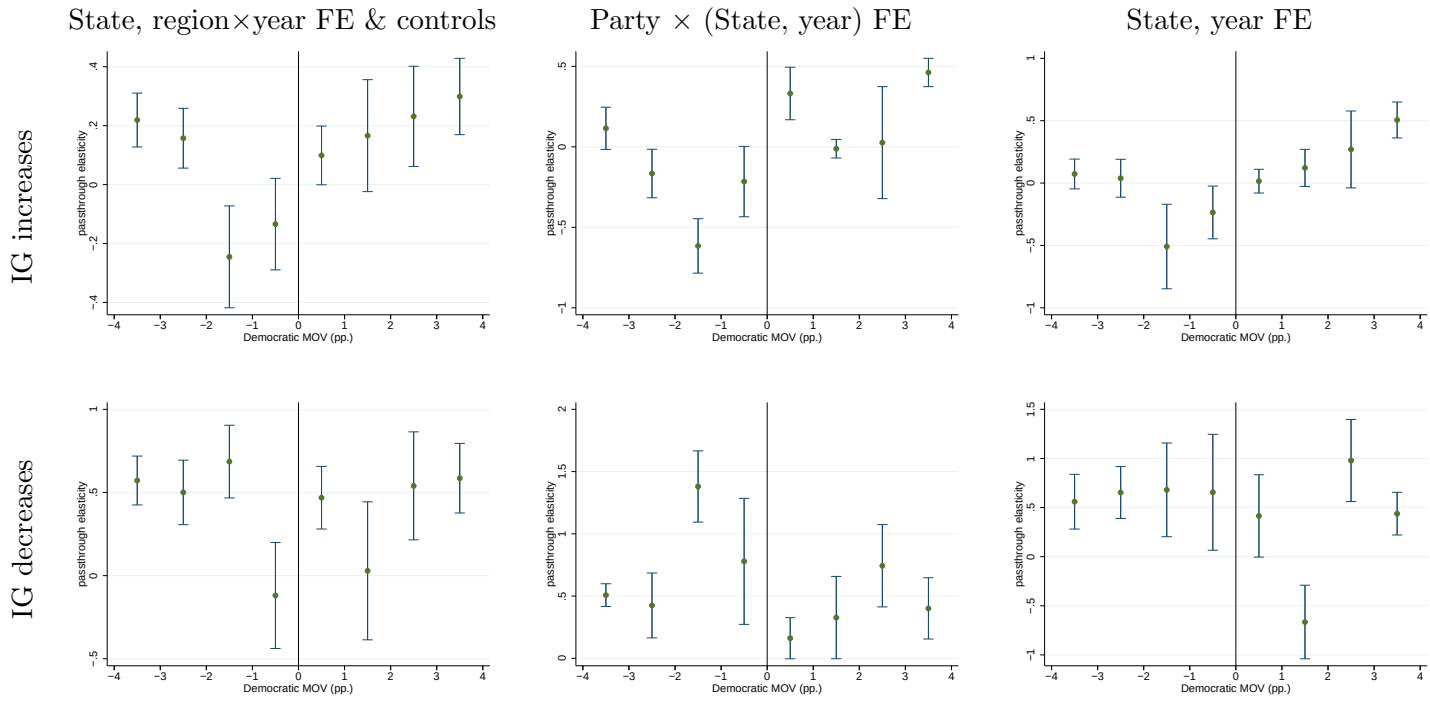
$$\Delta \log \frac{IG_t}{GDP_t} = \underset{[2.03]}{0.008} - \underset{[2.21]}{0.950} \times \Delta \log GDP_t - \underset{[2.04]}{0.015} \times \mathbf{1}\{Reagan\}_t, \qquad N = 152.$$

Robust (absolute) t-statistics in brackets based on Newey-West standard errors with three lags.

Figure A.7: Intergovernmental transfers (Grants-in-aid to state and local governments) since 1980

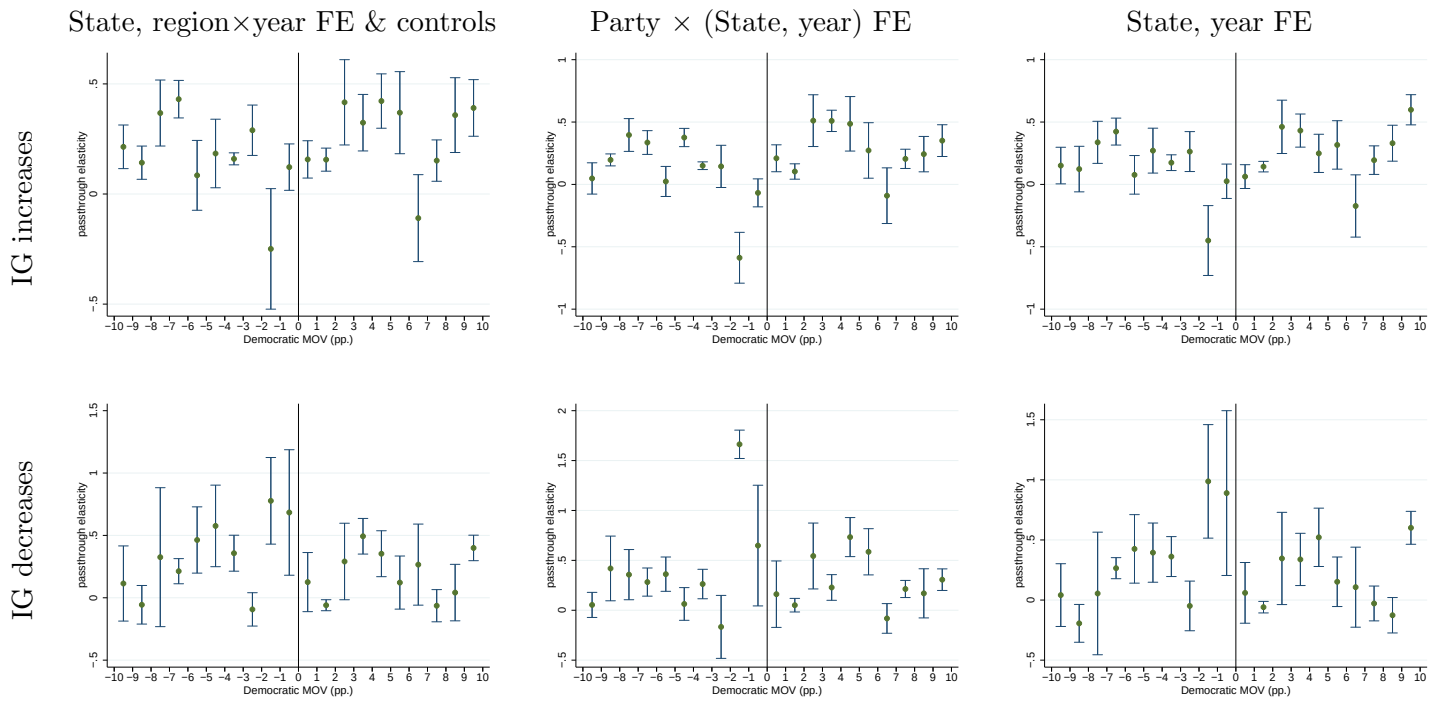
B Additional estimates

B.1 Expenditure growth



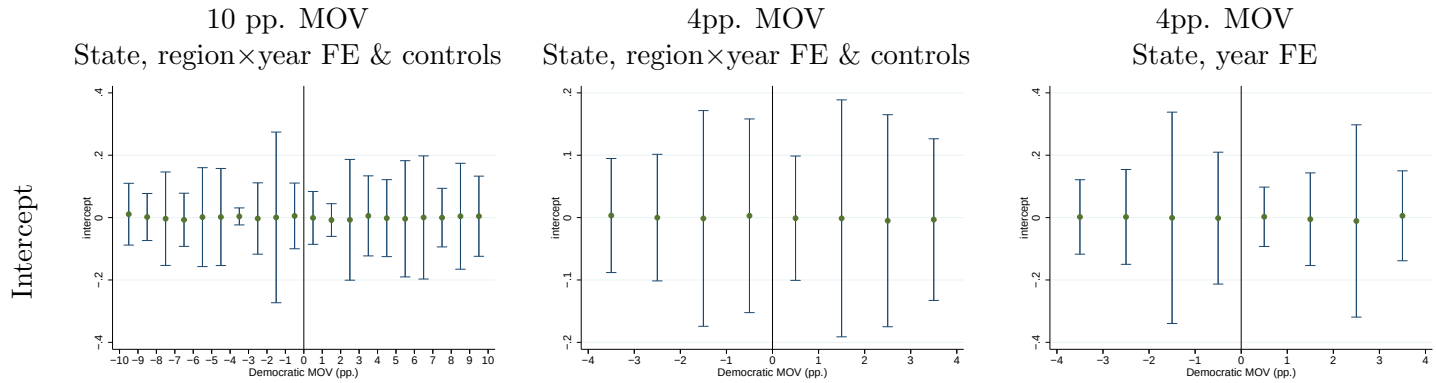
To construct the plots, we first remove fixed effects and, if applicable, controls in the full sample 10pp and 4pp samples, respectively. We then estimate slopes for one percentage point bins. The figures show the estimated slopes and heteroskedasticity-robust ± one standard error. The standard errors are meant to be suggestive only. When we report direct estimates of (4.1), we quantify the uncertainty coming from the controls and fixed effects and clusters standard errors.

Figure B.8: Illustrating our regression discontinuity in slopes: Republican Governors pass less of IG increases on to spending and pass more of IG decreases on to spending cuts. 4pp.



To construct the plots, we first remove fixed effects and, if applicable, controls in the full sample 10pp and 4pp samples, respectively. We then estimate slopes for one percentage point bins. The figures show the estimated slopes and heteroskedasticity-robust ± one standard error. The standard errors are meant to be suggestive only. When we report direct estimates of (4.1), we quantify the uncertainty coming from the controls and fixed effects and clusters standard errors.

Figure B.9: Illustrating our regression discontinuity in slopes: Republican Governors pass less of IG increases on to spending and pass more of IG decreases on to spending cuts. up to 10pp.



To construct the plots, we first remove fixed effects and, if applicable, controls in the full sample 10pp and 4pp samples, respectively. We then estimate slopes for one percentage point bins. The figures show the estimated slopes and heteroskedasticity-robust ± one standard error. The standard errors are meant to be suggestive only. When we report direct estimates of (4.1), we quantify the uncertainty coming from the controls and fixed effects and clusters standard errors.

Figure B.10: Illustrating our regression discontinuity in slopes: No average difference between Republican and Democratic governors.

	(1) 100 pp.	(2) 6 pp.	(3) 5 pp.	(4) 4 pp.	(5) 3 pp.
IG incr.	1.073***	1.558***	1.395***	1.238***	1.341**
	(6.90)	(7.73)	(12.00)	(5.49)	(2.29)
IG decr.	0.346***	0.126	0.836	0.851	0.120
	(3.04)	(0.36)	(1.63)	(1.57)	(0.14)
Rep x IG incr.	0.738***	0.639***	0.649**	0.104	-0.336
	(4.79)	(3.27)	(2.21)	(0.29)	(-0.71)
Rep x IG decr.	1.135***	1.414***	2.051***	1.928***	1.052
	(7.42)	(2.89)	(5.07)	(3.99)	(1.14)
Diff–IG incr.	-0.334**	-0.919***	-0.746**	-1.134***	-1.677**
	(-2.35)	(-3.45)	(-2.14)	(-2.69)	(-2.48)
Diff–IG decr.	0.788***	1.288**	1.215**	1.077**	0.932
	(5.16)	(2.46)	(2.54)	(2.25)	(1.41)
Observations	1499	374	300	239	119

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.4: Expenditure growth: Dollar to dollar pass-through based on Table 2

	(1)	(2)	(3)	(4)	(5)	(6)
IG incr.	0.375***	0.354***	0.338***	0.360***	0.338***	0.339***
	(12.17)	(5.10)	(24.58)	(10.03)	(4.74)	(11.68)
IG decr.	0.166	0.121	0.219*	0.161	0.091	0.201
	(1.66)	(0.92)	(1.74)	(1.67)	(0.69)	(1.61)
Republican Gov.	0.008		0.010	0.008		0.015**
	(1.30)		(1.43)	(1.11)		(2.13)
Rep x IG incr.	-0.259***	-0.235**	-0.176**	-0.241***	-0.222**	-0.182**
	(-4.85)	(-2.28)	(-2.22)	(-3.82)	(-2.07)	(-2.14)
Rep x IG decr.	0.278**	0.356*	0.288**	0.280**	0.393**	0.298**
	(2.53)	(1.93)	(2.37)	(2.53)	(2.15)	(2.55)
R-squared	0.62	0.70	0.75	0.62	0.71	0.75
R-sq, within	0.27	0.25	0.29	0.28	0.26	0.30
Observations	319	313	300	319	313	300
States	43	43	43	43	43	43
Years	32	32	31	32	32	31
StateFE	Yes	By party	Yes	Yes	By party	Yes
YearFE	Yes	By party	By region	Yes	By party	By region
Controls	No	No	No	Yes	Yes	Yes

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.5: Expenditure growth: Various specifications, 5pp MOV

	(1) 100 pp.	(2) 6 pp.	(3) 5 pp.	(4) 4 pp.	(5) 3 pp.
IG incr.	0.267***	0.369***	0.338***	0.307***	0.335**
	(6.67)	(8.72)	(24.58)	(4.91)	(2.28)
IG decr.	0.091***	0.050	0.219*	0.226*	0.075
	(3.29)	(0.64)	(1.74)	(1.80)	(0.41)
Republican Gov.	0.003	0.008	0.010	0.028***	0.000
	(0.91)	(1.27)	(1.43)	(2.89)	(0.00)
Rep x IG incr.	-0.087**	-0.209***	-0.176**	-0.291**	-0.488**
	(-2.67)	(-3.32)	(-2.22)	(-2.72)	(-2.51)
Rep x IG decr.	0.190***	0.289**	0.288**	0.263**	0.160
	(4.67)	(2.31)	(2.37)	(2.21)	(1.07)
Expenditure/IG Rev.	4.01	4.11	4.12	4.08	4.15
R-squared	0.50	0.67	0.75	0.78	0.77
R-sq, within	0.17	0.22	0.29	0.24	0.16
Observations	1499	374	300	239	119
States	48	45	43	40	28
Years	32	32	31	31	27

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.6: Expenditure growth: State FE, Region x Year FE, with controls

	(1)	(2)	(3)	(4)	(5)	(6)
IG incr.	0.430	0.434	0.229	0.385	0.366	0.179
	(1.18)	(1.25)	(0.58)	(1.12)	(1.10)	(0.48)
IG decr.	-0.379	-0.690**	-0.148	-0.381	-0.715**	-0.289
	(-1.08)	(-2.23)	(-0.28)	(-1.05)	(-2.26)	(-0.52)
Republican Gov.	0.089**		0.108**	0.096**		0.104*
	(2.70)		(2.30)	(2.14)		(1.85)
Rep x IG incr.	-0.798*	-0.876*	-0.375	-0.764*	-0.789	-0.323
	(-1.94)	(-1.71)	(-0.63)	(-1.93)	(-1.61)	(-0.54)
Rep x IG decr.	1.221**	2.167***	0.853	1.212**	2.198***	0.901
	(2.60)	(3.16)	(1.58)	(2.45)	(3.15)	(1.64)
R-squared	0.28	0.39	0.51	0.29	0.40	0.52
R-sq, within	0.03	0.04	0.03	0.04	0.06	0.04
Observations	266	259	239	266	259	239
States	41	41	40	41	41	40
Years	32	32	31	32	32	31
StateFE	Yes	By party	Yes	Yes	By party	Yes
YearFE	Yes	By party	By region	Yes	By party	By region
Controls	No	No	No	Yes	Yes	Yes

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.7: Capital Expenditure growth: State FE, Region x Year FE, with controls

	(1)	(2)	(3)	(4)	(5)	(6)
IG incr.	0.686***	0.563**	0.581***	0.651***	0.574**	0.549***
	(3.66)	(2.65)	(2.91)	(3.15)	(2.50)	(2.95)
IG decr.	-0.201	-0.152	-0.059	-0.219	-0.239	-0.082
	(-0.88)	(-0.58)	(-0.20)	(-0.94)	(-0.89)	(-0.26)
Republican Gov.	0.048***		0.045*	0.048**		0.018
	(2.79)		(2.03)	(2.31)		(0.65)
Rep x IG incr.	-0.834***	-0.835***	-0.781***	-0.798***	-0.844***	-0.705***
	(-4.73)	(-3.20)	(-2.95)	(-4.03)	(-3.31)	(-2.81)
Rep x IG decr.	0.817***	1.005**	0.542	0.822***	1.126***	0.481
	(3.32)	(2.72)	(1.67)	(3.40)	(3.12)	(1.58)
R-squared	0.35	0.52	0.60	0.40	0.55	0.63
R-sq, within	0.11	0.09	0.09	0.17	0.15	0.16
Observations	266	259	239	266	259	239
States	41	41	40	41	41	40
Years	32	32	31	32	32	31
StateFE	Yes	By party	Yes	Yes	By party	Yes
YearFE	Yes	By party	By region	Yes	By party	By region
Controls	No	No	No	Yes	Yes	Yes

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.8: Municipal Transfer Expenditure growth: State FE, Region x Year FE, with controls

	(1)	(2)	(3)	(4)	(5)	(6)
IG incr.	0.199	0.301	-0.028	0.266	0.330	0.007
	(0.98)	(1.46)	(-0.12)	(1.54)	(1.62)	(0.03)
IG decr.	0.000	-0.091	0.134	-0.011	-0.068	0.136
	(0.00)	(-0.28)	(0.32)	(-0.04)	(-0.20)	(0.30)
Republican Gov.	0.002		0.007	0.015		0.031
	(0.15)		(0.40)	(0.70)		(1.04)
Rep x IG incr.	-0.223	-0.313	0.224	-0.311	-0.336	0.207
	(-0.86)	(-1.28)	(0.84)	(-1.38)	(-1.34)	(0.79)
Rep x IG decr.	0.358	0.320	-0.035	0.408	0.272	0.026
	(1.11)	(0.53)	(-0.09)	(1.31)	(0.46)	(0.07)
R-squared	0.66	0.69	0.75	0.66	0.70	0.76
R-sq, within	0.02	0.01	0.01	0.04	0.03	0.03
Observations	266	259	239	266	259	239
States	41	41	40	41	41	40
Years	32	32	31	32	32	31
StateFE	Yes	By party	Yes	Yes	By party	Yes
YearFE	Yes	By party	By region	Yes	By party	By region
Controls	No	No	No	Yes	Yes	Yes

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.9: Household Transfer Expenditure growth: State FE, Region x Year FE, with controls

	(1)	(2)	(3)	(4)	(5)	(6)
IG incr.	0.362***	0.319***	0.316***	0.336***	0.308**	0.314***
	(4.79)	(2.80)	(3.11)	(4.55)	(2.59)	(3.01)
IG decr.	0.341**	0.409*	0.435**	0.350**	0.409*	0.427**
	(2.56)	(1.96)	(2.61)	(2.55)	(1.94)	(2.39)
Republican Gov.	-0.000		0.014	-0.005		0.013
	(-0.06)		(1.01)	(-0.54)		(0.84)
Rep x IG incr.	-0.301***	-0.250	-0.137	-0.269***	-0.236	-0.133
	(-3.31)	(-1.58)	(-1.05)	(-2.83)	(-1.49)	(-1.05)
Rep x IG decr.	-0.022	-0.138	0.033	-0.043	-0.137	0.035
	(-0.17)	(-0.62)	(0.21)	(-0.32)	(-0.58)	(0.22)
R-squared	0.58	0.66	0.70	0.59	0.66	0.70
R-sq, within	0.23	0.17	0.26	0.24	0.18	0.26
Observations	266	259	239	266	259	239
States	41	41	40	41	41	40
Years	32	32	31	32	32	31
StateFE	Yes	By party	Yes	Yes	By party	Yes
YearFE	Yes	By party	By region	Yes	By party	By region
Controls	No	No	No	Yes	Yes	Yes

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.10: Other Expenditure growth: State FE, Region x Year FE, with controls

B.2 Revenue growth

	Net general revenue				
	(1) 100 pp.	(2) 6 pp.	(3) 5 pp.	(4) 4 pp.	(5) 3 pp.
Debt	0.029	-0.004	0.055	0.047	0.018
	(1.46)	(-0.08)	(1.57)	(0.85)	(0.40)
GDP growth	0.364***	0.071	-0.074	-0.016	-0.298
	(4.78)	(0.48)	(-0.39)	(-0.07)	(-1.18)
Rep x Debt	0.018	0.036	-0.066	-0.063	-0.155**
	(0.59)	(0.68)	(-1.27)	(-1.05)	(-2.23)
Rep x Growth	-0.031	0.173	0.209	0.333	0.878**
	(-0.55)	(0.67)	(0.97)	(1.27)	(2.50)
Republican Gov.	0.003	-0.000	0.007	0.005	0.000
	(0.80)	(-0.04)	(0.66)	(0.42)	(0.00)
IG incr.	0.074	0.099*	0.155**	0.169**	0.171*
	(1.63)	(1.99)	(2.36)	(2.50)	(1.72)
IG decr.	-0.030	-0.083	-0.127	-0.189	-0.383**
	(-1.02)	(-0.97)	(-0.78)	(-1.09)	(-2.60)
Rep x IG incr.	-0.026	-0.118	-0.176**	-0.174	-0.131
	(-0.52)	(-1.53)	(-2.08)	(-1.63)	(-0.70)
Rep x IG decr.	0.018	0.183	0.348**	0.293*	0.127
	(0.38)	(1.59)	(2.43)	(1.94)	(0.92)
R-squared	0.48	0.66	0.73	0.77	0.86
R-sq, within	0.05	0.04	0.06	0.09	0.31
Observations	1499	374	300	239	119
States	48	45	43	40	28
Years	32	32	31	31	27

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.11: Growth of net general revenue components: State FE, Region x Year FE, with controls

	Tax revenue				
	(1) 100 pp.	(2) 6 pp.	(3) 5 pp.	(4) 4 pp.	(5) 3 pp.
Debt	0.015	0.036	0.061	0.090	0.046
	(0.68)	(1.03)	(1.52)	(1.26)	(0.90)
GDP growth	0.510***	-0.006	-0.159	-0.153	-0.346
	(5.69)	(-0.04)	(-0.75)	(-0.70)	(-1.67)
Rep x Debt	0.015	-0.003	-0.057	-0.095	-0.123**
	(0.47)	(-0.05)	(-0.98)	(-1.32)	(-2.34)
Rep x Growth	-0.007	0.329	0.340	0.539*	1.273***
	(-0.09)	(1.47)	(1.38)	(2.00)	(3.72)
Republican Gov.	-0.002	0.005	0.012	0.013	0.000
	(-0.26)	(0.56)	(1.18)	(0.93)	(0.00)
IG incr.	0.047	0.114**	0.175**	0.201***	0.144
	(1.01)	(2.28)	(2.44)	(3.40)	(1.27)
IG decr.	-0.080*	-0.198**	-0.173	-0.276*	-0.349**
	(-1.86)	(-2.44)	(-1.11)	(-1.75)	(-2.77)
Rep x IG incr.	0.059	-0.176**	-0.217**	-0.220*	-0.162
	(0.80)	(-2.59)	(-2.15)	(-1.73)	(-0.97)
Rep x IG decr.	0.045	0.317***	0.463***	0.414**	0.338***
	(0.54)	(2.95)	(2.86)	(2.17)	(2.81)
R-squared	0.49	0.66	0.71	0.79	0.90
R-sq, within	0.07	0.05	0.07	0.13	0.39
Observations	1499	374	300	239	119
States	48	45	43	40	28
Years	32	32	31	31	27

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.12: Growth of tax revenue components: State FE, Region x Year FE, with controls

	Income & sales tax				
	(1) 100 pp.	(2) 6 pp.	(3) 5 pp.	(4) 4 pp.	(5) 3 pp.
Debt	0.034	0.079**	0.099*	0.094	0.045
	(1.42)	(2.06)	(1.97)	(1.38)	(0.91)
GDP growth	0.371***	-0.070	-0.162	-0.197	-0.336*
	(4.90)	(-0.43)	(-0.81)	(-0.92)	(-1.80)
Rep x Debt	-0.001	-0.035	-0.048	-0.063	-0.127**
	(-0.03)	(-0.60)	(-0.72)	(-0.87)	(-2.45)
Rep x Growth	0.024	0.267	0.225	0.565**	1.294***
	(0.26)	(1.11)	(0.99)	(2.40)	(4.09)
Republican Gov.	-0.003	0.009	0.021*	0.017	0.000
	(-0.49)	(0.83)	(1.88)	(1.03)	(.)
IG incr.	0.039	0.094	0.195**	0.224***	0.144
	(0.74)	(1.35)	(2.20)	(3.22)	(1.17)
IG decr.	-0.101**	-0.161*	-0.195	-0.282	-0.339**
	(-2.27)	(-1.86)	(-1.16)	(-1.64)	(-2.42)
Rep x IG incr.	0.088	-0.133*	-0.273**	-0.234*	-0.103
	(1.07)	(-1.70)	(-2.58)	(-1.74)	(-0.64)
Rep x IG decr.	0.053	0.332***	0.548***	0.490**	0.515***
	(0.58)	(2.94)	(2.90)	(2.41)	(4.11)
R-squared	0.51	0.70	0.73	0.79	0.91
R-sq, within	0.05	0.06	0.09	0.15	0.44
Observations	1499	374	300	239	119
States	48	45	43	40	28
Years	32	32	31	31	27

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.13: Growth of income and sales tax revenue growth: State FE, Region x Year FE, with controls

B.3 Private sector activity

	Future $(t+\frac{1}{2})$ private GDP		Future $(t+\frac{1}{2})$ private profits		Future $(t+\frac{1}{2})$ private compensation	
	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.	(1) 100 pp.	(4) 4 pp.
Debt	-0.022	-0.042	-0.041	-0.059	-0.008	-0.037*
	(-1.02)	(-1.30)	(-1.37)	(-1.04)	(-0.46)	(-1.78)
GDP growth	0.174***	0.043	0.165	-0.029	0.205***	-0.006
	(2.85)	(0.28)	(1.62)	(-0.09)	(3.83)	(-0.07)
Rep x Debt	0.023	0.025	0.040	0.013	0.008	0.016
	(0.80)	(0.63)	(1.04)	(0.17)	(0.35)	(0.49)
Rep x Growth	-0.019	-0.040	-0.042	-0.113	-0.012	0.114
	(-0.56)	(-0.26)	(-0.85)	(-0.38)	(-0.25)	(1.44)
Republican Gov.	0.000	-0.009	0.001	-0.015	-0.001	-0.007
	(0.04)	(-0.90)	(0.34)	(-0.89)	(-0.63)	(-1.32)
IG incr.	0.020*	-0.142**	0.035	-0.249**	0.009	-0.115**
	(1.70)	(-2.66)	(1.66)	(-2.40)	(0.62)	(-2.26)
IG decr.	-0.054***	0.176***	-0.103***	0.320***	-0.015	0.055
	(-2.76)	(3.10)	(-2.96)	(3.15)	(-0.87)	(1.30)
Rep x IG incr.	-0.000	0.152**	-0.006	0.262**	0.004	0.134**
	(-0.02)	(2.71)	(-0.16)	(2.17)	(0.23)	(2.18)
Rep x IG decr.	0.029	-0.119**	0.069	-0.066	-0.007	-0.145***
	(1.33)	(-2.32)	(1.51)	(-0.58)	(-0.23)	(-3.16)
R-squared	0.50	0.78	0.31	0.66	0.73	0.88
R-sq, within	0.03	0.10	0.01	0.11	0.06	0.12
Observations	1499	239	1499	239	1499	239
States	48	40	48	40	48	40
Years	32	31	32	31	32	31

t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

Table B.14: Per capita real private GDP growth and its components: State FE, Region x Year FE, with controls

C Model Appendix [under construction]

Nominal federal budget

$$(1-\gamma_f)(P_{H,t}nG_{fed,t} + P_{F,t}(1-n)G_{fed,t} + IG_t) = \tau_{fed,t}(nW_tN_t + (1-n)W_t^*N_t^*) \tag{C.1}$$

Real federal purchases

$$\ln G_{fed,t} = (1-\rho_{G,fed})\ln(\bar{G}_{fed}) + \rho_{G,fed})\ln G_{fed,t-1} + \omega_{G,fed}\epsilon_{G,fed,t}. \tag{C.2}$$

Nominal federal transfers:

$$\ln IG_t = (1-\rho_{IG})\ln(\overline{IG}) + \rho_{IG})\ln IG_{t-1} + \omega_{IG}\epsilon_{IG,t}. \tag{C.3}$$

Monetary policy for log of nominal rate:

$$r_{n,t} = \rho_r r_{n,t-1} + (1-\rho_r)(\ln \bar{r}_n + \psi_{r,\pi}\pi_t^{agg} + \psi_{r,y}(\ln y_t^{agg} - \ln \bar{y}^{agg})) \tag{C.4}$$

Aggregate GDP

$$y_{t,agg} = ny_t + (1-n)y_t^*$$

Aggregate inflation

$$\pi_{agg,t} = n\pi_t + (1-n)\pi_t^*$$

Home Euler equation [with nominal SDF]

$$1 = \mathbb{E}_t[M_{t+1}e^{r_{n,t}}] \qquad \text{or} \qquad 1 = \mathbb{E}_t[M_{t+1}e^{r_{n,t}}]\left(1 + \left(\frac{nb_t}{(1-n)b_t^*}\right)^{-\eta_b}\right)$$

In the incomplete markets world, a positive net foreign asset position lowers the return to the household. For this to be resource-neutral, it should be in terms of total holdings, though.

Foreign Euler equation [usually redundant]

$$1 = \mathbb{E}_t[M_{t+1}e^{r_{n,t}}]\left(1 - \left(\frac{nb_t}{(1-n)b_t^*}\right)^{-\eta_b}\right)$$

Home nominal SDF

$$M_t = \beta\frac{u_{c,t}}{u_{c,t-1}}e^{-\pi_t}$$

Foreign nominal SDF [redundant with full risk sharing]

$$M_t^* = \beta\frac{u_{c,t}^*}{u_{c,t-1}^*}e^{-\pi_t}\frac{X_{t-1}}{X_t}$$

Risk-sharing / Backus-Smith with equal initial financial wealth

$$X_t = \frac{u_{c,t}^*}{u_{c,t}}$$

Home price index for private sector [normalized to unity]

$$1 = \left(\phi_H P_{H,t}^{1-\eta} + (1-\phi_H) P_{F,t}^{1-\eta}\right)^{\frac{1}{1-\eta}}$$

Home price index for state government

$$P_{G,t} = \left(\phi_G P_{H,t}^{1-\eta} + (1-\phi_G) P_{F,t}^{1-\eta}\right)^{\frac{1}{1-\eta}}$$

Foreign price index for private sector

$$X_t = \left(\phi_H^* P_{H,t}^{1-\eta} + (1-\phi_H^*) P_{F,t}^{1-\eta}\right)^{\frac{1}{1-\eta}}$$

Home cost minimization / markups

$$\frac{W_t}{S_t} = (1-\alpha)\frac{Y_t}{N_t}$$

Foreign

$$\frac{W_t^*}{S_t^*} = (1-\alpha)\frac{Y_t^*}{N_t^*}$$

Definition of inflation [given normalization of price level]

$$\frac{\pi_{H,t}}{\pi_t} = \frac{P_{H,t}}{P_{H,t-1}}$$

Home production

$$y_t = ((K_{t-1}\nu_t)^\alpha N_t^{1-\alpha})^{1-\zeta} K_{G,t-1}^\zeta.$$

Home private capital law of motion

$$K_t = (1-\delta)K_{t-1} + I_t\left(1 - \frac{\kappa_I}{2}\left(\frac{I_t}{I_{t-1}} - 1\right)^2\right)$$

Home private investment FOC

$$1 = Q_t\left(1 - \frac{\kappa_I}{2}\left(\frac{I_t}{I_{t-1}} - 1\right)^2 - \kappa_I \frac{I_t}{I_{t-1}}\left(\frac{I_t}{I_{t-1}} - 1\right)\right) + \mathbb{E}_t\left[M_{t+1}\pi_{t+1}Q_{t+1}\kappa_I\left(\frac{I_{t+1}}{I_t} - 1\right)\left(\frac{I_{t+1}}{I_t}\right)^2\right]$$

Capital demand:

$$\frac{R_{k,t}}{S_t} = \alpha\frac{y_t}{k_{t-1}\nu_t}$$

Real state purchases – exogenous

$$\ln G_{s,t}^x = (1-\rho_{G,s})\ln(\bar{G}_s^x) + \rho_{G,s}\ln G_{s,t-1}^x + \omega_{G,s}\epsilon_{G,s,t}. \tag{C.5}$$

Real state purchases – endogenous

$$G^e_{s,t} = \bar{G}^e_s + \psi_{G,y}(y_{s,t} - \bar{y}) + \psi_{G,IG}(I_t - \overline{IG}) \tag{C.6}$$

Calvo denominator

$$\mathbf{D}_t = y_t + \xi \mathbb{E}_t[M_{t+1}\mathbf{D}_{t+1}\pi_{t+1}]$$

Calvo numerator

$$\mathbf{C}_t = y_t S_t + \xi \mathbb{E}_t[M_{t+1}\mathbf{C}_{t+1}\pi_{t+1}]$$

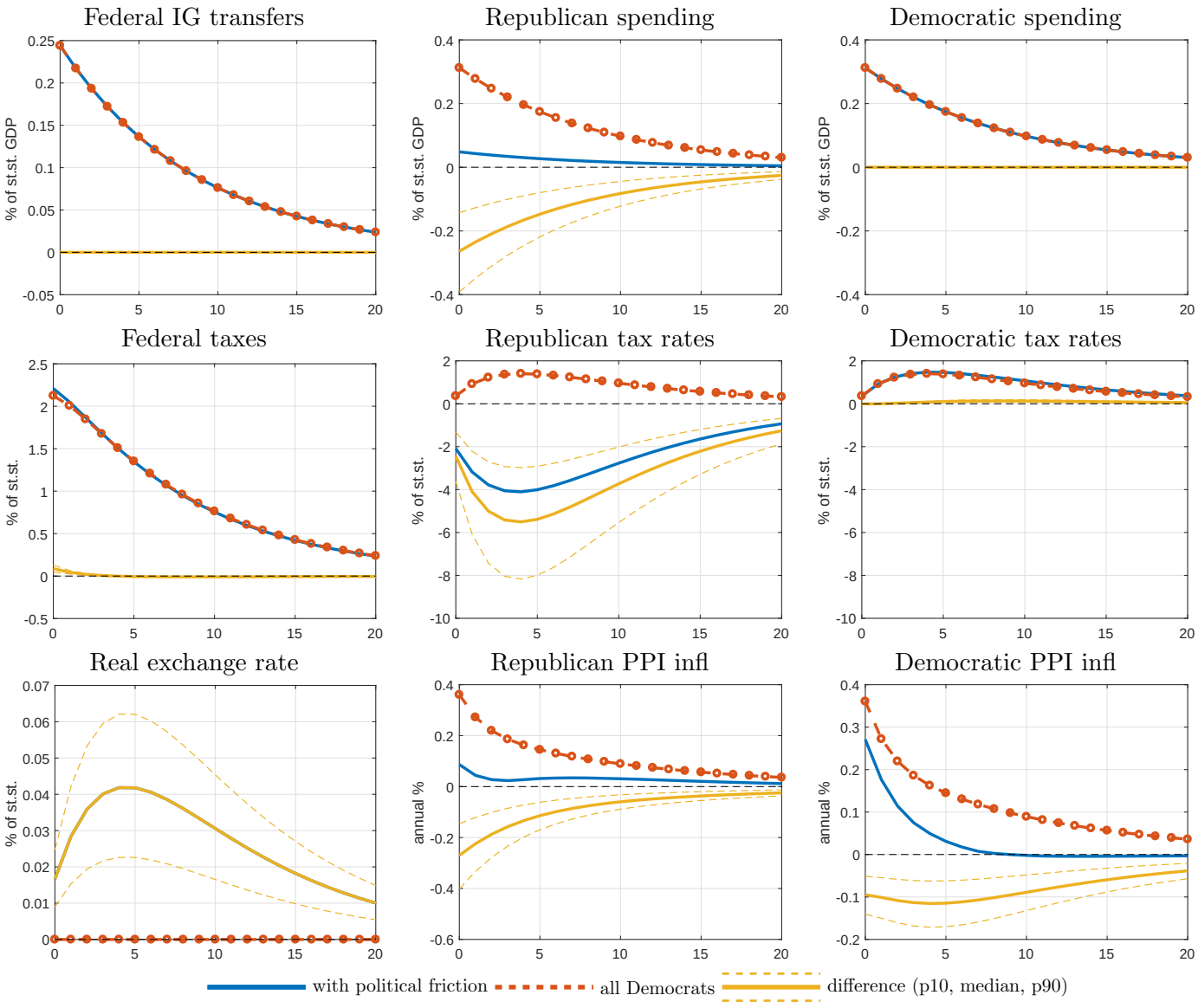


Figure C.11: IRFS: Initial shock, fiscal policy responses, and price effects with state consumption as a substitute

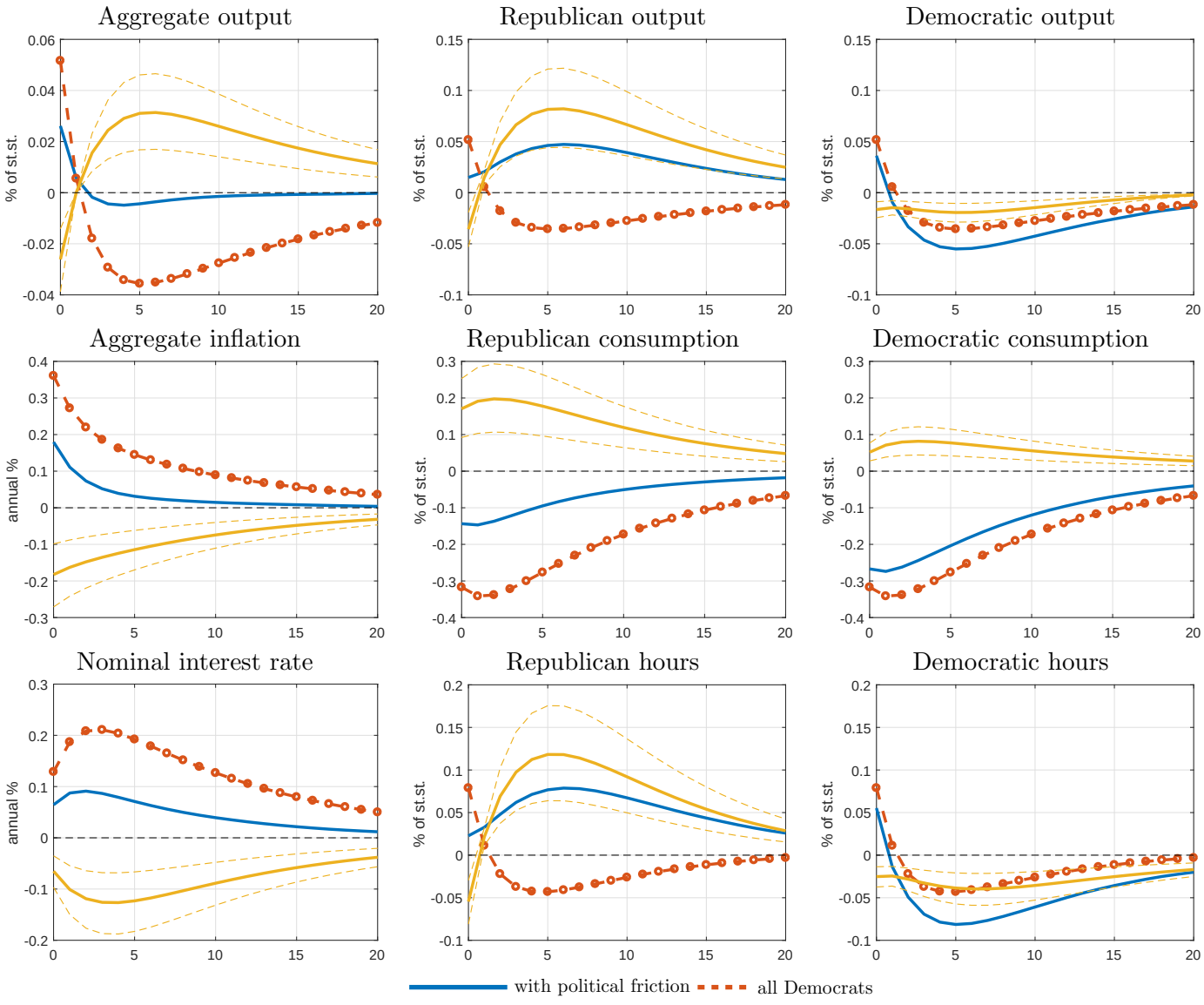


Figure C.12: IRFs: output, consumption, and hours with state consumption as a substitute

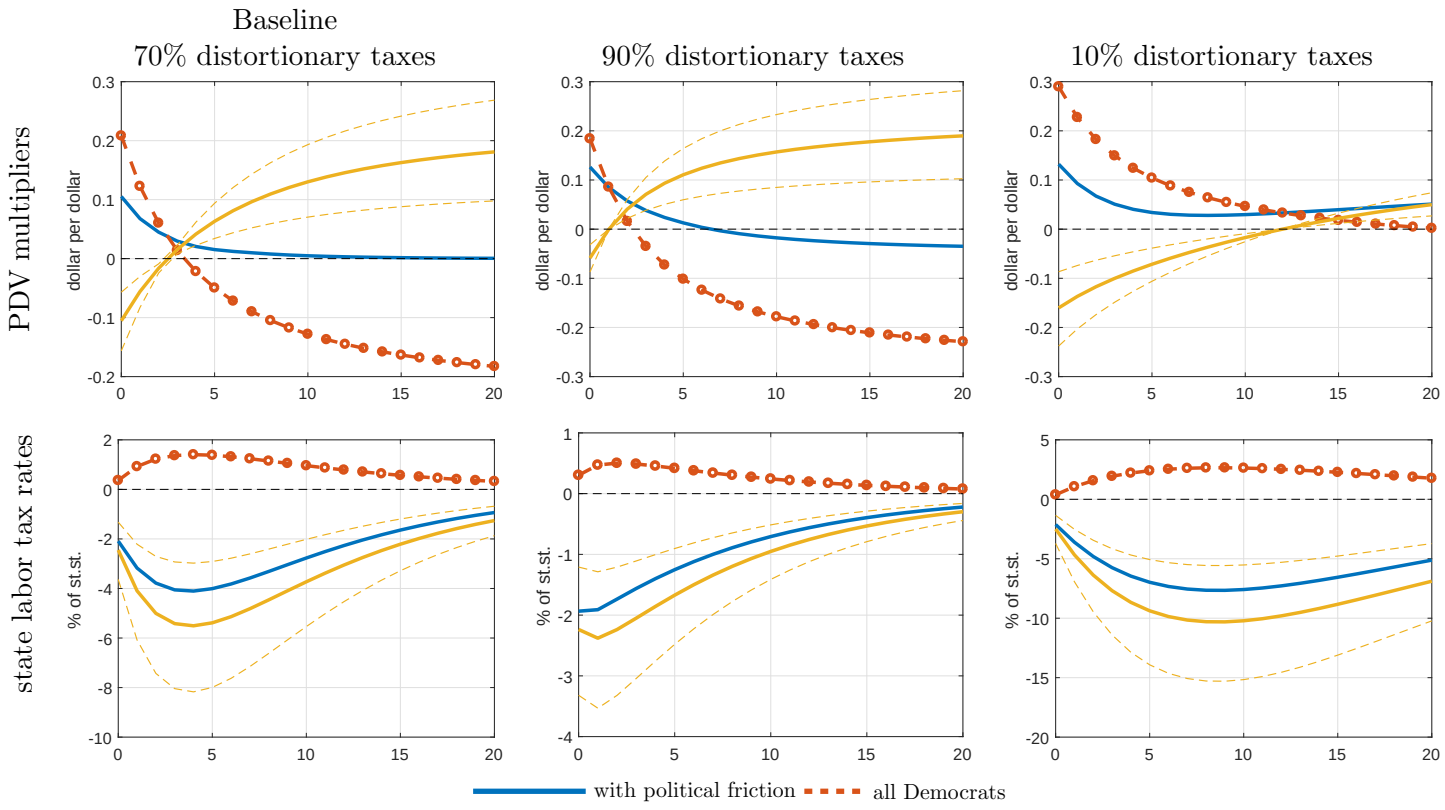


Figure C.13: PDV multipliers and distortionary taxes with state consumption as a substitute

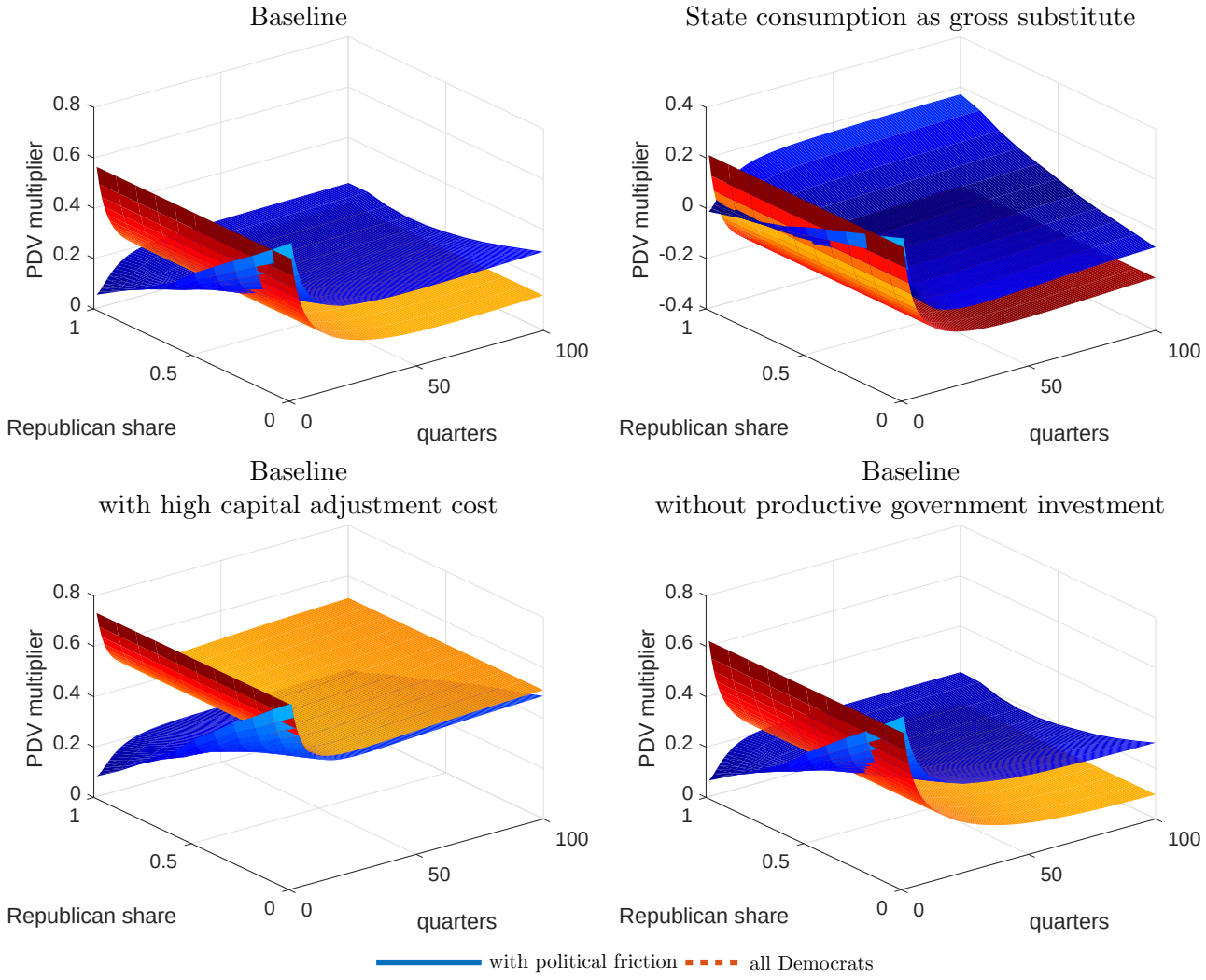


Figure C.14: PDV multipliers over time and as a function of the share of Republican governors: Public consumption as a gross substitute

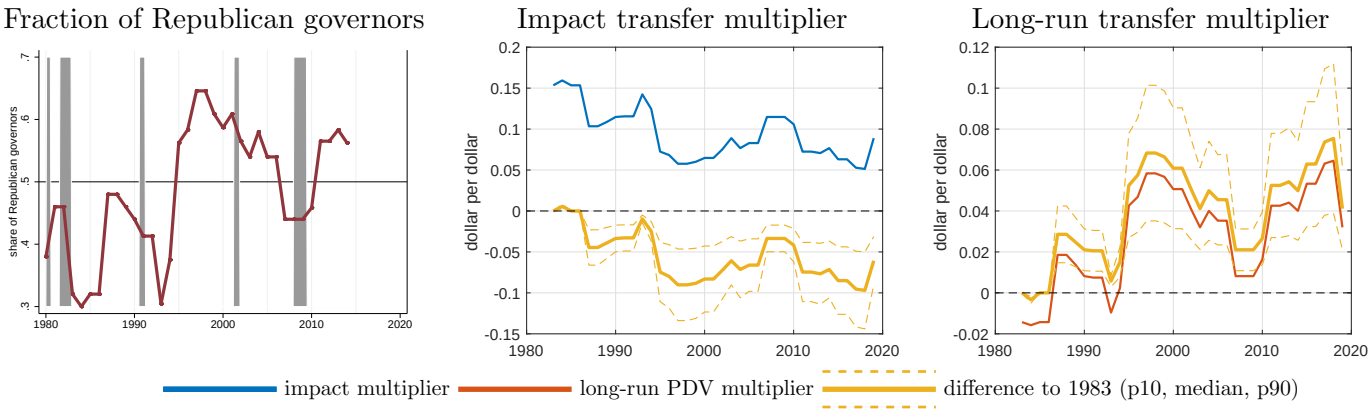
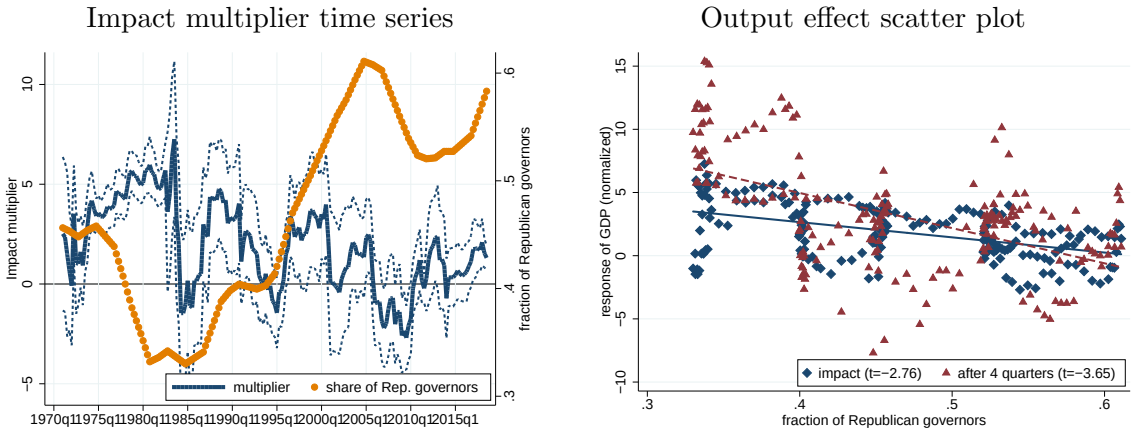


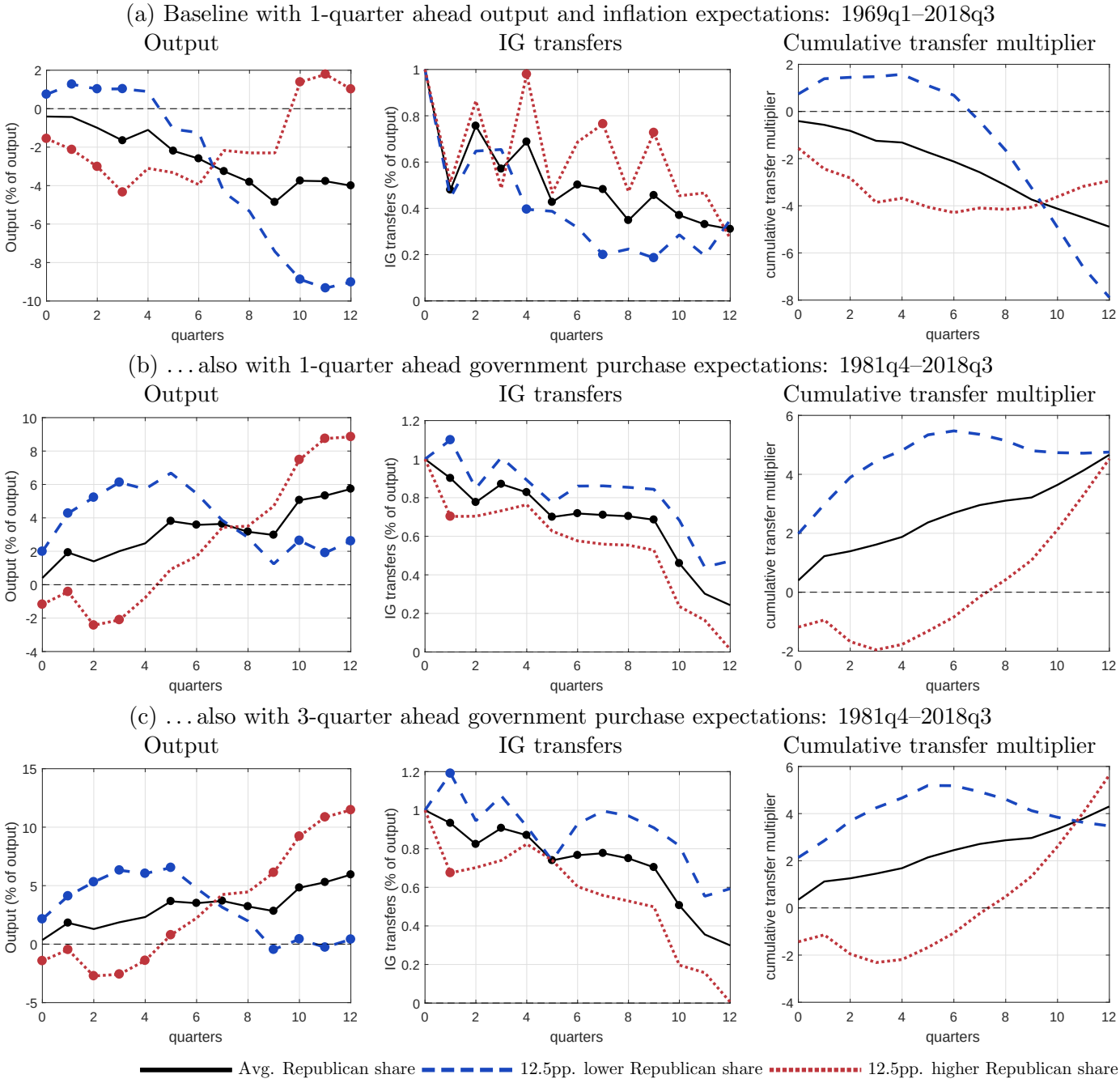
Figure C.15: Party control and transfer multipliers over time: State consumption as a substitute

D Additional time series estimates



Note: Only the impact response has the interpretation of a multiplier; the four-quarter ahead result is the cumulative effect on GDP relative to the impact effect on IG.

Figure D.16: Reduced-form 10-year rolling window output effects of IG transfers and share of Republican governors.



For the output and IG transfer IRF, filled markers denote significance at the 10% level or higher. Inference based on Newey-West heteroskedasticity and autocorrelation robust standard errors with two more lags than the response horizon. For the deviations from the baseline, the markers indicate significant differences from the baseline. For the cumulative multiplier, the figure shows point estimates only. Panel (a) adds the (lagged) one quarter ahead real GDP growth and GDP inflation expectations to the variables in the baseline model in Figure 12. Panel (b) additionally includes the (lagged) one quarter ahead real growth in federal government purchases and in state and local government purchases. Panel (c) also adds the (lagged) three quarter ahead real growth in federal government purchases and in state and local government purchases. In all three cases, we also add the interactions with the lagged share of Republican governors.

Figure D.17: Responses to innovations in intergovernmental transfer: Direct regressions with controls for expectations

(a) Intergovernmental transfers on GDP

	Impact	h=1	h=2	h=3	h=4
Intergov. Transfers (IG)	-0.008	-0.007	-0.023	-0.027	-0.017
	(-0.80)	(-0.42)	(-1.08)	(-1.29)	(-0.71)
Fraction Rep Gov x IG	-0.176**	-0.325*	-0.476**	-0.542**	-0.495*
	(-2.08)	(-1.92)	(-2.50)	(-2.33)	(-1.88)
Fraction Rep Gov.	0.892	1.709	2.745	3.347	4.202
	(1.26)	(1.22)	(1.39)	(1.38)	(1.56)
R-squared	1.00	1.00	1.00	0.99	0.99
Observations	219	218	217	216	215

(b) Intergovernmental transfers on IG transfers

	Impact	h=1	h=2	h=3	h=4
Intergov. Transfers (IG)	1.000	0.532***	0.837***	0.668***	0.806***
		(2.76)	(6.82)	(3.45)	(4.70)
Fraction Rep Gov x IG	0.000	-0.309	0.558	-0.752	1.708
		(-0.38)	(0.84)	(-0.75)	(1.35)
Fraction Rep Gov.	0.000	-2.243	-0.784	-2.445	0.814
		(-0.52)	(-0.12)	(-0.27)	(0.07)
R-squared	1.00	0.99	0.99	0.98	0.97
Observations	219	218	217	216	215

(c) Government purchases on GDP

	Impact	h=1	h=2	h=3	h=4
Gov. purchases (G)	0.153**	0.077	0.105	0.022	0.032
	(2.21)	(0.76)	(0.77)	(0.14)	(0.17)
Fraction Rep Gov x G	-0.365	-0.664	-0.101	0.183	0.625
	(-0.48)	(-0.66)	(-0.08)	(0.14)	(0.42)
Fraction Rep Gov.	0.605	1.390	2.423	3.090	4.009
	(0.81)	(0.98)	(1.22)	(1.28)	(1.47)
R-squared	1.00	1.00	1.00	0.99	0.99
Observations	219	218	217	216	215

(d) Government purchases on purchases

	Impact	h=1	h=2	h=3	h=4
Gov. purchases (G)	1.000	1.039***	1.091***	1.183***	1.306***
		(13.06)	(7.84)	(7.36)	(7.17)
Fraction Rep Gov x G	0.000	-0.097	0.574	1.502	1.257
		(-0.17)	(0.66)	(1.32)	(0.97)
Fraction Rep Gov.	0.000	1.584	3.775**	5.904**	8.036***
		(1.60)	(2.07)	(2.45)	(3.02)
R-squared	1.00	0.99	0.98	0.98	0.97
Observations	219	218	217	216	215

Inference based on Newey-West heteroskedasticity and autocorrelation robust standard errors with six lags. Coefficients on control variables omitted. Standard errors on impact in panels (b) and (d) are not well defined since the equation fits perfectly.

Table D.15: Reduced-form output effects of innovations to government spending and share of Republican governors: Direct regression with single lag for various horizons.