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Evidence from U.S. States

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WP 20-20

PUBLISHED

June 2020

REVISED

June 2023

ISSN: 1962-5361

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DOI: <https://doi.org/10.21799/frbp.wp.2020.20>

Partisanship and Fiscal Policy in Economic Unions: Evidence from U.S. States

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January 2023

Abstract

Partisanship of state governors affects the efficacy of U.S. federal fiscal policy. Using close election data, we find partisan differences in the marginal propensity to spend federal intergovernmental transfers: Republican governors spend less than Democratic governors. Correspondingly, Republican-led states have lower debt, (delayed) lower taxes, and initially lower economic activity. A New Keynesian model of partisan states in a monetary union implies sizable aggregate effects: The intergovernmental transfer impact multiplier rises by 0.58 if Republican governors spend like Democratic governors, but due to delayed tax cuts, the long-run multiplier is higher with more Republican governors, generating an intertemporal policy trade-off.

Keywords: partisanship, flypaper effect, intergovernmental transfers, fiscal multiplier, monetary union, regression discontinuity.

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

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1 Introduction

The United States and many other important democracies are economic unions: collections of politically independent but economically integrated state or provincial economies.¹ While the national government provides for aggregate income, price stability and funding for national public goods, state and local governments are often responsible for implementing national policies. Examples include health care, higher education, national infrastructure, and unemployment and income insurance. The political independence of the subnational governments creates a principal-agent problem between separately elected national and state elected officials. The national government is the “principal” who funds these services via intergovernmental (IG) transfers, while state governments act as “agents” who provide the funded services.² The agency problem is well documented: States spend much of IG transfers (the so-called flypaper effect), but not necessarily as intended by the federal government [Nicholson-Crotty \(2004\)](#). We examine whether partisan preferences over spending and tax relief affect this principal-agent relationship and show that the strength of the flypaper effect varies by political party. We then study how these partisan differences influence the aggregate impact of federal IG transfers, a crucial tool for stimulating the national economy.

The importance of IG transfers has grown over time, and particularly during severe downturns. [Figure 1](#) illustrates the growth of IG transfers in the U.S. since 1929, rising from just 0.1% of GDP to as much as 4.7% of GDP during the COVID-19 pandemic. In 2021, U.S. federal transfers to state and local governments were 15.5% of the federal budget; in the prior decade it averaged 12.8%. Temporary increases in federal aid often occur during recessions (see the shaded recession bands in [Figure 1](#)). The Great Depression saw the introduction of nationally funded but state-administered transfer programs to lower-income households and the unemployed. The importance of IG aid as a stimulus for the macro economy was also evident in the 2009 American Recovery and Reinvestment Act (ARRA) as a response to the Great Recession. ARRA reserved \$318 billion of its \$796 billion in aggregate economic stimulus for allocation by U.S. states and localities. In total, the Coronavirus Aid, Relief, and Economic Security Act and the Consolidated Appropriations Act appropriated \$278 billion to state and local governments. In 2021, the Democratic-controlled Congress approved the American Rescue Plan and allocated another \$350 billion to IG aid. Apparently aware of potential partisan differences in how that aid might be allocated, the bill included an explicit provision meant to prevent states from using the new IG aid for tax relief; see [The WSJ](#)

¹In addition to the U.S., economic unions include Argentina, Australia, Brazil, Canada, India, South Africa, and, for most policies, the European Union and several of its member countries.

²There is a large literature on the theory of optimal intergovernmental transfers beginning with [Musgrave \(1959\)](#), [Oates \(1972\)](#), and summarized in ([Inman and Rubinfeld, 2020](#), ch. 8)

Editorial Board (2021).

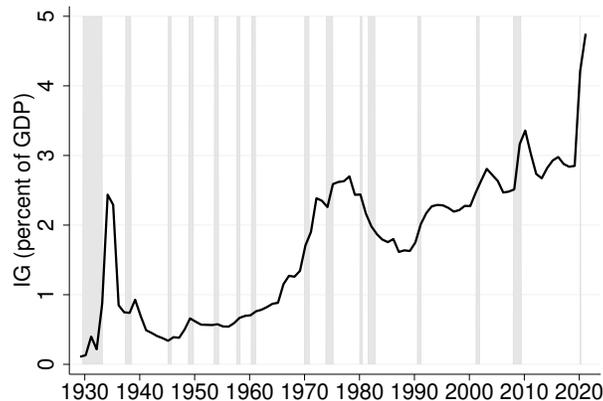


Figure 1: U.S. Federal Intergovernmental Transfers

Notes: U.S. federal intergovernmental transfers as percentage of GDP, 1929 to 2021. Data from [U.S. Bureau of Economic Analysis \(n.d.a\)](#). Gray shaded areas indicate NBER recession dates.

Recent theoretical work has stressed the importance of IG transfers for aggregate fiscal policy in currency unions, see [Galí and Monacelli \(2008\)](#), [Ferrero \(2009\)](#), and [Farhi and Werning \(2017\)](#). Because of free trade, state stabilization policies are likely to have significant consumption spillovers; see [Carlino and Inman \(2013\)](#) and [Auerbach, Gorodnichenko and Murphy \(2020\)](#). This leads to an inefficient under-provision of union-wide expansionary policies if left to state governments. Central government borrowing to finance IG transfers is one policy response. Intergovernmental transfers from the central to union-member governments play two important roles: First, they provide income insurance for residents. Second, they help stabilize the union-wide economy. We focus on the second objective in this paper.

Understanding how IG transfers are allocated to state governments and then how states allocate those transfers is essential for predicting the effects of such aid on the aggregate economy. As politically independent agents, elected state officials may choose to allocate transfers in ways counter to the intention of national policy-makers.³ Partisan differences is the explanation we study. For example, many Republican governors blocked the expansion of Medicaid as part of the Democratic healthcare reform bill; see [Kaiser Family Foundation \(2019\)](#). Partisan decisions are not limited to Republicans. Democratic governors refused funding for a Republican-approved federal education program promoting sexual abstinence

³While we focus on the U.S. fiscal union, our lessons generalize; see [Ivanova et al. \(2017\)](#) for re-allocations of EU aid by member states.

(Raymond et al., 2008). We argue that these partisan differences are important, and particularly so since the presidency of Ronald Reagan.

Our paper makes two contributions. First, we show that the flypaper effect measuring the budgetary impacts of federal aid on state and local government spending varies by partisanship; see Hines and Thaler (1995) and Inman (2009). Estimates of the impact of the aid and potential partisan differences are essential for the implementation of federal fiscal policies using state and local governments. When averaged across parties, our results are consistent with past estimates in the literature of the flypaper effect. However, we find the familiar approaches to estimating this effect conceal large partisan differences. We focus on the political party of the governor as our measure of partisanship. We measure partisan differences by the marginal propensity to spend from federal aid (MPS) under Democratic or Republican governors.

We identify partisan differences in MPS using panel data on close gubernatorial elections, similar to the regression discontinuity design (RDD) used by Lee, Moretti and Butler (2004) and Ferreira and Gyourko (2009) in their studies of U.S. representatives and mayors, respectively. We find statistically significant and economically important differences. Democratic governors favor spending, while Republicans favor tax relief, similar to the findings of Besley and Case (2003) for state legislatures. Unlike Besley and Case (2003), we use an RDD for causal inference and focus on differences conditional on IG aid. Our estimated partisan differences in the MPS have increased with national partisan polarization (McCarty, Poole and Rosenthal, 2016; Azzimonti, 2018) and became significant during and following the tenure of President Reagan. We find no evidence of partisan spending differences in the pre-Reagan era with its lower levels of polarization, a result consistent with evidence surveyed in Potrafke (2018). Though identified by regression discontinuity, we provide evidence suggesting that these partisan differences are informative for governors further from the discontinuity.

Along with partisan differences in the propensity to spend, the states' cash flow constraint forces differences in other fiscal policies as well. Republican governors tend to have lower debt following increased federal aid, and lower taxes, albeit with a delay. Compared to Besley and Case (2003), a novel finding is that, following increases in IG aid, state-level GDP is initially relatively higher under Democratic governors. The Democratic policy mix is more stimulative in the short-run.

Second, we quantify the macroeconomic effects of these partisan differences in MPS. We focus on the federal multiplier of IG transfers on national GDP. The national IG multiplier is the product of two effects: First, the MPS from IG aid and, second, the effect of changes in state government spending or tax relief on aggregate GDP. Our state-level estimates of partisan spending provide the first effect. To quantify the second effect, we use a macroeco-

conomic model calibrated to match recent estimates of multipliers for non-IG federal spending and tax multipliers. Our state-level estimates specify the fiscal rules for state government purchases and tax policy for a representative Democratic and Republican governor. The model features states in a monetary union and shares the many New Keynesian features of [Nakamura and Steinsson \(2014\)](#), [Auclert, Dobbie and Goldsmith-Pinkham \(2019\)](#), and [Brueckner, Pappa and Valentinyi \(2019\)](#). It gives a role to demand-side and supply-side policies through nominal frictions, constrained households, and distortionary taxes as well as endogenous labor supply. Both Democratic policies, which are estimated to favor increased spending, as well as Republican policies, which favor tax and debt reduction, have roles to play.

Without partisan differences in governor allocations (“pre-1980”), the aggregate impact multiplier of federal IG transfers is 1.22. Allowing for partisan differences with half the states assigned the policy preferences of a Republican governor and half those of a Democratic governor reduces the impact multiplier to about 0.64. The reason for the decline is initially lower state spending and thus lower aggregate demand in Republican states, which is not, yet, offset by delayed tax cuts. The long-run multiplier, however, is higher with more Republican governors. Finally, we vary the partisan division among states to match that of U.S. states from 1983 to 2019. The model predicts that the aggregate impact multiplier falls as the share of Republican governors rises.

Our paper is one of several to estimate the aggregate effect of federal aid. [Chodorow-Reich \(2019\)](#) reviews studies evaluating the impact of ARRA intergovernmental aid on local jobs and income. He concludes the best estimate of aid’s impact, if fully deficit-financed, is a national multiplier of 1.7. Our multiplier estimates are smaller, in part because our baseline calibration does not account for the Zero Lower Bound constraint on interest rates at the time of the ARRA. Our focus, however, is less on the level of the IG multiplier, but rather on estimating and quantifying how partisan differences matter for its relative impact. Overall, our analysis points to the potential importance of partisan differences in policy-makers’ preferences as a new source of heterogeneity in the macro economy.

2 Partisan Differences in Aid Allocation: Estimation

Our first task is to estimate the marginal propensity for states to spend federal IG transfers by the partisan preferences of state governors. In an ideal world, we would run a simple regression of expenditure changes on changes in IG aid, separately for governors from each party. Concerns over non-random allocation of elected officials and the possible

endogeneity of aid require us to use a more pointed empirical analysis. The next section specifies the institutional features that guide our analysis.

2.1 Specification

State and local governments receive federal IG aid in one of four ways: (1) lump-sum aid with no constraints on purpose (e.g., General Revenue Sharing), (2) lump-sum aid for spending on a specific policy objective (e.g., ESEA Title I aid for the education of lower-income children), (3) closed-ended matching aid paying a share of program expenses up to a cap on total aid (e.g., Federal Highway Aid), and finally (4) open-ended matching aid with no limit on assistance (e.g., Medicaid). Each of the first three forms of assistance provides a fixed sum of aid, with or without programmatic restrictions on how the money may be spent. While efforts are often made by the federal funding agency to enforce spending restrictions (so-called maintenance of effort provisions) such constraints are difficult to enforce. Effort provisions are binding only for new programs with no prior state or local spending. Without enforcement, the recipient government can use categorical grants other than open-ended matching as it wishes at the margin, allocations known as the “fungibility” of aid. Fungibility has been well documented.⁴

We assume full fungibility and aggregate all assistance in the first three categories into a single lump-sum transfer (at the margin) as our measure of IG aid, thus removing open-ended matching aid. We use three measures for IG aid from two sources. Our primary source is the Census of Government, *State Government Finances*. Using Census data, we first net welfare aid from total IG aid.⁵ Second, again for Census data, we net both welfare and highway from total IG aid. Though highway aid is “capped” matching aid – aid category (3) above – we remove this assistance in our second measure of IG aid as a precaution.⁶ Third, we use the Federal Assistance Award Data System (FAADS, [U.S. Census Bureau \(n.d.a\)](#))

⁴See [Government Accountability Office \(2004\)](#), [Nicholson-Crotty \(2004\)](#), and [Carlino and Inman \(2016\)](#).

⁵The Census category called Welfare Aid includes the important open-ended matching aid programs, such as Medicaid. It also includes the open-ended matching grant Aid to Families with Dependent Children and, from 1996 on, its successor, a lump-sum grant known as Temporary Assistance for Needy Families (TANF). Because of data limitations, TANF aid will be excluded from our Census definition of lump-sum aid. We do include TANF aid in an alternative measure of IG Aid, called FAADS, that we define below.

⁶Highway aid as reported by the Census Bureau does include matching provisions but they are closed-ended, or capped, provisions. We thus include highway aid in the main measure of IG aid. See the [Government Accountability Office \(2004, p. 68\)](#) report, which concludes: “[S]tates pay 100 percent of the cost of funding *additional* highway projects. . . [T]he federal highway program creates the opportunity for substitution because states typically spend substantially more than the amount required to meet federal matching requirements. . . . Thus states can reduce their own highway spending and still obtain increased federal funds.”

describing the administrative details of all federal aid programs to remove all programs with any possible matching requirements.⁷

The institutional features of state budgeting motivate our econometric specification. First, governors set state budgets. Granted agenda powers, governors propose the initial budget and in most states can successfully veto individual legislative changes with the item veto; see [Holtz-Eakin \(1988\)](#). These budgetary powers coupled with the governor’s appointment and administrative powers underlie our choice of the governor as the decisive agent for setting state budgets; see [Barrilleaux and Berkman \(2003\)](#) and [Kousser and Phillips \(2012\)](#). Second, registered voters from Democratic and Republican parties choose party candidates to run for governor from a set of “citizen-candidates” wishing to represent the party in the general election; see [Besley and Coate \(1997\)](#). The citizen-candidate specification ties party policy directly to preferences of the candidate chosen to represent the party. If governors are “policy-motivated” rather than “office-motivated” and if party voters care about policies, then the familiar Downsian result ([Downs, 1957](#)) of convergence to the preferences of the state-wide median voter is overturned; see [Wittman \(1983\)](#), [Alesina and Spear \(1988\)](#), and [Harrington \(1992\)](#). Policy preferences of the winning governor will therefore be those of her party’s median voter. Third, the state’s reversion budget implemented by continuing resolutions sets the status quo in the governor’s negotiations with the legislature. The governor remains decisive, but the legislature can block changes that move “too far” from the status quo. Thus observed budgets will be changes from the last year’s status quo or reversion budget; see [Persson and Tabellini \(2000, Chapter 2\)](#). For this reason we specify our dependent variables as yearly changes in budgetary outcomes. Fourth, we explicitly allow for the possibility of asymmetry in the impact on spending of increases and decreases in IG aid to allow for the possibility of entrenched program interests (i.e., “habit formation”) favoring current levels of state services or tax relief.⁸

⁷Online Appendix [A](#) has a section that describes how we identify open-ended matching aid policies for exclusion from the FAADS definition of IG Aid based on the descriptions of the 100 largest aid programs (for the years with FAADS data, 1983–2010). This procedure uses data from [SAM \(n.d.\)](#). The FAADS data are obligations and will differ slightly from the Census data based on actual disbursements. To control for possible differences between obligations and disbursements, we use the current and past year average of the FAADS obligation when estimating IG aid.

⁸A fixed effects regression with spending and aid in log-levels reveals the usual flypaper effect in our sample. But a level specification conceals the asymmetric partisan effects of aid on spending that are central to our analysis and clearly seen in the first difference specification. Therefore, our preferred specification is in first differences. State budgets are set incrementally from a status quo, the state’s current budget. Level regressions require a fully specified model for each state’s fiscal history defining that status quo – i.e., the “intercepts” of spending and tax equations. State and year fixed effects are possible controls but may not be enough, particularly with shifting partisanship and increasing polarization, a focus of our work here. First differences as specified in eq. [\(2.1\)](#) are a convenient way to estimate asymmetric partisan responses to increases and decreases in IG aid.

Eq. (2.1) provides the core specification for changes in log expenditures, denoted $\Delta \ln E_{s,t}$, in state s in fiscal year t in response to changes in \ln IG aid in state s and year t . We focus initially on log-differences (elasticity estimates) for our main analysis. We allow for the effect of aid to differ when aid increases ($\Delta \ln IG_{s,t}^{inc} = \max\{0, \Delta \ln IG_{s,t}\}$) or decreases ($\Delta \ln IG_{s,t}^{cut} = \min\{0, \Delta \ln IG_{s,t}\}$), and, importantly, to differ by governors' party as Republican ($Rep_{s,t-1} = 1$) or Democratic ($Rep_{s,t-1} = 0$):

$$\begin{aligned} \Delta \ln E_{s,t} = & (\gamma_{0,inc} + \gamma_{r,inc} \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^{inc} \\ & + (\gamma_{0,cut} + \gamma_{r,cut} \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^{cut} \\ & + \mu_0 + \mu_r \times Rep_{s,t-1} + \text{fixed effects} + e_{s,t}. \end{aligned} \quad (2.1)$$

We lag the governor's party affiliation by one year as state budgets are decided one year prior to their implementation. Finally, $e_{s,t}$ is the error term and is allowed to be correlated across states and time. The coefficient μ_0 and $\mu_0 + \mu_r$ are the average spending growth under Democratic and Republican governors, respectively. In our preferred fixed effects specification, μ_0 and μ_r are not identified.

Our aim is to identify the marginal propensity to spend IG aid separately for Democratic and Republican governors and for when aid is increased or decreased. A Democratic governor's estimated MPS elasticity equals $\gamma_{0,inc}$ when IG aid is increased ($\Delta IG_{s,t}^{inc} > 0$) and equals $(\gamma_{0,cut})$ when IG aid is decreased $\Delta IG_{s,t}^{cut} < 0$. A Republican governor's MPS elasticity is measured relative to that of the Democratic governor – that is, $\gamma_{0,inc} + \gamma_{r,inc}$ for an increase in aid and $\gamma_{0,cut} + \gamma_{r,cut}$ for a decrease in aid. Since the Republican party is often associated with spending cuts and the Democratic party with spending increases, we expect $\gamma_{r,inc} < 0$ and $\gamma_{r,cut} > 0$, as previously documented by [Besley and Case \(2003\)](#).

Our core specification in eq. (2.1) also includes state and year fixed effects, interacted with the governor's party or the state's census region. We include fixed effects to control for the effects on state spending of annual changes in the national economy and national fiscal policies and for state and regional differences in state economies and state public goods prices. State-party fixed effects allow, for example, a Texas Republican governor to differ from a Massachusetts Republican governor. Year-party fixed effects allow, for example, a Republican governor to allocate differently during a Bush or Obama administration. Year-party fixed effects also control for strategic congressional or presidential allocations of IG aid to states conditional on the governor's party affiliation; see [Albouy \(2013\)](#).

2.2 Estimation

OLS estimation of eq. (2.1) may lead to inconsistent estimates of the causal effects of partisanship. The governor’s party is not assigned randomly and may be correlated with unobserved events that simultaneously affect state spending. For example, IG aid allocated to a state may reflect state political preferences that determine both state spending and national political representation that determines aid; see [Knight \(2002\)](#). If so, the OLS estimated impact of aid on spending will be biased, likely upward.

While our fixed effects are meant to control for unobservables, they do so imperfectly. To address this concern, we use a regression discontinuity design (RDD) based upon election outcomes for state governors. In the RDD design, unobserved factors, such as local political conditions are captured as a continuous function of the elected official’s margin of victory (MOV). We will measure MOV in a positive (negative) direction for the election of Democratic (Republican) governor. MOV equal to zero is a 50-50 vote outcome. As MOV changes from negative to positive the elected governor suddenly changes from being a Republican to being a Democrat; however, the MOV proxy for unobservables changes continuously. The exogenous change in partisan preferences near $MOV = 0$ provides a consistent estimate of partisan preferences on policy; see [Lee, Moretti and Butler \(2004\)](#) and [Ferreira and Gyourko \(2009\)](#).

Standard RDDs identify differences in average effects. Our specification in eq. (2.1) identifies partisan differences in the marginal effect of IG aid, however. Therefore, we also need to assume that IG growth does not change discontinuously when the governor’s party affiliation changes. Our specification of IG aid as excluding matching aid seeks to control for this possibility. Furthermore, our preferred specification uses fixed effects to guard against common changes in aid brought about by changing alignments of the governor’s party with the ruling party in Congress or in the White House. Our preferred RDD design estimates the following equation for observations with a sufficiently small MOV:

$$\begin{aligned}
 \Delta \ln E_{s,t} = & (\gamma_{0,inc} + \gamma_{r,inc} \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^{inc} \\
 & + (\gamma_{0,cut} + \gamma_{r,cut} \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^{cut} \\
 & + \sum_{s \in \{cut, inc\}} \sum_{p=1}^q (\gamma_{0,s,m,p} + \gamma_{r,s,m,p} \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^s \times MOV_{s,t-1}^p \\
 & + \sum_{p=1}^q (\beta_{0,m,p} + \beta_{r,m,p} \times Rep_{s,t-1}) MOV_{s,t-1}^p \\
 & + \mu_0 + \mu_r \times Rep_{s,t-1} + \text{fixed effects} + e_{s,t}.
 \end{aligned} \tag{2.2}$$

Compared to eq. (2.1), eq. (2.2) includes a MOV polynomial of order q and its interactions with IG changes to control for selection and unobservables.

We adapt the insights for estimating standard RDDs from [Calonico, Cattaneo and Titiunik \(2014\)](#) to our setting. Our estimator first chooses a bandwidth \bar{m} for the absolute MOV, and then estimates eq. (2.2). We cross-validate our estimate of \bar{m} by omitting one state or year at a time; \bar{m} minimizes the mean-squared error (MSE) using linear MOV controls ($q = 1$). We call this estimator “Linear MSE.” This approach may yield biased estimates in finite samples. We thus follow [Calonico, Cattaneo and Titiunik \(2014, Remark 7\)](#) and use as an alternative a robust estimator with $q = 2$, with the same estimate \bar{m} as the linear MSE estimator. We call this the “robust” estimator. Notably, the analysis in [Calonico, Cattaneo and Titiunik \(2014\)](#) applies to a simpler RDD framework in eq. (2.1), namely one without *IG* aid interactions. We use a Monte Carlo study to verify that the [Calonico, Cattaneo and Titiunik \(2014\)](#) approach works in our setting.⁹

Our identifying assumption is that IG aid and the remaining unobservables are independent of the party affiliation of governors who win in close elections. Under this assumption, each estimator will allow us to identify the partisan difference in MPS. But we will not be able to identify all preference parameters. Despite fixed effects, there could still be omitted variables potentially correlated with IG aid. Such a correlation would obscure our estimate of a baseline MPS $(\gamma_{0,inc}, \gamma_{0,cut})$. Importantly, our RDD estimators do allow us to identify partisan differences $(\gamma_{r,inc}, \gamma_{r,cut})$ under our identifying assumption of independence.¹⁰ We therefore focus on partisan differences for our estimation. We assess the independence assumption using internal validity tests and robustness checks that control for potentially omitted variables. But we will impose a plausible, *though exogenously specified benchmark*, to calibrate Democratic and Republican values for MPS conditional on estimated partisan differences for our policy simulations. We use the baseline estimate to that end; see Section 6.

2.3 Data and Sample

The model is estimated using panel data encompassing fiscal and political outcomes for the fiscal years 1983 to 2014. The year 1983 is the first fiscal year for state governments to respond to the new fiscal federal policies following the election of Ronald Reagan as president, typically viewed as the start of polarization in U.S. politics; see [McCarty, Poole and Rosenthal \(2016\)](#). Our sample includes all states except those states with large sovereign wealth funds financed through severance taxes. These states have the luxury of treating

⁹See online Appendix Section B.1. In the empirical work, we also consider alternative undersmoothing and local randomization estimators; see Appendix Section C.2 for results.

¹⁰We prove that the RDD estimate identifies partisan differences in online Appendix B.2.

ΔIG as a change in wealth, rather than income. We therefore exclude Alaska, Wyoming, and North Dakota (after the 2009 fracking boom) from the analysis.¹¹ Our data combine U.S. Census of Government data on state finances with election data from the [The Council of State Governments \(2020\)](#) and additional state fiscal, economic, and political data ([U.S. Bureau of Economic Analysis \(n.d.a\)](#), [U.S. Bureau of Economic Analysis \(n.d.b\)](#), [U.S. Bureau of Labor Statistics \(n.d.\)](#), [Federal Reserve Bank of St. Louis \(n.d.\)](#), [S&P Global Ratings \(n.d.\)](#), [Klarner \(2013\)](#)); see online Appendix Section A for details.

Table 1: Summary Statistics by Political Party

Type	Variable	All elections		MOV \leq 10pp.			Test H_0 : Dem=Rep
		(1) Mean	(2) S.D.	(3) All	(4) Democrats	(5) Republicans	(6) RD robust
possibly endogenous	Expenditure growth	2.6	4.1	2.6	2.8	2.4	-1.3
	Net general rev growth	2.2	4.7	2.5	2.6	2.5	-0.2
	Income and sales tax rev growth	2.1	5.7	2.4	2.3	2.4	0.2
	Tax rev growth	2.0	5.5	2.3	2.2	2.4	-0.1
	IG growth	3.3	8.1	3.3	3.2	3.4	-0.8
	IG increases	5.0	5.9	4.9	4.9	4.9	-0.6
	IG decreases	-1.6	3.8	-1.6	-1.7	-1.5	-0.1
exogenous IG	IG growth excl welfare	2.2	10.5	2.1	2.1	2.0	-1.0
	IG incr excl welfare	4.9	7.3	4.9	4.9	4.9	-1.1
	IG decr excl welfare	-2.8	5.5	-2.8	-2.8	-2.8	-0.5
	Highway IG share in non welf IG	24.1	6.9	23.8	23.2	24.4	1.1
	Education IG share in non welf IG	36.9	8.0	37.6	38.3	37.1	0.5
	Other IG share in non welf IG	15.3	6.6	14.7	14.4	15.0	-1.0
	IG growth excl welf and hway	2.3	11.1	2.2	2.4	1.9	-0.4
IG growth FAADS	3.7	14.2	4.0	4.9	3.1	-0.8	
exogenous other	Prior term exp growth	2.9	2.2	2.9	2.9	2.9	1.4
	Prior term IG growth	3.2	4.6	3.4	3.1	3.6	0.8
	Prior term IG growth excl welfare	2.0	5.6	2.2	2.1	2.4	0.9
	5 y lag of Republican gov	47.1	49.9	41.9	44.5	39.6	-0.7
	Dem share in legislature	55.9	16.4	56.5	55.3	57.6	-1.0
	Observations	1508.0	.	632.0	299.0	333.0	.

Notes: Variable means and significance of partisan differences from 1983-2014. All growth rates are in real per capita terms. Shares and ratios in percent. Significance of partisan differences for gubernatorial elections by $MOV \leq 10pp$ and based on the robust estimator proposed in [Calonico, Cattaneo and Titiunik \(2014\)](#) with standard errors clustered by state and year.

Table 1 summarizes the data used in our analysis and provides tests for the identifying assumption that the measured economic, political, and fiscal attributes used to explain changes in state spending are similar across states electing Democratic and Republican governors in close elections. We have grouped the variables into those that are possibly endogenous, those that are exogenous and used to measure IG aid, and other exogenous variables. Columns (1) and (2) report the mean and standard deviation of each variable for the sample of all

¹¹We drop these states starting in the year that they instituted their wealth fund: Wyoming (1975), Alaska (1976), and North Dakota (2009). Only these states have severance tax revenue shares $\geq 20\%$. Our main results are robust to including these states, however.

elections. Columns (3) to (5) report means across Democratic and Republican governors for close elections ($\text{MOV} \leq 0.1$). Our concern is that our RDD estimates will be “local” in nature, telling us little about fiscal behavior away from the discontinuity. Testing for equal means in the group of exogenous variables allows us to assess the internal validity of our RDD design. Column (6) reports robust t -statistics for equality of Democratic and Republican means using the STATA package accompanying [Calonico, Cattaneo and Titiunik \(2014\)](#). We compute clustered standard errors by state and year following [Cameron, Gelbach and Miller \(2011\)](#). None of the partisan differences are statistically different from zero at the 90 percent confidence level. These results are consistent with the internal validity of the RDD.¹² Also, the close election sample is similar to the overall sample when comparing variable means. Finally, Table 1 shows we have 299 state-years with closely elected Democratic governors and 333 state-years with closely elected Republicans,¹³ representing 183 unique elections. The number of (close) elections in our sample is thus relatively modest, leading to the selection of a wider bandwidth and thus a potential imprecision of our RDD estimates.

3 Partisan Difference in Aid Allocation: Results

3.1 Graphical Analysis

Figure 2 illustrates the RDD. Governors are grouped into subsamples by their MOV, with all winning Democratic governors collected in positive MOV bins and all Republicans in negative bins. Bins have a width of 1 percentage point. A positive MOV equal to 2 corresponds to the subsample of Democratic governors winning their election by a margin of more than 1pp, but no more than 2pp (i.e., within a 51% to 49% margin). Each panel shows the estimated MPS for an increase in IG aid (γ^{inc}) for each bin as an elasticity, with the ± 1.65 standard error band for each estimate shown as the shaded area.¹⁴ The point estimates shown in Panel A omit fixed effects and have a calibrated MOV bandwidth of 11pp. Panel B shows the MPS estimates after removing party-specific fixed effects from the LHS variable prior to estimation and has a calibrated MOV bandwidth of 10pp.

Both panels in Figure 2 show a clear break in the estimated MPS elasticities as the MOV approaches zero (tied elections): The Republican governors’ estimated MPS elasticities are

¹²In the online Appendix Section C.3, we verify that our results are not driven by pre-trends.

¹³A [McCrary \(2008\)](#) test for manipulation of the running variable fails to reject the null hypothesis of continuity of our running variable (MOV); see the online Appendix Figure D.1.1.

¹⁴The +2pp to +3pp bin contains two influential observations: Ann Richards, a Democratic governor of Texas in the early 1990s and Bob Wise a Democratic governor of West Virginia in the early 2000s. Under the tenure of these two governors, their states experienced particularly high growth in both IG aid and expenditures. Without either, the elasticity would also be around 0.3 also in the +3pp bin.

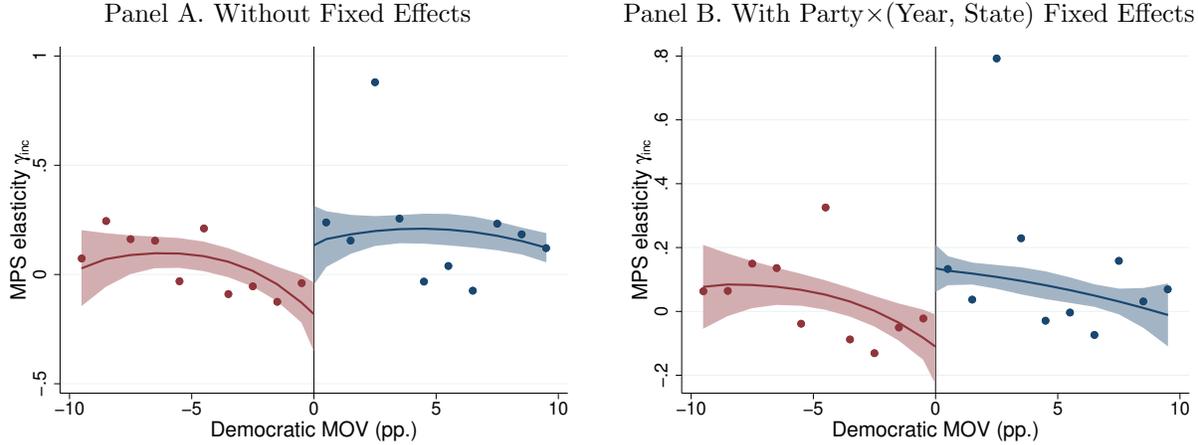


Figure 2: Regression Discontinuity in Slopes

Notes: The figures show the point estimates of the MPS elasticities for each 1 percentage point MOV bin, estimated using eq. (2.2) with $q = 0$ separately for each bin from 1983-2014. The regression function is estimated on the raw (unbinned) data using eq. (2.2) with $q = 2$ without (Panel A) or with (Panel B) fixed effects. The bandwidth minimizes the root mean squared error with linear MOV controls. The lines (area plots) show the quadratic fit (90% confidence bands). Standard errors clustered by state and year.

close to zero, while closely elected Democrats have an estimated MPS elasticity of about 0.25. The difference between these elasticities at zero identifies the partisan difference in the MPS. Fitting eq. (2.2) with quadratic MOV controls to the underlying raw data in Panel A yields, after rounding, a difference in the MPSs of 0.33 (s.e. = 0.13; the Democratic intercept is 0.13 and the Republican intercept is -0.19): For a 1% increase in IG aid Democrats increased expenditure growth by 0.33pp more than Republicans. The estimated partisan differences with fixed effects, shown in Panel B, are only slightly smaller at 0.25 (s.e.=0.07).

Though not shown, governors' responses to cuts in IG aid also show significant partisan differences as the MOV approaches zero, but of opposite sign.¹⁵ Without fixed effects, the Republican MPS elasticity in response to a decrease in IG aid is to cut spending and is 0.44 (s.e.=0.20) larger than the MPS of Democratic governors. With party by year and party by state fixed effects, the difference in MPS elasticities is 0.36 (s.e. = 0.22) and again shows larger cuts in spending by Republican governors. These estimates are based on a smaller effective sample size and are less precisely estimated.

The fact that only Democrats increase state expenditure growth in times of high transfer growth is central to our policy analysis – and drives our results. Figure 3 shows the estimates for a (standard) RDD for samples with high and low IG aid growth. The thick black lines

¹⁵Online Appendix Figure D.2.1 shows that Republicans pass-through more IG cuts as spending cuts.

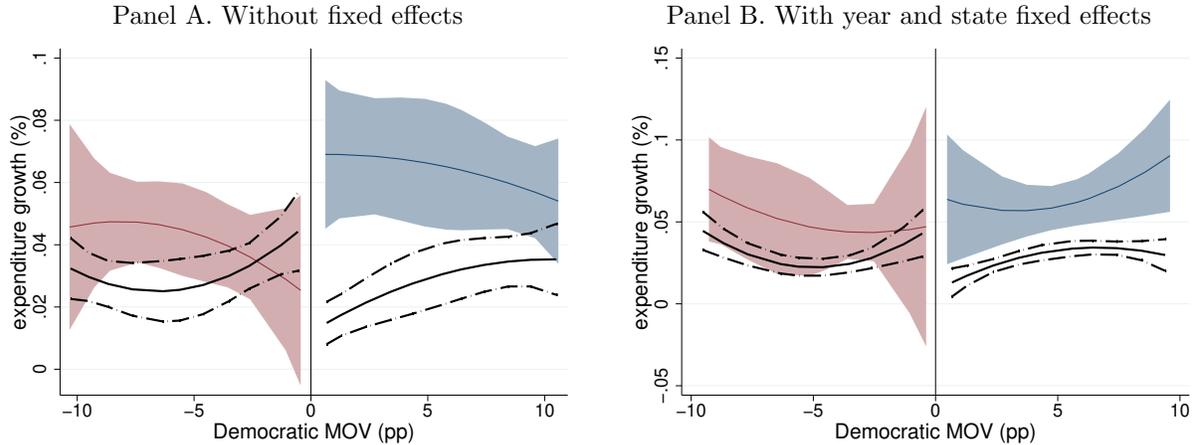


Figure 3: Average Expenditure Growth by Size of IG Increase

Notes: The figure shows the predicted mean expenditure growth for a given MOV with its 90th percentile confidence band, first for the observations with IG increases above the 75th percentile (thin line, shaded band) and then for observations with IG increases below the 75th percentile (dark line, dashed band). Coefficient standard errors clustered by year and state.

show the estimated quadratic MOV polynomial and 90% confidence intervals (dashed bands) for the mean expenditure growth conditional on IG increases below the 75th percentile. The thin lines and the 90% confidence interval (shaded band) show the same for the mean expenditure growth conditional on IG aid increases above the 75th percentile. The MOV bandwidths are the same as used for Figure 2. Panel A shows estimates without fixed effects and Panel B shows the estimates with state and year fixed effects. For Republican governors ($MOV < 0$) the two confidence intervals (shaded and dashed) strongly overlap near the MOV cutoff of zero, indicating that their spending does not respond to changes in IG aid. In contrast, expenditure growth is significantly higher when IG growth is high for Democratic governors ($MOV > 0$), as suggested by the well-separated confidence intervals at the cutoff.

We conclude that significant partisan differences are most likely to be observed following a major federal IG aid policy initiative. The mean percentage increase in IG aid for observations above the 75th percentile is 18.9%, or \$114 per resident (in 2010 dollars). For the remainder of the sample, the percentage increase in aid was 4.6 percent, or \$25 per resident. Significant partisan effects are most apparent when there are large and economically significant increases in IG aid, as may be the case during recessions.

3.2 Estimates of Partisan MPS

Table 2 shows estimates of the impact of $\Delta \ln IG_{s,t}$ on $\Delta \ln E_{s,t}$ for increases and decreases in IG aid conditional on the state governor’s political party, as specified in eq. (2.2). The estimated coefficients for “Pos IG growth” ($\gamma_{0,inc}$) and “Neg IG growth” ($\gamma_{0,cut}$) measure the MPS of Democratic governors in response to increases and decreases in aid. The coefficients for “Rep gov x Pos IG growth” ($\gamma_{r,inc}$) and “Rep gov x Neg IG growth” ($\gamma_{r,cut}$) measure the partisan difference in MPS when a state’s elected leadership switches from Democrat to Republican. Earlier studies suggest that Democrats spend more than Republicans of any increase in aid and cut less following any decrease in aid; see, for example, [Besley and Case \(2003\)](#). The results presented in Table 2 confirm these earlier results.

Table 2: Estimated MPS Elasticities

	Without fixed effects			With fixed effects		
	(1) Linear MSE	(2) Robust	(3) OLS	(4) Linear MSE	(5) Robust	(6) OLS
Pos IG growth	0.203 (0.077)	0.134 (0.109)	0.154 (0.025)	0.180 (0.043)	0.119 (0.077)	0.134 (0.025)
Rep gov x Pos IG growth	-0.260 (0.104)	-0.327 (0.130)	-0.093 (0.018)	-0.272 (0.075)	-0.290 (0.098)	-0.068 (0.021)
Neg IG growth	0.199 (0.067)	0.132 (0.102)	0.111 (0.037)	-0.016 (0.067)	-0.069 (0.123)	0.016 (0.017)
Rep gov x Neg IG growth	0.187 (0.080)	0.442 (0.200)	0.133 (0.025)	0.332 (0.099)	0.524 (0.239)	0.114 (0.019)
Rep gov	0.022 (0.005)	0.039 (0.010)	0.003 (0.002)	0.000 (0.000)	0.000 (0.000)	0.000 (.)
R-squared	0.18	0.19	0.14	0.54	0.54	0.41
R-sq, within	0.18	0.19	0.14	0.11	0.12	0.06
Observations	678	678	1508	630	630	1508
States	48	48	48	47	47	48
Years	32	32	32	32	32	32
State FE	None	None	None	By party	By party	By party
Year FE	None	None	None	By party	By party	By party
MOV bandwidth (pp)	11.0	11.0		10.0	10.0	

Notes: RDD and OLS estimates of partisan effects of IG aid without welfare aid, 1983-2014. MOV polynomials not shown. Standard errors clustered by state and year in parentheses.

Table 2 shows estimates of the partisan effects of IG aid on state spending using the RDD estimators outlined in Section 2.2 and the OLS estimator for comparison. Columns (1) and (4) provide estimates with the calibrated bandwidth of $MOV \leq 11pp$ (without fixed effects) and $MOV \leq 10pp$ (with fixed effects) using linear MOV controls. Columns (2) and (5) provide robust estimates (with quadratic MOV controls) for the same bandwidths as calibrated with linear MOV controls. OLS results are shown in columns (3) and (6).

All RDD estimates confirm our graphical analysis in Figures 2 and 3. Specifically, Republican governors spend 0.260% to 0.327% ($= \gamma_{r,inc}$) *less* of a 1% increase in IG aid than Democratic governors. All four elasticity estimates of the Republican partisan spending differences are highly significant with t -statistics of -2.3 to -2.5 without fixed effects and -3.0 to -3.6 with fixed effects. The comparable estimates but higher t -statistics with fixed effects suggest that these controls improve the precision; see Lee and Lemieux (2010). Estimates for governor responses to cuts in IG aid also reveal statistically significant partisan differences ($\gamma_{r,cut}$), ranging from a 0.187% (column (1)) to a 0.524% (column (5)) larger cut in spending for Republican governors for a 1% cut in aid. Because columns (2) and (5) use the robust RDD estimator following Calonico, Cattaneo and Titiunik (2014), they are our preferred estimates.

Columns (3) and (6) in Table 2 present OLS estimates of partisan spending differences based on the full sample of all governors. OLS estimates of partisan effects for the full sample are highly significant, though smaller than our RDD estimates. Republican governors spend 0.093% less of a 1% aid increase than Democratic governors without fixed effects and 0.068% less with fixed effects. This attenuation of the partisan differences with OLS could either reflect bias due to a failure to control for unobservables (captured by the MOV-terms) or indicate that governors have indeed smaller partisan differences when elected by a landslide (which are excluded from the RDD analysis). We consider both explanations.¹⁶

Importantly, our estimates of partisan differences are also robust to using our alternative measures of non-matching IG aid. Table 3 provides estimates of partisan differences using the linear and robust estimators with fixed effects using: (i) the Census measure of aid excluding both welfare and highway aid (columns (1) and (2)) and (ii) the FAADS-specified measure of IG aid excluding all programs with administratively identified matching provisions (columns (3) and (4)). The Census measures are available for all sample years, 1983-2014. The FAADS measure is available from 1983 to 2010. The estimated partisan differences are mostly statistically significant and comparable in magnitude. For a 1% increase in each measure of IG aid, the robust estimator of partisan differences shows Republican governors spend -0.251% less when aid is measured as Census IG aid excluding welfare and highway aid (column (2)) and -0.404% less when aid is measured by FAADS. For a 1% cut in measured IG aid, our robust estimator shows Republican governors cut spending by 0.438% using IG aid less highway and welfare aid. The FAADS estimate for cuts in IG aid is smaller, 0.186%, and not statistically significant.¹⁷

¹⁶Later on in Figure 4, we document similar OLS estimates of partisan differences using OLS up to an MOV of 30pp, representing more than 80% of our observations.

¹⁷The smaller estimated effect for the cut in spending with a cut in FAADS IG aid may be because FAADS reports only federal legislated obligations. While close, obligations are not equal to appropriations

Table 3: MPS Elasticity: Alternative IG Measures

	Excl. welfare & highway aid		Top FAADS programs	
	(1) Linear MSE	(2) Robust	(3) Linear MSE	(4) Robust
Pos IG growth	0.113 (0.040)	0.108 (0.051)	0.133 (0.045)	0.217 (0.103)
Rep gov x Pos IG growth	-0.178 (0.081)	-0.251 (0.077)	-0.300 (0.053)	-0.404 (0.134)
Neg IG growth	-0.001 (0.076)	-0.129 (0.111)	-0.120 (0.051)	-0.149 (0.140)
Rep gov x Neg IG growth	0.353 (0.094)	0.438 (0.175)	0.369 (0.076)	0.186 (0.140)
Rep gov	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
R-squared	0.52	0.53	0.54	0.54
R-sq, within	0.09	0.10	0.05	0.06
Observations	676	676	562	562
States	48	48	48	48
Years	32	32	26	26
State FE	By party	By party	By party	By party
Year FE	By party	By party	By party	By party
MOV bandwidth (pp)	11.0	11.0	11.0	11.0

Notes: Columns (1) and (2) show results for Census IG aid without welfare and highway aid, 1983-2014. Columns (3) and (4) show results for FAADS measures, 1983-2010. MOV polynomials not shown. Standard errors clustered by state and year in parentheses.

3.3 Level Differences and the Flypaper Effect

Much of the previous literature estimating the impact of IG aid on spending has focused not on the elasticity of state spending, but on the dollar-for-dollar effect of aid on spending, known as the flypaper effect. While we could adjust the elasticity estimates in Table 2 to show the ratio of expenditure to IG aid, we prefer estimates that directly scale expenditure growth on the LHS with the lagged ratio of expenditures to IG aid. Columns (1) and (2) in Table 4 present the Linear and Robust RDD dollar estimates. For comparison, columns (3)-(5) present OLS estimates first for our full specification, then for asymmetry-only in aid increases or cuts, and last for the familiar simple average effect of aid on spending.¹⁸

Table 4: Dollar Estimates of MPS Elasticity

	RDD		OLS		
	(1) Linear RDD	(2) Robust RDD	(3) Full	(4) Asymmetry only	(5) Basic
Pos (or overall) IG growth	1.285 (0.356)	1.346 (0.600)	1.276 (0.186)	0.935 (0.134)	0.805 (0.109)
Rep gov x pos IG growth	-0.944 (0.517)	-1.576 (0.892)	-0.702 (0.181)		
Neg IG growth	0.135 (0.367)	0.332 (0.805)	0.073 (0.141)	0.612 (0.184)	
Rep gov x neg IG growth	1.785 (0.627)	2.831 (1.105)	1.096 (0.159)		
R-squared	0.46	0.47	0.41	0.41	0.41
R-sq, within	0.09	0.09	0.07	0.06	0.06
Observations	1070	1070	1508	1508	1508
States	48	48	48	48	48
Years	32	32	32	32	32
State FE	By party	By party	By party	By party	By party
Year FE	By party	By party	By party	By party	By party
MOV cutoff (pp)	≤22	≤22			

Notes: The table shows estimates for Census IG measure excluding welfare aid, 1983-2014. Estimated MOV polynomials not shown for columns (1) and (2). Standard errors clustered by state and year in parentheses.

The RDD estimates of partisan differences are $-\$0.944$ (Linear RDD) and $-\$1.576$ (Robust RDD) for a dollar increase in IG aid. To relate these coefficients to the flypaper literature, we need to take the Democratic baseline into account, whose causal interpretation would require stronger assumptions.¹⁹ The estimates for an increase in aid on spending for a Democratic (what the states actually receive). If FAADS measures of obligations are cut, but actual appropriations in the budget are cut less, then the denominator in the marginal effect with FAADS will overstate the actual decline in aid received by the state leading to an underestimate of the true marginal effect based on received state aid. Census measures of IG aid are from state audited accounts of actual dollars received.

¹⁸In the online Appendix Section C.4 we present various RDD estimates for the MPS level differences using both scaled growth rates and dollar-on-dollar regressions.

¹⁹The effects for the Democratic baseline have a causal interpretation only if the fixed effects are sufficient controls and there is no omitted variable bias.

governor are \$1.285 (Linear RDD) and \$1.346 (Robust RDD); neither is statistically different from \$1.00. For Republican governors, however, the RDD estimates of partisan differences suggest that for a \$1 increase in IG aid, a Republican governor will increase spending by \$.341 ($= 1.286 - 0.944$; Linear RDD) or perhaps even reduce spending by \$.230 ($= 1.346 - 1.576$; Robust RDD). Neither estimated effect of aid on spending for Republican governors is statistically different from zero, however. For a marginal \$1 decrease in aid, Democrats make small (and not statistically significant) cuts in spending – by \$.135 (Linear RDD) to \$.332 (Robust RDD). In contrast, Republicans are estimated to make large marginal cuts in spending following the loss of \$1 in aid, cutting spending by \$1.785 (Linear RDD) to \$2.831 (Robust RDD) more than Democratic governors. The estimated final cuts by Republican governors are greater than the dollar loss in aid and equal \$1.92 ($= 0.135 + 1.785$; Linear RDD) to perhaps as much as \$3.163 ($= 0.332 + 2.831$; Robust RDD). Such large marginal cuts suggest, perhaps, that closely elected Republicans are using the loss of a marginal dollar of aid to justify closing down federally favored programs and to thereby save average (inclusive of overhead) dollars as well – a policy that may play well with their partisan base.

It is instructive to compare our RDD estimates to what they might imply about the familiar estimates of the flypaper effect not making allowances for partisan differences or asymmetries in changes in IG aid. Weighting the Robust RDD estimates for Democratic and Republican governors responses to an increase in aid by the overall sample share of budgets decided by Democratic (0.48) and Republican (0.52) governors implies an estimated weighted average impact of a \$1 increase in aid on spending of \$0.53 for a dollar of new aid ($0.526 = 1.346 \times 0.48 + (-0.230) \times 0.52$). For a \$1 cut in aid, the weighted average (Robust RDD) estimate is a cut in spending of \$1.80 ($1.804 = 0.332 \times 0.48 + 3.163 \times 0.52$). Weighting these partisan average effects for aid increases and aid cuts by the share of observations that are (real dollar) increases (.59) or decreases (.41) in aid implies an overall average effect of dollar change in aid on spending of \$1.05 ($1.051 = 0.53 \times 0.59 + 1.80 \times 0.41$). This weighted average of the RDD estimates of a \$1.05 change in spending for a \$1 change in aid *is* the familiar flypaper effect (Inman, 2009) and close to the simple OLS estimate (\$.81) of aid on spending reported in column (5) of Table 4 for our full sample of all governors and years. Importantly for policy work, we can “deconstruct” the familiar flypaper estimate into differences by partisan preferences and by differences for increases and decreases in aid.

Our policy analysis of the macroeconomic consequences of IG aid in Section 6 respects these differences and uses the estimates from Table 4 to illustrate their importance for understanding how changes in IG aid impact economic activity at the federal level.

4 External Validity

A concern for our RDD estimates of MPS is that they identify the effects of partisanship on MPS only for governors elected in close elections. RDD estimates may tell us little about governor preferences elected by margins away from the MOV discontinuity. In this section, we consider whether our estimated sample average treatment effects also apply more broadly.

4.1 Is the Close Election Sample Special?

We address this question in four steps. First, do partisan spending differences observed for governors winning in close elections also hold for governors winning by wider margins? In Figure 4, Panel A (elasticity estimate) and B (level estimate) show the correlation between MOV and partisan differences, using a rolling window OLS estimator without fixed effects for MOV intervals of 5pp from 0 to 45pp. The estimated partisan spending differences are for increases in IG aid ($\gamma_{r,inc}$) and are plotted centered within each MOV interval, along with their 68% and 90% confidence intervals. For both the elasticity and level estimates, we observe lower spending increases for Republican than Democratic governors in response to aid increases ($\gamma_{r,inc} < 0$) across all MOV intervals up to MOV equal to 0.30. In the online Appendix, Figure D.3.1 shows that the patterns for IG cuts are similar, but of opposite sign. For governors elected by margins greater than 30pp, what might be called “landslides,” we observe no partisan differences. Perhaps those governors, whether Democrat or Republican, have the luxury to pursue a Downsian agenda most beneficial to the median state voter. As a precaution and for a comparison, we will simulate the effects of IG aid on the state economies using both our RDD estimates of MPS and OLS estimates for the full sample; see Table 4 column (3).

Second, we offer a direct comparison of governor partisan ideologies for those elected with MOV near zero and for governors generally. Bonica (2014) offers a direct measure of governor ideology based on the preferences of donors contributing to the candidate’s election. We use these data to specify the ideological difference between Democratic and Republican candidates for governor as a measure of ideological differences; positive differences represent an election with more polarized candidates. See Online Appendix A.3 for a data description. Figure 5 presents a scatter plot of the 205 elections with complete ideological scores for our sample of governors by comparing candidates’ ideological differences to the absolute value of MOV for each election. A quadratic specification relating MOV to differences in candidate ideology cannot reject the null hypothesis of no relationship.²⁰ We conclude that

²⁰Online Appendix Section C.5 provides the full description and results for this polarization analysis.

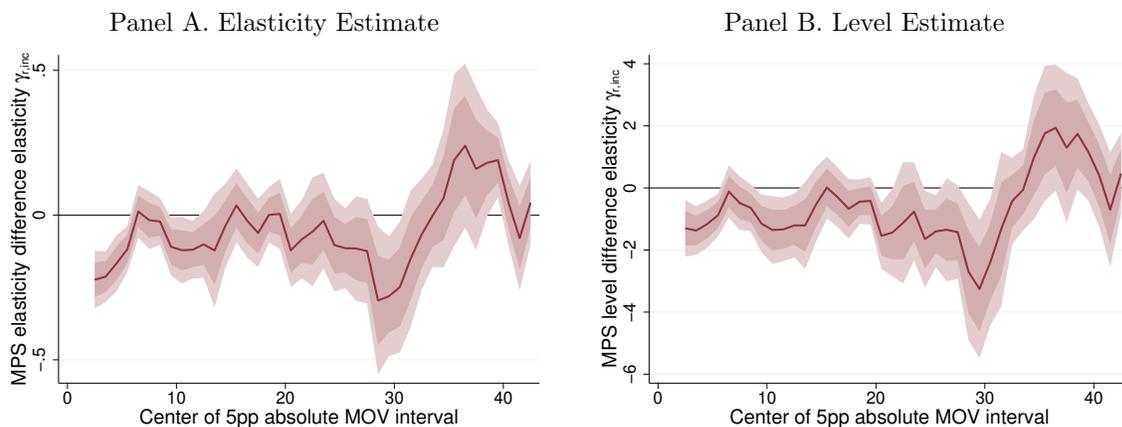


Figure 4: Rolling Window- MPS Elasticity Estimates

Notes: Panel A shows the MPS elasticity estimates for IG increases using an absolute 5pp rolling window. For example, the 20pp estimate in the panel is estimated based on all elections with 17.5pp to 22.5pp absolute margin of victory. Panel B shows the corresponding estimates for MPS in levels.

the ideological differences between candidates are unrelated to the closeness of the elections.²¹

Third, might state economic conditions affect partisan differences, perhaps by being smaller when the state’s economy, or the state’s status in the bond market, is under stress? Table 5 examines this question by interacting each measure of partisan differences with either of two alternative measures of fiscal stress: increases in the state’s rate of unemployment (U.S. Bureau of Labor Statistics, n.d.) or a downgrade in the state’s bond rating (S&P Global Ratings, n.d.). Columns (1) and (2) show results when partisan differences are interacted with a dummy variable indicating whether the state’s unemployment rate is above its state-median. Columns (3) and (4) show the results when partisan differences are interacted with a dummy indicating a downgrade in a state’s S&P bond rating in the prior three years. All regressions include fixed effects. Importantly for our policy simulations in Section 6 below, increases in the state unemployment rate do not impact partisan MPS. Democrats still spend all their aid increases while Republicans use aid to cut taxes. (With negative cuts in aid, Republicans appear somewhat less aggressive in cutting spending.) Downgrades in state bond ratings also have plausible marginal effects on the partisan allocation of aid. In both cases, the estimates of the partisan pass-through for IG increases of -0.272 for unemployment

²¹The p -value for the null hypothesis of no relationship between MOV and ideological differences equals 0.18 for the full sample and 0.99 for a sample excluding elections where MOV is greater than 40pp. The online Appendix Table C.5.1 estimates whether a candidate’s ideological score interacted with partisanship affects the estimated partisan MPS differences $\gamma_{r,cut}$ and $\gamma_{r,inc}$, and estimates no significant interaction effects.

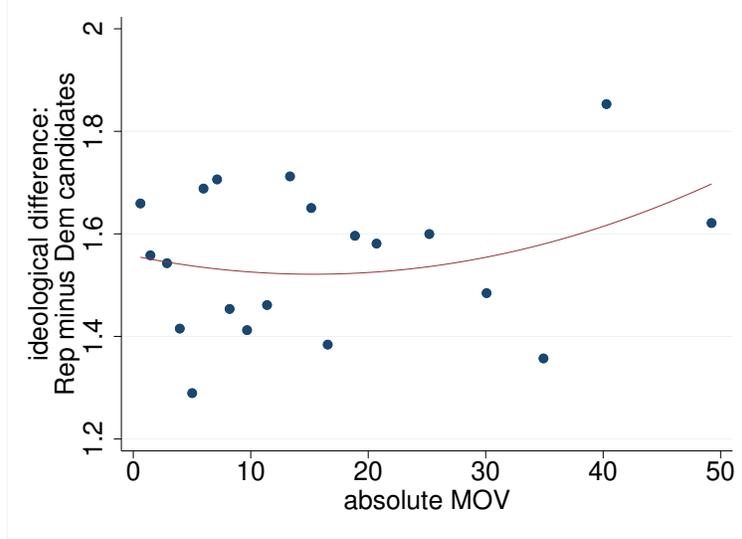


Figure 5: Ideological Differences by Party and MOV

Notes: This figure shows the relationship between closeness of elections and ideological differences between Republican and Democratic candidates for governor, 1990-2012. Each absolute 1p.p. margin of victory bin averages the ideological difference score between the Republican and Democratic candidates for governor for each election within the bin.

rate increases and -0.313 for debt downgrades are similar to the baseline estimate of -0.327 under the robust estimation procedure. The triple interactions for positive IG increases are insignificant. The baseline interaction estimate, which may not have a causal interpretation, indicates that all governors spend less of an aid increase after a downgrade. Republican governors continue to spend relatively less of aid increases than Democrats.

Fourth, are the estimated partisan differences perhaps a passing phenomenon, with state politics returning to the median politics of the 1960s and 70s? We now allow partisan differences to vary with the degree of political polarization for the period 1964 to 2014 and re-estimate our model. Our measure of political polarization is the historical time series proposed by [Azzimonti \(2018, 2014\)](#) based on news coverage of partisan policy conflict. We interact the national polarization measure with an indicator variable for Republican governor setting spending policy. Polarization is denoted by PPC_t and normalized to have a zero mean and unit variance; PPC_{t-1} represents polarization lagged one period to reflect that state budgets are set in the year before spending is realized. Standard errors are reported within parentheses and clustered by state and year. We estimate the following elasticity specification:

Table 5: State-Level Economic Conditions and MPS Elasticity Estimates

Interaction variable (IA) =	Unemployment increases $\min\{0, U_{s,t} - U_s^{median}\}$		Debt downgrade dummy (relative to 3 years ago)	
	(1) Linear MSE	(2) Robust	(3) Linear MSE	(4) Robust
Pos IG growth	0.186 (0.06)	0.185 (0.12)	0.169 (0.05)	0.153 (0.08)
pos IG x IA	0.019 (0.05)	0.065 (0.12)	-0.209 (0.09)	-0.392 (0.16)
Rep gov x Pos IG growth	-0.248 (0.10)	-0.272 (0.16)	-0.214 (0.09)	-0.313 (0.11)
pos IG x Rep gov x IA	-0.029 (0.07)	-0.009 (0.15)	-0.014 (0.15)	0.329 (0.27)
Neg IG growth	0.010 (0.08)	-0.241 (0.14)	-0.026 (0.04)	-0.135 (0.10)
neg IG x IA	-0.032 (0.03)	0.116 (0.04)	-0.107 (0.13)	-0.325 (0.22)
Rep gov x Neg IG growth	0.296 (0.12)	0.705 (0.26)	0.361 (0.08)	0.105 (0.31)
neg IG x Rep gov x IA	-0.012 (0.05)	-0.245 (0.09)	0.295 (0.68)	-4.059 (1.92)
Rep gov x IA	-0.011 (0.00)	-0.017 (0.01)	-0.011 (0.02)	0.128 (0.05)
R-squared	0.55	0.56	0.54	0.55
R-sq, within	0.13	0.15	0.12	0.13
Observations	630	630	676	676
States	47	47	48	48
Years	32	32	32	32
State FE	By party	By party	By party	By party
Year FE	By party	By party	By party	By party
MOV cutoff (pp)	10.0	10.0	11.0	11.0

Notes: In this table, we consider how state-level economic conditions affect the partisan pass-through of transfers to spending. To address the pass-through issue, we interact our main specification with two distinct measures of state-level fiscal distress: increases in state-level unemployment rate in Columns (1) and (2) as well as debt rating downgrades in Columns (3) and (4). MOV polynomials and their IA interactions are not shown. Standard errors clustered by state and year are shown in parentheses.

$$\begin{aligned}
\Delta \ln E_{s,t} = & \left(\underset{[0.05]}{0.20} + \underset{[0.05]}{0.02} PPC_{t-1} + \left(\underset{[0.07]}{-0.15} - \underset{[0.06]}{0.13} PPC_{t-1} \right) Rep_{s,t-1} \right) \Delta \ln IG_{s,t}^{inc} \\
& + \left(\underset{[0.01]}{0.04} + \underset{[0.04]}{-0.01} PPC_{t-1} + \left(\underset{[0.07]}{0.17} + \underset{[0.06]}{0.04} PPC_{t-1} \right) Rep_{s,t-1} \right) \Delta \ln IG_{s,t}^{cut} \\
& + (\text{MOV}, \text{MOV}^2) \times \text{IG} \times PPC_{t-1} \times \text{party interactions} \\
& + \text{fixed effects} + e_{s,t}.
\end{aligned} \tag{4.1}$$

The estimated average effect of polarization on partisan differences in the governors' MPS for increases in aid is statistically and economically significant and equals $(-0.13 \times PPC_{t-1})$ suggesting that as polarization increases Republican governors are more likely to allocate increased aid to the revenue side of the state budget. Similarly, as polarization has risen, Republican governors are more likely to cut spending when aid is cut $(.04 \times PPC_{t-1})$, though now the effect of polarization on partisan responses is not statistically significant. Before 1980, PPC_{t-1} averaged 1.1 standard deviations below the sample mean, implying no significant partisan differences in the allocation of aid: $0.01 \approx -0.15 + (-1.1) \times (-0.13)$ (s.e. = 0.08) for increases in aid and $0.12 \approx 0.17 + (-1.1) \times 0.04$ (s.e. = 0.11) for cuts in aid. After 1990, however, PPC_{t-1} averaged 0.8 standard deviations above the sample mean implying a positive and rising partisan difference when aid is increased: $-0.25 \approx -0.15 + 0.8 \times (-0.13)$ (s.e. = 0.09). Since polarization is 1.4 standard deviations above the sample mean for the last year (2014) of our sample, its effect on partisan differences is greater still.²² Political polarization could subside, of course, but the work of [Callander and Carbajal \(2022\)](#) suggests that polarization, and thus our estimated partisan differences, are likely to persist.

4.2 Are Partisan Differences Different by Geography?

In the policy simulations to be conducted we assign a common IG aid response to all states. It is thus important to see if there are significant regional differences in how governors' respond to IG aid. To test for regional difference we use an estimator proposed by [Hartman \(2021\)](#) to compare population average treatment effects. We reestimate the RDD specification in eq. (2.2) separately for each of the nine U.S. Census divisions without including fixed effects. Next, we compute the average of the nine division estimates, first as a simple average (Division Mean) and then weighted by the inverse of each division's sample size or by each division's population (Population Weighted). Figure 6 shows three different weighted averages and their 90% confidence intervals. Also shown in the figure is the pooled

²²In the online Appendix Section C.6, the full results for the polarization analysis are provided. The Regression (4.1) corresponds to the robust estimator shown in Table C.5.1.

estimate taken from the baseline Robust RDD found in Table (2) Column (2). An examination of the figure shows that there are no significant differences in the estimated partisan differences across the different averages for the U.S. Census regions, suggesting that our full sample RDD estimates are appropriate for simulating national IG policies.

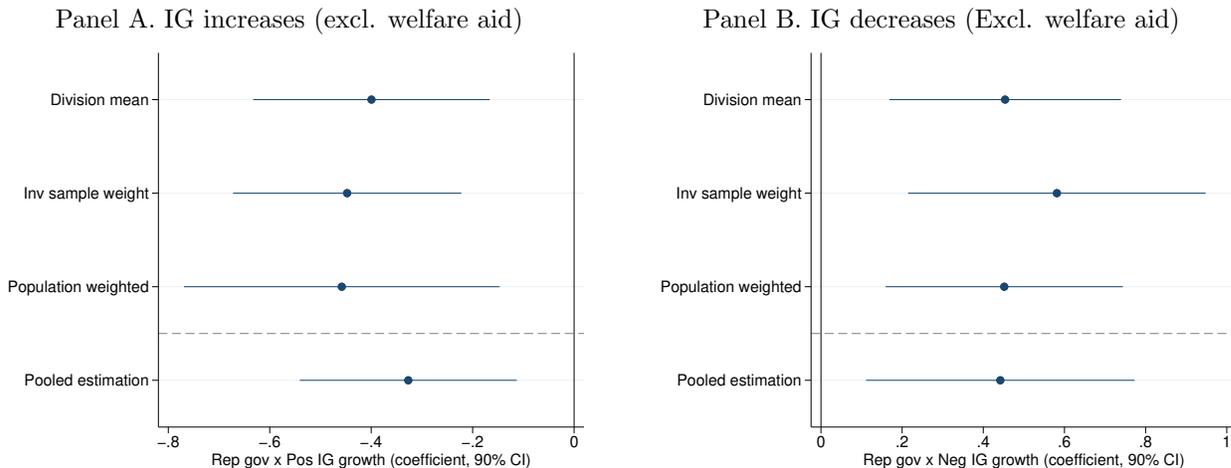


Figure 6: Partisan Differences and Geography

Notes: The top three rows in each figure show different weighted averages of the partisan differences estimated separately in each of the nine Census divisions with a 11pp MOV bandwidth and quadratic MOV polynomial, 1983-2014. “Division mean” refers to a simple average; “Inv sample weight” weighs by the inverse of the number of observations; “Population weighted” weighs each division with its relative population from the year 2000. “Pooled estimation” is our baseline robust RDD estimate.

5 From the Micro Model to the Macro Model

Having established partisan differences in spending, the question remains how the IG revenue is allocated. Whether extra revenues are used for spending, debt relief, or tax cuts determines the effect on economic activity. Further, we provide a benchmark for the macro model in Section 6, and estimate directly the effects of partisan differences in the allocation of aid on state GDP growth.

5.1 Alternative Policy Instruments: Debt and Tax Cuts

What do Republican states do with an additional dollar of aid if they do not increase spending? By the cash-flow identity, they must either cut revenues, reduce debt, or increase

savings. We use the cross-sectional identification strategy to answer this question by replacing expenditure growth on the LHS of eq. (2.2) with changes in the log-level of debt and top marginal income tax rates over different time horizons to current IG growth. Panel A in Figure 7, plots the partisan interactions ($\gamma_{r,inc}$) for the response of state-level debt to IG aid increases, while Panel B plots the response of the tax rate. Given that states might not immediately choose to change either debt or taxes, placing aid initially in a “rainy day” fund, we consider the pass-through both in the current year and up to three years out.²³

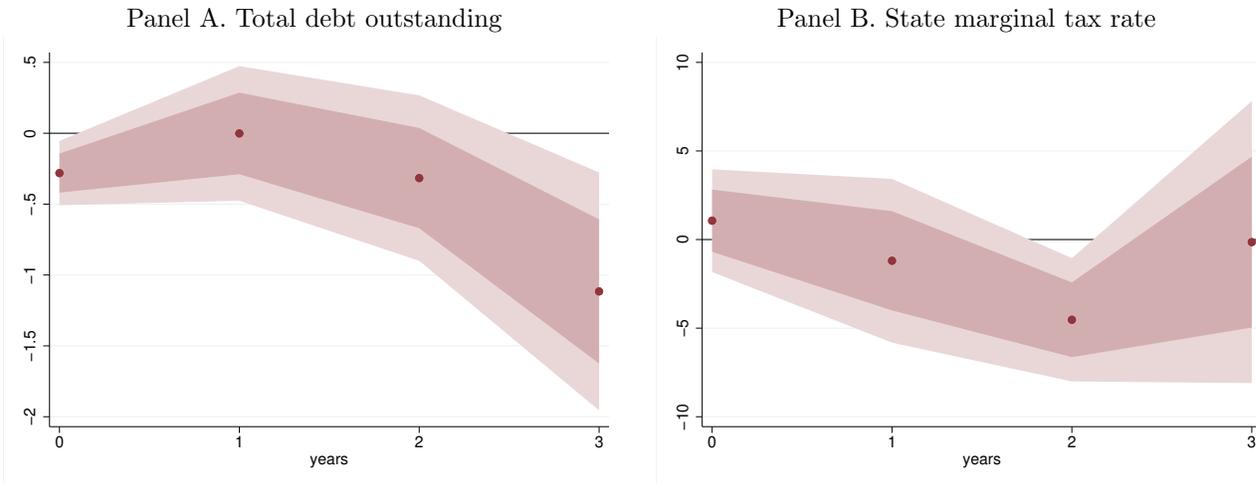


Figure 7: Debt and Tax Elasticities to IG Increases

Notes: Point estimate for difference between Republican minus Democratic outcome (1983-2014) with 90% and 68% point-wise confidence interval (quadratic MOV controls). Complete estimates, including effects of cuts in aid, are available in the online Appendix Section C.7.

Panel A of Figure 7 shows that Republican governors immediately lower debt outstanding relative to Democratic governors in response to increased aid and then keep debt levels lower thereafter. A 1% increase in IG aid leads to a fall in total debt outstanding that is 0.25% greater for states with Republican governors. The evidence suggests relatively larger declines in debt outstanding in Republican-led states that last for at least three years.

Panel B of Figure 7 shows partisan differences in states’ top marginal income tax rate. On impact, there is no partisan difference. But after two years, for each 1% increase in IG growth, the top tax rate in Republican states is about 1%, or 0.05pp, lower than in

²³A separate question is whether Democrats and Republicans differ in the composition of their spending. Breaking down spending, we only find robust partisan difference in capital expenditure growth and transfers to state and local governments and suggestive evidence for current expenditures. Together, these categories account for 83% of total expenditure. We found no differences transfers to households. We thus interpret the increase as increased government consumption. See the online Appendix Tables C.8.1-C.8.4.

Democratic states. The fact that Republicans are more likely to allocate aid to tax rate cuts and that such cuts are not observed until two years after the receipt of aid is consistent both with popular reports of Republicans’ fiscal preferences²⁴ and with the reality of lengthy budget negotiations in state politics; immediate debt relief is easier. This evidence for the timing of tax adjustments – immediate debt relief but delayed changes in tax rates – is crucial for the predicted aggregate income effect of aid in our simulations of the macro model.

5.2 Direct Estimate: Aid and State Economic Activity

Does IG aid’s differential effect on state spending, revenues, and debt relief lead to differential effects on the growth rate of state incomes? Figure 8 provides a direct estimate, showing the difference in state GDP growth across Republican- and Democratic-led states for an increase in aid ($\gamma_{r,inc}$), again specified as in eq. (2.2) but with cumulative GDP growth as the dependent variable. Following positive IG growth states with Democratic governors enjoy relatively higher GDP than states with Republican governors – that is, $\gamma_{r,inc} < 0$. The estimates imply income growth under Democratic is 0.334% higher for a 1% increase in aid. The result is robust for up to three years after the receipt of aid. For IG cuts, the results are of opposite sign and similar in magnitude, but marginally insignificant (see the online Appendix Section C.9).

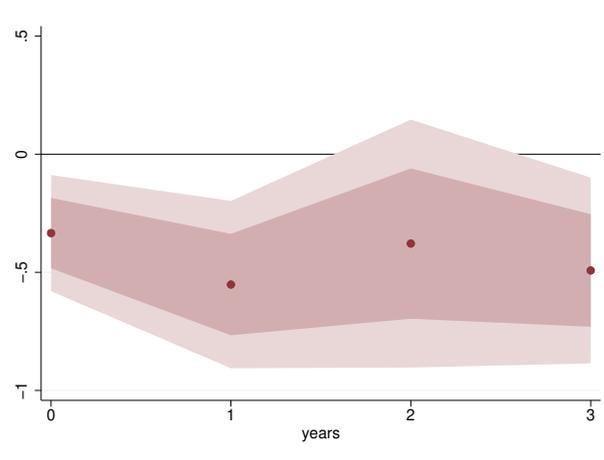


Figure 8: State GDP Growth Elasticity to IG Increases

Notes: Point estimate for difference between cumulative Republican minus Democratic GDP growth (1983-2014) with 90% and 68% point-wise confidence interval (quadratic MOV controls). Complete estimates, including effects of cuts in aid, are available in the online Appendix Section C.9.

²⁴Republican states are suing to permit the use of COVID IG aid for tax cuts (Bishop-Henchman, 2021).

Our results provide novel evidence that partisan differences in state policy, conditional on IG cuts, do affect economic activity, in contrast to [Besley and Case \(2003\)](#). In the next section, we use a structural New Keynesian model to interpret these results on different fiscal policy instruments and economic activity.

6 Partisan States in a Macroeconomic Model

To assess the aggregate effects of partisan differences in states responses to IG aid, we specify a macroeconomic business cycle model that features two representative states in a monetary union, each endowed with the estimated preferences of a Democratic or Republican governor. We use the model to evaluate the effects of a national fiscal stimulus through IG transfers conditional on our estimated difference in governors' MPS.

6.1 Environment

There are two states, inhabited by representative households and intermediate firms. The home state is of size n , while the other state is of size $1 - n$, $n \in [0, 1]$. The states trade with each other. Households are immobile across states. Production is subject to decreasing returns, reflecting fixed factors. Each state has its own government. There is a federal fiscal authority and a common monetary authority. Except for state policy-makers' preferences and possibly state size, the home (H) and other ("foreign" F) states are symmetric. We thus focus our discussion on the home state. As needed, variables pertaining to the other state are denoted by an asterisk. The full set of model equations is in online Appendix [E](#).

Households. There is a unit measure of households in each state, divided into constrained and unconstrained households, with shares of $1 - \mu$ and μ . Unconstrained households have access to complete markets and accumulate private capital and government debt. Credit-constrained households have no savings and consume their income every period. Households have identical utility over consumption, leisure, and state government services:

$$u(C_t, N_t, G_{st,t}) = \frac{1}{1 - 1/\varepsilon_C} C_t^{1-1/\varepsilon_C} - \kappa_n^i \frac{N^{1+1/\varepsilon_N}}{1 + 1/\varepsilon_N} + v(G_{st,t}), \quad (6.1)$$

where C_t is an aggregate consumption good, N_t is labor supply, and $G_{st,t}$ is state government expenditure. We also consider [Greenwood, Hercowitz and Huffman \(1988\)](#), referred

to as GHH) preferences for robustness.²⁵ The elasticity of substitution, ε_C , and the Frisch elasticity of labor supply, ε_N are common across households. The household's preferences for leisure are governed by κ_N^i ; κ_N^i differs by type of household ($i \in \{c, u\}$ for constrained and unconstrained). While state spending may affect household welfare, preferences for $G_{st,t}$ are separable and thus do not affect our positive analysis below.

Households pay proportional federal and state labor income tax rates τ_t^f and τ_t^{st} , respectively, on their labor income $W_t N_t$, receive transfers Tr_t , and have profit income Pr_t derived from firm ownership. Only unconstrained households can hold nominal bonds B_t , whose interest rate is R_t^n . The price index for consumption goods is P_t . The budget constraint for unconstrained agents is:

$$P_t C_t^u + B_t^u \leq (1 - \tau_t^f - \tau_t^{st}) W_t N_t^u + B_{t-1}^u R_{t-1}^n + Pr_t + Tr_t^u. \quad (6.2)$$

Unconstrained agents trade in complete markets via Arrow-Debreu securities, which are omitted from the equation for simplicity. The budget constraint is similar for constrained households, but with $B_t^c = 0$ and without Arrow-Debreu securities. Constrained agents receive transfers Tr_t^c . Household demand for consumption and investment is characterized by nested CES preferences over varieties produced at home and in the other region, with a weight on home goods given by ϕ_H . P_t is the cost-minimizing price index.

Firms. Each state has a measure of intermediate goods producers $z \in [0, 1]$. Each produces its variety $y_{h,t}(z)$ using labor only, subject to decreasing returns to scale:

$$y_{h,t}(z) = A_t \times N_t(z)^{1-\alpha}, \quad (6.3)$$

where A_t is TFP and $\alpha \in (0, 1)$ is the share of the fixed factor. Firms face a constant elasticity of demand (θ) and set prices in monopolistic competition subject to a Calvo-friction: With probability ξ , the firm cannot reoptimize in a given period and its prices rise at the rate of trend inflation. Absent frictions, firms would set a constant markup $\theta/(\theta - 1)$ over marginal cost.

State governments. States adjust their government consumption and labor income tax rates in response to changes in IG transfers, and may accumulate debt to finance temporary budget deficits. Both state governments are symmetric, except for the propensity to spend

²⁵Consumption and hours worked are complementary with GHH preferences feature and there is no wealth effect on labor supply. The GHH utility function is: $\left(C_t - \kappa_n^i \frac{N^{1+1/\varepsilon_N}}{1+1/\varepsilon_N} \right)^{1-1/\varepsilon_C} / (1 - 1/\varepsilon_C)$.

IG transfers. In the (Republican) home state, the MPS is ψ_{IG} , while it is ψ_{IG}^* in the other (Democratic) state.

Government consumption is the sum of an exogenous (“pre-transfer”) component $G_{st,t}^x$, and the MPS (ψ_{IG}) of an increase in real IG aid IG_t/P_t :

$$G_{st,t} = \psi_{IG} \left(\frac{IG_t}{P_t} - \overline{IG} \right) + G_{st,t}^x. \quad (6.4)$$

State demand for $G_{st,t}$ and household demand have the same CES structure, with a home share ϕ_H and price elasticity η .

States adjust distortionary taxes to stabilize debt. They smooth tax rates, and gradually adjust labor income tax rates in response to their debt burden and level of net expenditure. Denoting trend inflation by $\bar{\Pi}$, our baseline state tax rule therefore takes the following form:

$$\begin{aligned} \tau_{st,t} = \rho_\tau \tau_{st,t-1} + (1 - \rho_\tau) & \left(\bar{\tau}_{st} + \psi_{st,b} \left((R_{t-1}^n - 1) \frac{b_{st,t-1}}{\Pi_t} - (\bar{R}^n - 1) \frac{\bar{b}_{st}}{\bar{\Pi}} \right) \right. \\ & \left. + \psi_{st,E} (G_{st,t-1} - \bar{G}_{st} - (IG_{t-1} - \overline{IG})/P_t) \right), \end{aligned} \quad (6.5)$$

where b_{st} is real state debt. States change tax rates according to the rate of adjustment ρ_τ , while $\psi_{st,b}$ and $\psi_{st,E}$ govern how states adjust tax rates to increased interest cost of outstanding debt and to increased expenditures net of IG revenue. Federal transfers to states, unlike expenditures and state transfers to households, are in nominal dollars.²⁶

Federal government. The federal government levies lump-sum and distortionary taxes to finance federal government consumption $G_{f,t}$ and to provide intergovernmental transfers to states. Real government consumption $G_{f,t}$ is equalized across states in per capita terms. Nominal per capita transfers, denoted IG_t , are equal in each region. IG_t transfers follow an exogenous AR(1) process with persistence ρ_{IG} , calibrated for persistence of ARRA IG aid. For comparison, we also consider exogenous shocks to defense spending $G_{f,t}$ and federal labor tax rates τ_t^f with the same persistence.

The federal government finances its steady state expenditures through labor income taxes, lump-sum taxes, and borrowing in steady state. For simplicity, we assume that shocks to IG, $G_{f,t}$, or federal tax rates are all financed with lump-sum taxes levied on unconstrained households.²⁷

²⁶State tax rates in our model respond to the same variables that we found to be significant determinants of tax rates in the data in our reduced form regressions in online Appendix Table E.6.1.

²⁷Since Ricardian equivalence holds with respect to lump-sum taxes on unconstrained households, we eliminate the need for a specific rule for fiscal adjustments. Our results for impact multipliers change little with slow adjustment of distortionary taxes, consistent with the time-path estimated in Ramey (2011).

Monetary authority. The monetary authority reacts to aggregate inflation and output when setting interest rates. It follows a standard Taylor rule, similar to Galí (2008).

$$R_t^n = \left(\frac{\bar{\Pi}}{\beta}\right)^{\rho_r} \left(\left(\frac{\Pi_t^{agg}}{\bar{\Pi}}\right)^{\psi_{r\pi}} \left(\frac{Y_t^{agg}}{Y_t^{flex}}\right)^{\psi_{ry}} \right)^{1-\rho_r}, \quad (6.6)$$

where aggregate inflation Π_t^{agg} and output Y_t^{agg} are simply weighted measures of regional consumer price inflation and output ($\Pi_t^{agg} \equiv n\Pi_t + (1-n)\Pi_t^*$ and $Y_t^{agg} \equiv nY_t + (1-n)Y_t^*$). The parameters $\psi_{r\pi}$ and ψ_{ry} determine how much the interest rates react to deviations of aggregate inflation and output from their steady state levels $\bar{\Pi}$ or flexible-price level Y_t^{flex} .²⁸

Equilibrium and solution. We solve for a standard symmetric, competitive equilibrium with each type of firm and household within each region behaving optimally, taking as given the stochastic processes for policy and the fiscal and monetary policy rules. To approximate the solution, we linearize the economy. We then solve for the equilibrium law of motion and decision rules using Dynare (Adjemian et al., 2011).

6.2 Calibration

Our calibration embeds our estimated partisan MPS differences in the model. We also calibrate the model to the additional micro estimates on tax adjustment and the relative output responses, while generating a purchase multiplier of 0.8 and a tax multiplier of similar magnitude in our baseline calibration, and twice as high with GHH preferences.

Households. We set $\beta = 1.02^{-1}$, corresponding to a 2% annual interest rate, and the intertemporal elasticity of substitution ε_c to one (log utility), as is common in the literature. We choose a Frisch elasticity of labor supply ε_N of three, as in Prescott (2004). Elastic labor supply makes our model consistent with the high tax multiplier in the literature. We set the fraction μ of constrained agents to 0.35, in between the (near) 0.50 share used in Auclert, Dobbie and Goldsmith-Pinkham (2019) and the SIGMA model in Coenen et al. (2012) and the 0.25 in Drautzburg and Uhlig (2015) and other models in Coenen et al. (2012). We calibrate elasticities across goods produced in the two states and for individual varieties and the home bias as in Nakamura and Steinsson (2014). The demand elasticity between goods from the two states is two, and the demand elasticity within states is seven. The home bias in consumption is given by $\phi_H = \frac{2}{3} + \frac{1}{3}n$.

²⁸Letting the monetary authority respond to the output gap raises the tax multiplier in the economy, as it allows the monetary authority to accommodate expansions brought about by supply shocks.

Firms. Firms have a labor share in production equal to $1 - \alpha = 0.65$. The (annual) degree of price stickiness ξ is chosen to yield a (peak) surprise defense spending multiplier of 0.8 as in Ramey (2011). This yields $\xi = .735$ at an annual frequency; with GHH preferences the value is $\xi = 629$. These values are higher than in Auclert, Dobbie and Goldsmith-Pinkham (2019), but close to the annualized posterior mean of 0.72 for the monetary regime in Leeper, Traum and Walker (2017).

Federal government. The federal government adjusts lump-sum taxes on unconstrained agents to pay for expenditures, as in Nakamura and Steinsson (2014). Steady state federal income taxes are set at $\bar{\tau}^f = .22$, based on the data in Leeper, Traum and Walker (2017). The steady state share of IG aid in GDP is 0.02 and the steady state share of federal government consumption in GDP is set equal to 0.12. Last, the debt-to-GDP ratio is 0.7.

Monetary authority. We calibrate a monetary policy rule as in Galí (2008), with a persistence of $\rho_r = 0.75$, and coefficients for inflation and the output gap of $\psi_{r,\pi} = 1.5$ and $\psi_{r,y} = 0.5$, adjusted for the annual model frequency. The steady state inflation rate is 2%.

State policy rules. The steady state share of GDP of state government consumption (\bar{G}_{st}/\bar{Y}) is equal to 0.08. The state debt-to-GDP ratio is 0.075 and IG accounts for 2% of GDP. The average state income tax rate equals 0.05.²⁹

The central parameters in our calibration of state fiscal policy are the marginal propensities to spend of Democratic and Republican governors, as estimated in column (2) of Table 4. Only the estimated difference between partisan propensities is causally identified. We therefore benchmark one of the two MPS. Since we cannot reject the hypothesis of a zero effect of IG aid on Republican spending ($p = .72$), we set the Republican governor’s MPS from new IG aid to zero. The resulting MPS for a Democratic governor is therefore 1.576 when applying our RDD estimates to the full sample. We designate the home state to be run by a Republican governor, and the other state to be run by a Democratic governor; therefore for increases in IG transfers, $\psi_{IG} = 0.0$ and $\psi_{IG}^* = 1.576$.³⁰

Our analysis also performs two counterfactuals. First, we eliminate partisan differences in state responses to aid ($\psi_{IG} = \psi_{IG}^* = 1.576$). Second, we vary the share (n) of the national economy run by Republican (n ; $\psi_{IG} = 0.0$) or Democratic ($1-n$; $\psi_{IG}^* = 1.576$) governors

²⁹Using NBER TAXSIM (Feenberg and Coutts, 1993; Feenberg, 2019), the top marginal tax rate averages 0.053 in our sample. The sum of sales tax revenue, payroll tax revenue, and individual income tax revenue to personal income is similar, at 4.3 percent.

³⁰The model is nearly linear in the MPS parameters. Consequently, our counterfactuals regarding the size of the partisan differences are not sensitive to our benchmark.

to illustrate how shifting partisan control of state governments affects the impact of federal IG aid on the aggregate economy. Otherwise, states are identical and of equal size.³¹ As a conservative alternative to our RDD estimates, we also use the smaller OLS estimate of 0.702 as a lower bound estimate of the partisan differences.

Table 6: Calibrated Parameters of State Governments

Parameter	Value	Source
State government consumption $\frac{G_{st}}{Y}$	0.08	Data
State debt-to-GDP \bar{b}_{st}/\bar{Y}	0.075	Data
IG revenue $\frac{IG}{Y}$	0.02	Data
State tax rate $\bar{\tau}_{st}$	0.05	State marginal tax rate data
Republican MPS ψ_{IG}	0	Consistent with Table 4 column (2)
Democratic MPS ψ_{IG}^*	1.576	Implied by ψ_{IG} and Table 4 column (2)
same, OLS estimate	0.702	Implied by ψ_{IG} and Table 4 column (3)
State tax persistence ρ_τ	0.35	Match tax adjustment
Reaction of state taxes to debt $\psi_{st,b}$	0.99	Determinacy
Reaction of state taxes to net expenditure $\psi_{st,E}$	0.85	Match tax adjustment
Persistence of fiscal shocks $\rho_{IG} = \rho_G = \rho_\tau$	0.63	2009 stimulus duration
Standard deviation of IG shock ω_{IG}	0.83	2009 IG shock size

Notes: See the online Appendix Table E.9.1 for the complete list of calibrated parameters.

Besides the propensities to spend, the rule governing distortionary tax rates at the state level is important. Consistent with reduced-form regressions, we model state tax rates as autocorrelated and responding positively to increases in interest paid on debt and to expenditures net of IG transfers. Table 6 shows the value of the calibrated parameters used for state governments. (Table E.6.1 in the online Appendix lists all calibrated parameters.) Unlike the reduced form evidence, we impose a one-period lag and calibrate the parameters to yield a response of the state tax rate consistent with these micro estimates. The estimates imply no significant change in tax rate in the first one or two years, a fall after two years, and a reversal to zero after three years. To match these patterns with a parsimonious rule, we choose a persistence of 0.35, the loading on interest payments of $\psi_{st,b} = 0.99$, and a loading on expenditure net of IG revenue of $\psi_{st,E} = 0.85$. This calibration yields no tax rate difference in the first year. In the second and third year, the elasticity of the tax rate differential between Republicans and Democrats with respect to IG aid is -0.36 and -0.37, respectively, declining to -0.29 by the end of the third year.

IG aid. The annual persistence of shocks to federal IG aid is set equal to $\rho_{IG} = .63$ to give a half-life of six quarters, matching the duration of the 2009 ARRA stimulus of about

³¹To quantify the uncertainty surrounding our estimates of partisan differences, we compute confidence intervals using the Delta method, based on the asymptotic standard error of the estimated partisan difference.

three years; see [Drautzburg and Uhlig \(2015, Fig. 1\)](#). For our comparisons to the aggregate income effects of other federal spending and tax policies we set $\rho_G = \rho_\tau = .63$ as well. We calibrate the discounted cost of the IG aid in our simulations to the cost of the IG transfer component of the 2009 stimulus of 2.2% of 2009 GDP; see [Carlino and Inman \(2016\)](#). This calibration yields a shock standard deviation ω_{IG} of $100 \times (1 - \rho_{IG}) \times 0.022 \approx 0.83$: IG transfers rise initially by 0.83% of GDP, after a one standard deviation shock.

6.3 Results

We quantify the role of partisanship on the effects of a surprise increase in IG transfers in two scenarios. First, we show how the dynamics of the economy vary with the preference of the home (“Republican”) governor. Our focus is on how the aggregate responses to the IG aid increase change if the initially partisan Republican governor behaves as her Democratic counterpart. Second, we set the partisan differences at the level prevailing in the post-Reagan era and vary the share (n) of the national population living in the Republican state to show how IG transfer multipliers might have changed over time with changes in the partisan composition across U.S. governors.

Dynamics. Figure 9 shows the average state fiscal policies and output responses to a federal IG transfer shock. Since the states are the same size, we just show simple averages. Panel A shows state spending, Panel B state tax rates, and Panel C state output. Panels A and C also show the IG shock (the black dashed line) for comparison. The online Appendix Figure E.7.1 provides additional detail, such as separate responses for each state.

To isolate the causal effect of partisanship identified in the state-level analysis, all panels show two scenarios: First, the solid lines with squares show the baseline scenarios arising from our partisan differences in the MPS. Second, the dashed lines with squares show the counterfactual case without estimated partisan differences, i.e., when the Republican governor has the same MPS as the Democratic governor. The difference between these two scenarios is the effect of the partisan MPS differences shown as the thin solid line with the surrounding 90% confidence interval.

Figure 9 Panel A shows the responses of state expenditures to increases in IG aid. Following an initial exogenous increase in aid of 0.83% of GDP (shown as the simple dashed line), state expenditures increase by 1.30% ($= 1.576 \times 0.83$) when all governors have Democratic preferences (the dotted dashed line). With partisan differences – half of the states with Democratic preferences and half Republican preferences – the average spending increase is only half as large (the dotted solid line). The difference in spending between all Democratic governors and equal partisanship shows that partisanship reduces the increase

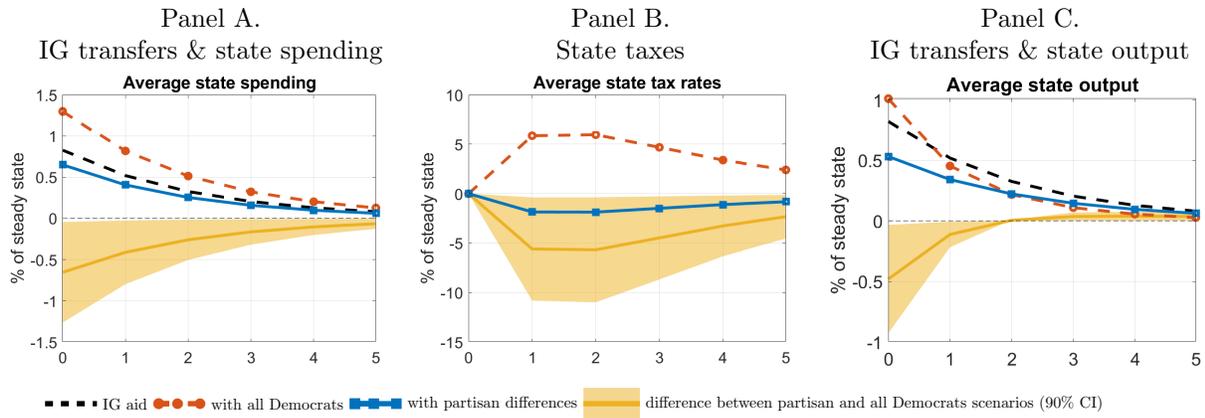


Figure 9: Impulse Responses to Federal IG Shock: Spending, Taxes and Output.

Notes: Results are for separable preferences.

in average state spending by 0.65% of GDP (the thin line) with a 90 percent confidence interval (-0.04%, - 1.26%).

Figure 9 Panel B shows the effects of partisanship on state tax rates. Again the path of average state tax rates when all governors have Democratic preferences is shown as the dotted dashed line, and that when there is equal partisanship as the dotted solid line. The thin line shows the difference between the two regimes, again significantly negative, and its 90 percent confidence interval with a peak difference of -5.7% (-0.4%, -11.0%). Democratic states need to increase state tax rates to finance state expenditures in excess of the increase in federal IG aid. In contrast when half the states are run by Republican governors, average state tax rates fall as Republicans allocate aid to tax cuts.

Figure 9 Panel C shows the impact of these partisan policy responses on average state output as private agents and the monetary authority react to federal and state fiscal policies. If all governors have Democratic preferences, average state output rises initially by 1.0% of GDP. When half the states are Democratic and half Republican, average state output rises by 0.5% of GDP initially. On impact the effect of partisanship is thus to lower the increase in output by 0.5% of GDP with a confidence interval of (-0.0%, -0.9%). However, as shown in Panel B higher state taxes in the Democratic states and lower state taxes in the Republican states will mean greater average output of 0.05% beginning in year 3 as Republican state tax cuts stimulate future work effort and consumption by private agents. While small, these future differences accumulate and suggest a possible policy trade-off between the impact and long-run multipliers for IG aid conditional upon the degree of state partisanship.

Comparing multipliers. Given partisan differences, how much does each dollar of new federal IG aid stimulate the economy? And how does the IG multiplier change as preferences of state policymakers change? We follow [Mountford and Uhlig \(2009\)](#) and analyze present discounted value (PDV) multipliers, defined as the ratio of the PDV of output relative to the PDV of federal outlays, for either IG transfers, defense spending, or forgone tax revenue.

Table 7 shows the multipliers for our baseline calibration in the top two rows. The first row shows the impact multiplier and the second row the long-run multiplier. Column (1) presents the counterfactual with all Democratic governors. The ratio of the predicted impact output increase of 1.01% relative to IG aid initial spending increase of 0.83% of GDP – both in Figure 9 Panel C – yields an impact multiplier of 1.22 shown in the first row of Table 7, column (1).³² However, as Democratic states need to raise future distortionary taxes on labor income to pay for aid-induced expenditures, the long-run multiplier falls to 0.64. When half the states are Republican-led, we observe the opposite pattern; the long-run multiplier now equals 0.80 and exceeds the short-run multiplier; see column (2). The result follows from Republican governors’ decision to save aid and offer (delayed) tax cuts in contrast to Democratic governors’ decision to increase spending immediately. Column (3) presents estimates of the difference in multipliers between the all Democratic and equal partisanship regimes along with the estimate’s standard error. The impact multiplier is 0.58 lower with equal partisanship, while the long-run multiplier is 0.16 higher.

A comparison of our estimates of IG multipliers to our estimates for federal spending and tax multipliers benchmarked to [Ramey \(2011\)](#) estimate for the federal spending multiplier of 0.80 in columns (4) and (5) illustrates the importance of partisan differences choosing to use IG aid or direct federal spending or tax cuts to stimulate the economy. A greater share of governors who are Democrats (Republicans) favors the use of IG aid if the objective is to stimulate the aggregate economy in the short-run (long-run). As stressed earlier, partisanship raises the important question of the appropriate time horizon for evaluating policy, here IG aid.

Limitations and robustness. While our results point to an economically significant effect of partisanship on the IG multiplier, there are limitations to our analysis. The small number of close elections in our sample constrains our RDD estimation, making the estimates of partisan differences, and thus multipliers, less precise. Also, partisan differences may not be constant across all margins of victory. As Figure 4 shows, OLS estimates indicate partisan differences for margins of victory of up to 30 percentage points. Within this range, the OLS

³²The “demand equivalence” approach by [Wolf \(2020\)](#) is a useful benchmark. Absent distortionary taxes, demand equivalence implies an impact IG multiplier with only Democrats of $0.8 \times 1.576 = 1.26$, where 0.8 is the calibrated defense spending multiplier. With an equal partisan split, the multiplier is only half as high.

Table 7: Impact and Long-Run Multipliers

Utility function	MPS estimate	Multiplier horizon	IG increase			Comparison	
			(1) All Democrats	(2) Equal partisanship	(3) Difference column (2)-column (1)	(4) Federal defense	(5) Federal tax cut
Separable	RDD	Impact	1.22	0.64	-0.58 (0.33)	0.80	0.85
		long-run	0.64	0.80	0.16 (0.09)	0.76	0.92
Separable	OLS	Impact	0.58	0.32	-0.26 (0.07)	0.80	0.85
		long-run	0.82	0.89	0.07 (0.02)	0.76	0.92
GHH	RDD	Impact	1.10	0.69	-0.41 (0.23)	0.80	1.64
		long-run	-0.27	1.06	1.33 (0.78)	0.69	2.06

Notes: This table presents both impact and long-run present discounted value (PDV) multipliers for positive shocks to IG aid (columns (1 - 3)), to federal defense spending (Column (4)), and to federal tax cuts (Column (5)) under different calibrations and preferences. Column (3) shows standard errors in parentheses.

suggests that the partisan differences may not be constant. Shedding more light on how the partisan difference varies by MOV would be an important extension. Instead, we use the OLS estimate of partisan differences based on all elections from Table 4 column (3) to provide a conservative estimate for the average impact of partisan differences. Rows 3 and 4 of Table 7 show the results based on OLS estimates. The impact multiplier is now -0.26 lower with equal partisanship than with all Democratic governors, with a standard error of 0.07. The long-run multiplier is now 0.07 higher with equal partisanship with a standard error of 0.02. The impact of partisanship remains significant.

Since we model Republicans as favoring (delayed) tax relief and Democrats as favoring purchases, the underlying tax and purchase multipliers are key. Ramey (2019) provides a survey of both. By construction, we match the mean spending multiplier of 0.8 in Column (4) of Table 7. However, the (negative of the) tax multiplier, reported in column (5), is only 0.85, lower than the range of 2 to 3 in Ramey (2019), and only about half of the anticipation-adjusted impact tax multiplier around (minus) 1.5 in Mertens and Ravn (2014). To address this discrepancy, we re-solve our model with GHH preferences. As the second to last row of Table 7, column (5) reports, this generates a tax multiplier of 1.64, albeit at the cost of no longer matching our micro estimate of the relative GDP at longer horizons. The higher tax multiplier reduces the partisan effect on the IG impact multiplier to a still sizable -0.41, from -0.58 (row 5, column (3)). Importantly, Republican tax cuts stimulate the economy more strongly, raising the long-run multiplier by 1.33 compared to a counterfactual with only Democratic policy preferences in all states.

We conclude that if the policy objective is to stimulate the aggregate economy and if IG aid is the preferred policy, then having relatively more Democratic governors setting state

fiscal policies will enhance the impact of policy in the short-run. In the long-run however the results tend to favor a greater share of Republican governors.

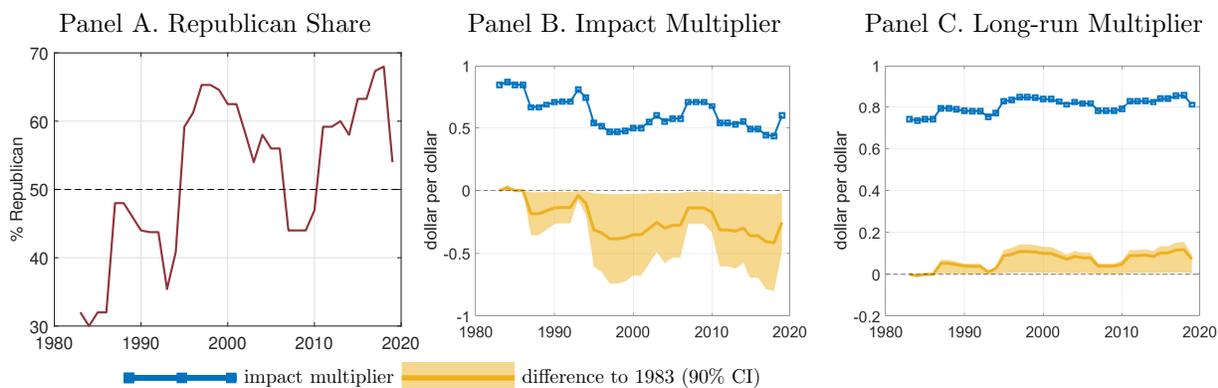


Figure 10: Republican Governor Share and Simulated IG Multiplier

Notes: Panel A shows the Republican share of governors, 1983-2019. Panel B shows the simulated impact multipliers for IG transfers for the baseline specification with separable preferences. The solid dark line traces the value of the simulated impact multiplier as the share of Republican governors changes over time. The thin line and the associated 90% CI show the difference between each year’s multiplier compared to a benchmark value of 0.87 when the Republican share of governors equaled its lowest value of .30 in 1984. Panel C shows the long-run multiplier compared its 1984 benchmark value of 0.74 (solid dark line) and its difference from that 1984 benchmark (thin line) and associated 90% CI.

Multipliers over time. In all cases, it is important to know how state governments will allocate federal IG transfers and, for us, how any partisan differences in governor preferences for state spending or state tax cuts will impact the aggregate economy. Figure 10 shows the response with our baseline (separable) preferences. Panel A shows the fraction of states governed by Republican governors over the post-Reagan period, omitting the rare independent governors. This Republican fraction ranges from a low of 30% in 1984, just after Reagan took office, to a high of 67% during President Trump’s term. Using these values to calibrate n in our model translates into sizeable differences in the IG impact multiplier, shown in Panel B. Allowing for estimation uncertainty of partisan effects yields the confidence interval for the difference between each year’s estimated impact multiplier (Panel B, line with markers) and the baseline multiplier estimate for 1984 when the Republican share was at its lowest. The IG impact multiplier peaks during periods of low Republican governorships, showing its highest values of 0.87 for 1984 and 1993. The thin line shows the (negative) difference between subsequent multipliers and its maximum in 1984; the shaded area represents the 90% CI for this difference. The maximal difference is -0.42 (1984 vs. 2018) and has a 90% CI of (-0.03, -0.80). In contrast, the long-run (PDV) multipliers average near 0.8

and have risen by no more than 0.13 (0.01, 0.18) despite the large increase in the share of Republican governors. It is for the impact multipliers that partisanship matters most.

7 Conclusion

While well understood for the implementation of microeconomic policy in fiscal unions, IG aid as a tool for macroeconomic stabilization policy has only recently received serious scholarly attention, primarily because of the importance of such aid in the U.S. government's response to the Great Recession. The American Recovery and Reinvestment Act of 2009 allocated \$318 billion to state and local governments. The Covid recession has also called for significant assistance to state and local government: Over the past two years, the CARES Act of 2020, the CARES Act of 2021, and the American Rescue Plan have allocated \$628 billion to state and local governments, not counting federal transfers for the state-run unemployment insurance programs. Finally, the recently approved Infrastructure Investment and Jobs Act of 2022 will spend \$260 billion for physical infrastructure, again primarily allocated as IG aid to states. The results presented in this paper suggest state partisan preferences will play an important role for how these funds are allocated. Tellingly, the third of the COVID relief bills, which was passed by a unified Democratic federal government, included a provision aimed specifically at preventing states from using new IG aid for tax relief, a policy Republican governors might prefer.

As fiscal “agents” of federal policy, states have significant discretion in how federal IG aid is finally allocated. Our empirical work documents that state partisan preferences are likely to play a significant role in these allocations. If the allocation of IG aid to spending or revenue relief differs by the partisan preferences of state politicians, one would expect to see differences in economic activity by state partisanship as well.

Our New Keynesian model helps us understand why partisan differences might matter. First, the relatively higher marginal propensity to spend federal transfers shown by Democratic governors has an immediate positive impact on their states' GDP. In contrast, Republican governors' propensity to cut state taxes, but only with a lag, delays the impact of aid on GDP in states with Republican governors. GDP in Republican-led states does eventually rise above GDP in Democratic-led state, however. Tax cuts induce gains in output in Republican-led states that lead to larger long-run multipliers for those states. These results suggest a trade-off in our policy choice, conditional upon the share of states run by Democratic or Republican governors. A larger share of governors who are Democrats will mean – by our analysis – relatively greater increases in aggregate output in the short-run following an increase in IG aid but relatively lower discounted gains in output in the long-run. Given

the growing importance of IG aid in fiscal unions, we have identified a new and potentially important source of model heterogeneity – state partisanship – requiring our attention.

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