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

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Abstract

In economic unions the fiscal authority consists not of one, but many governments. We analyze whether partisanship of state-level politicians affects federal policies, such as fiscal stimulus in the U.S. Using data from close elections, we find partisan differences in the marginal propensity to spend federal transfers: Republican governors spend less. This partisan difference has tended to increase with measures of polarization. We quantify the aggregate effects in a New Keynesian model of Republican and Democratic states in a monetary union: Lowering partisan differences to levels prevailing during less polarized times increases the transfer multiplier by 0.30. The observed changes in the share of Republican governors lead to variation in the multiplier of 0.20 in the model. Local projection methods support this prediction.

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

Economic unions as collections of politically independent but integrated economies implement policy for many democracies, including the U.S. In such unions, the national government is responsible for national public goods, such as defense, and for stabilization policies. But even in implementing national policies, subnational governments are often important. For example, the 2009 American Recovery and Reinvestment Act (ARRA) allocated $318 billion of its $796 billion stimulus budget to U.S. state and local governments. Such nationally funded but locally implemented policies give rise to a principal-agent relationship: the national government, the principal, pays intergovernmental (IG) transfers to subnational governments such as states to provide services. In the U.S., federal transfers to states have grown ten times faster than GDP after WWII and now account for more than 3% of GDP. Yet, this principal-agent relationship has agency problems, as state governments have been documented to not fully spend transfers (Hines and Thaler, 1995) and to divert funds for other purposes (Nicholson-Crotty, 2004).[1] We argue that political partisanship is an important source of such agency frictions: When federal stimulus money is channeled through the states, state partisanship has first order effects on the resulting national multiplier.

There is a popular notion that partisanship in state-level politics may hamper national policies. Consider the recent expansion of Medicaid. The Washington Post (2013) reported that, at the state level, Republican politicians often blocked the Medicaid expansion that formed part of the Democratic healthcare reform bill. On a smaller scale, other federal programs have seen partisan uptake.[2] We argue that the partisan affiliation of governors has been important more broadly, namely for determining the effects of federal transfer programs at least since the presidency of Ronald Reagan in the 1980s.

We begin by estimating the impact of state-level partisanship on the implementation of

[1] The principal-agent relationship between national and state governments has been long appreciated in the design of national micro-economic policy; see, e.g., Craig and Inman (1982).

[2] For example, only Republican states have introduced work requirements for Medicaid recipients introduced under President Trump, see Kaiser Family Foundation (2019). Funding for "abstinence-only-until-marriage" education programs was rejected by Democratic-run states, see Raymond et al. (2008).

national government policies paid for by IG transfers. We measure partisanship through the political party of the governor, who largely determines state budgets (Kousser and Phillips, 2012). Central to our analysis is the identification of partisan differences in state responses to national transfers – that is, the marginal propensity to spend federal aid (MPS) under Democratic or Republican governors. We identify partisan differences in spending using cross-sectional data on close gubernatorial elections, similar to the regression discontinuity design (RDD) used by Ferreira and Gyourko (2009) in their study of mayoral partisan differences in government spending. We find statistically significant and economically important differences in the fiscal choices of closely elected Republican and Democrat governors. Compared to the pre-Reagan era, governors who are Democrats typically spend one dollar more for each dollar received in federal aid than do governors who are Republicans. Our results suggest that Democrats favor spending; Republicans favor tax relief. These partisan differences in policy choices largely emerged during and following the tenure of President Reagan, a Republican. They are now firmly in place, in line with the increased partisan polarization of U.S. politics documented by McCarty et al. (2016) and Azzimonti (2018).

The partisan differences in states' responses to federal policies not only imply differential state-level effects, but may also determine the aggregate effects of such policies. To quantify national effects, we need a macroeconomic model since our state-level analysis only speaks to cross-state variation. We use the causal state-level estimates to parameterize the partisan fiscal rules of representative Democratic and Republican governors in a New Keynesian model of states in a monetary union. The model is otherwise similar to Nakamura and Steinsson (2014) and Auclert et al. (2019). It gives a role to demand-side and supply-side policies through nominal frictions, constrained households, and distortionary taxes as well as labor supply and capital accumulation. The model is thus flexible enough to give a role to Democratic policies, which are estimated to favor increased spending, including transfers to lower income households, as well as Republican policies, which favor tax relief for households. The model calibration balances these forces by matching the multiplier for shocks to defense spending – not our policy – as estimated by Ramey (2011). We measure the aggregate effect of state-level partisanship by its impact on the national IG transfer multiplier. This multiplier matters for national policymakers trying to stimulate the economy via IG transfers.

We first benchmark the model to remove partisan differences between governors and find that the aggregate impact multiplier of federal transfers to state governments has an impact multiplier of almost 1.0. We then provide two scenarios. First, we assign half the states to have governors with Republican preferences and half with Democrat preferences. The resulting impact multiplier is 0.3 lower than in the counterfactual without partisan differences. The result is due to the initially lower aggregate demand in Republican states because of less stimulus spending. The second scenario varies the share of governors with Republican preferences. From 1983 to 2014, the share of Republican governors has varied from 30 to 68 percent. For our baseline calibration, this variation in the fraction of Republican governors causes the aggregate impact multiplier to vary from 0.6 to 0.8: The impact multiplier declines as the share of Republican governors rises. Local projection estimates confirm this prediction of the model: The estimated response of aggregate GDP to an innovation in intergovernmental transfers falls with the share of Republican governors. Our analysis thus points to partisanship of policy-makers as a novel source of heterogeneity in macroeconomics, beyond the well documented importance of households (e.g. Kaplan et al., 2018) and firm heterogeneity (e.g. Ottonello and Winberry, 2018). Time variation in partisanship is also a novel source of time-dependence of fiscal multipliers, different from the economic slack commonly considered (Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2014).

Literature. While many studies focus on government consumption and investment, or government expenditure as a whole, this paper contributes to a literature that highlights differential effects of various spending measures. Bermperoglou et al. (2017) analyze the government wage bill. Oh and Reis (2012) analyze government transfers. Drautzburg and Uhlig (2015) provide separate multiplier estimates for government consumption, government investment, and government transfers to households. Our focus, in contrast, is on intergovernmental transfers. Carlino and Inman (2016) also analyze IG transfers, and Chodorow-Reich et al. (2012) analyze IG transfers as part of the 2009 ARRA stimulus. Compared to their work, we focus on partisan differences and also characterize national multipliers.

A number of papers have estimated cross-sectional multipliers, see Chodorow-Reich (2019) for a recent review. As emphasized by Chodorow-Reich (2019) and Nakamura and Steinsson (2014), cross-sectional multipliers differ from national multipliers because of spillovers, differences in financing, and monetary policy. Here, we use our New Keynesian model to take these effects into account. When monetary policy does not react, for example, because it may be constrained by the lower bound on interest rates, fiscal multipliers are larger, and so is the effect of partisan differences in our model.

Our paper also fits in an established literature in macro-political economy. As Alesina (1988) discusses, there is a literature that analyzes coordination games with different policymakers running different economies at the same time. In our model, different policymakers run different state economies at the same time. However, we take the equilibrium of the political game as given, and aim to estimate the resulting partisan policy rules. Our positive analysis is thus similar to the cross-country work by Hibbs (1977) on political parties and the macroeconomy. Similar to how Alesina and Sachs (1988) find support for the "'partisan view' of monetary policy" (p. 79) in the U.S., we find empirical support for partisan differences in state-level fiscal policy. This is in contrast to the negative result of Ferreira and Gyourko (2009) for U.S. cities, but is consistent with the more recent finding of partisan differences in pension funding under Democratic and Republican mayors (Dippel, 2019). Leigh (2008), Beland (2015), and Hill and Jones (2017) also find significant partisan differences between governors. We show that partisanship at the state level also has aggregate effects.

Structure. Section 2 describes the empirical specification and the data. Section 3 presents state-level findings. Section 4 analyzes our macro model with the state-level estimates. Section 5 uses time series data to test model predictions. Section 6 concludes.

2 Empirical specification and data

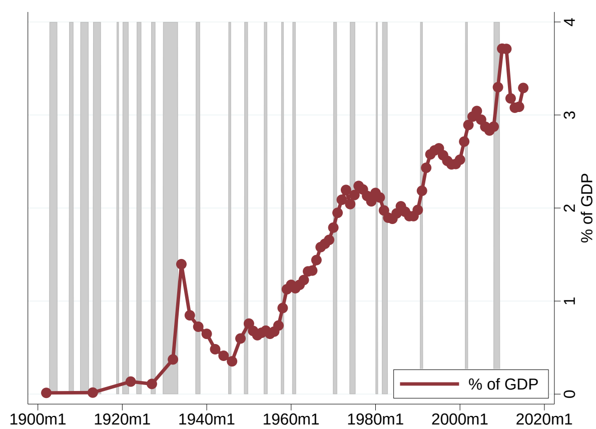
We begin this section by providing evidence that governors are key actors when it comes to state budgets, and that federal IG revenue is a key component of state budgets. We then derive the estimating equation and describe the data and sample period.

2.1 The role of governors and IG transfers

Governors in most U.S. states are in a strong bargaining position when it comes to setting the state budget. The line-item veto, recognized by all but seven states, gives the governor particularly strong powers to check any significant deviations from her initial agenda; see Holtz-Eakin (1988) and Bohn and Inman (1996). And, while legislatures generally can afford not to pass new legislation, since the fallback is just the status quo, not passing a budget is politically costly for legislatures (Kousser and Phillips, 2012). Divided government and increasing partisanship between legislative parties make a coordinated legislative effort to undo the governor's budget very difficult; see McCarty et al. (2016, chapter 8) and Bohn and Inman (1996) for evidence from state budgets. It is therefore unsurprising that Kousser and Phillips (2012) conclude that "each dollar of . . . changes proposed by the governor in January translates into roughly 70 cents in the final budget deal."

Underlying our analysis is also the assumption that states have discretion in the use of federal IG transfers. This may not be obvious: with the exception of "general revenue sharing," a program introduced by Nixon and axed by Reagan, IG transfers take the form of aid and grants. These funds are intended for specific types of spending. However, states do circumvent these rules. Nicholson-Crotty (2004) provides two illustrative examples: For Medicaid, he summarizes how "states were using their federal matching funds to supplant, rather than supplement, state-level funds." (p. 114) Similarly, for the largest category of funds in the area of criminal justice in the early 1990s, he concludes that "there is essentially no monitoring of individual states by the Justice Department" (also p. 114). This is consistent with the survey in Hines and Thaler (1995), which finds that subnational governments spend as little 25 cents for each dollar received in intergovernmental transfers.

Intergovernmental transfers have risen sharply in the U.S., as Figure 1 shows. While the first ongoing federal cash grant to states dates back to 1879 (Congressional Research Service, 2019), IG transfers to states still accounted for as little as 0.01% of GDP in 1902. The modern system of transfers took off during the Great Depression, with transfers peaking at 1.4% of GDP in 1934, before dropping back to 0.36% of GDP after WWII. From 1946 to 2010, however, transfers grew ten times faster than GDP, peaking in the Great Recession at 3.7% of GDP, before declining modestly to 3.1% of GDP in 2014 and ticking up slightly with the Medicaid expansion in 2015. While part of the increase in transfers in big downturns was automatic, since the unemployment insurance is state-run and partly funded through federal grants, the 2009 stimulus bill included large discretionary transfers to U.S. states (Carlino and Inman, 2014).



Source: U.S. Census for grant data, Ramey and Zubairy (2014) for historical GDP data, and authors' calculations. Gray shaded areas indicate NBER recession dates.

Figure 1: U.S. federal intergovernmental transfers to states since 1902

Federal IG transfers have become an important source of state revenues. Averaged over our main sample period from 1983 to 2014, these transfers account for 28% of state revenue, second only to tax revenue. Among the tax revenue, sales taxes account for almost half. Almost 30% of tax revenue comes from individual income taxes. Figure A.3 in the Appendix shows these numbers and provides additional details.

A large part of the expenditure of states is transfers to households and municipalities. Breaking down expenditures by type, we find that, on average, 13% are transferred to households, 25% are transferred to municipalities, and 8% go towards capital outlays, with the remainder spent on capital outlays. See Figure A.3 in the Appendix. A break in the underlying data makes it harder to split expenditures by end use. We can only match 66% of expenditures across the sample break. The three largest matched positions are education spending (31% of the total), public welfare (20%), and infrastructure spending (8%).

2.2 Empirical strategy

Our focus is on recovering the average MPS, denoted γ_p, for Democratic and Republican governors, ($p \in \{D, R\}$), out of intergovernmental transfers, IG. Let s denote a state and t denote time. Then one could naively regress per capita expenditures, $E_{s,t}$, on per capita transfers, $IG_{s,t}$, and a constant, μ_p, separately for Republican and Democratic governors. Letting $\epsilon_{s,t}$ denote the error term yields the following regression:

$$E_{s,t} = \mu_p + \gamma_p IG_{s,t} + \epsilon_{s,t}, \qquad p \in \{D, R\}. \tag{2.1}$$

In practice, the challenge in running such a regression is to distinguish policymakers' preferences from those of the electorate and socio-economic conditions in the state. While, as Besley and Case (2003) point out, much variation can be accounted for by fine enough fixed effects or possibly control variables, it is hard to fully control for omitted variables. For example, IG inflows could promote a "big-government" attitude among the electorate, which, in turn, could be associated with voting for Democratic governors and more spending. We might then falsely identify a higher MPS for Democrats, simply because Democratic governors tend to be selected when transfers and expenditures are high.

An obvious approach to control for unobservables is to condition on close elections. Intuitively, we can estimate equation (2.1) by OLS regressions of expenditure, $E_{s,t}$, on IG transfers, $IG_{s,t}$, separately for Democratic and Republican governors, i.e., for $p \in \{D, R\}$. For consistency, the covariance $\mathrm{Cov}_p[IG, \epsilon_E]$ of IG revenue with expenditure growth residuals should vanish for Democrats and Republicans. Generally, however, γ_p is biased and inconsistent, as omitted variables may cause a non-zero covariance between IG growth and expenditure growth residuals. Our strategy is to difference this bias out:

$$\hat{\gamma}_\Delta \equiv \hat{\gamma}_R - \hat{\gamma}_D = \gamma_R - \gamma_D + \frac{\widehat{\mathrm{Cov}}_R[IG, \epsilon_E]}{\widehat{\mathrm{Var}}_R[IG]} - \frac{\widehat{\mathrm{Cov}}_D[IG, \epsilon_E]}{\widehat{\mathrm{Var}}_D[IG]}. \tag{2.2}$$

If the bias is the same for Democrats as for Republicans, it cancels out and we have identified the causal difference in MPS. The bias will be the same if the distribution of IG transfers and expenditure residuals is the same for governors of either party. We assume that this is satisfied under two conditions: First, we control for the margin of victory (MOV). The MOV controls for the unobserved heterogeneity between state-years with Democratic or Republican governors.

Second, we need to exclude IG transfers related to welfare programs to remove any endogeneity related to these programs. Welfare IG programs, such as Medicaid, are partially funded by federal grants with a requirement for matching funds by states. In these cases, the federal IG transfers are a function of state expenditures. A simple way to model this endogeneity is to represent IG as the sum of an exogenous component $X_{s,t}$ and a multiple θ of state expenditures: $IG_{s,t} = X_{s,t} + \theta E_{s,t}$, where expenditure follows (2.1) and thus differs by party. We show in Appendix B.2, that when the standard deviation, denoted ω_p, of the exogenous spending component is equal to ω for both parties, the (asymptotic) difference between propensities to spend is:

$$\hat{\gamma}_{R,OLS} - \hat{\gamma}_{D,OLS} \xrightarrow{p} (\gamma_R - \gamma_D)\frac{\mathrm{Var}[X]}{\mathrm{Var}[X] + \theta^2\omega^2}.$$

This difference between propensities to spend is proportional to the object of interest $\gamma_R - \gamma_D$, but is downwardly biased. The factor of proportionality approaches unity as the role of matching declines to zero, either because IG is largely exogenous ($\mathrm{Var}[\omega]/\mathrm{Var}[X] \to 0$) or because $\theta \searrow 0$. As a consequence, we exclude welfare spending, so that the remaining matching rate θ is low and non-welfare IG is dominated by exogenous factors. We also use the difference in estimates based on overall IG transfers and non-welfare IG transfers to verify the downward bias with $\theta > 0$.

Expenditures and IG revenue have grown secularly. When taking the model to the data we use log differences and fixed effects to make the model stationary. We allow for state-specific trends by removing state fixed effects in growth rates.

We also analyze transfer increases and transfer cuts separately. Increases in IG transfers are the norm for states, accounting for 70% of all observations for total IG and 60% without welfare IG. One may expect governors to react differently to transfer increases and decreases: For example, a partisan policy preference to lower taxes over high spending would result in tax cuts after positive transfer shocks and spending cuts after negative transfer shocks. Pooling positive and negative shocks would not allow this behavior. In addition, institutional or political constraints may push states to react differently even in the absence of partisan differences. For example, a self-interested bureaucracy may make it easier to increase spending than to cut it. In contrast, limits on borrowing by states may have the opposite effect.

Our baseline empirical specification resembles a regression discontinuity, where the partisan effect at the cutoff is mediated by IG growth:

$$\begin{aligned}\Delta E_{s,t} =& (\gamma_{0,+} + \gamma_{r,+} \times Rep_{s,t-1})\Delta \ln IG^{+}_{s,t} + (\gamma_{0,-} + \gamma_{r,-} \times Rep_{s,t-1})\Delta \ln IG^{-}_{s,t} \\ &+ \sum_{\mathfrak{s}\in\{-,+\}} (\gamma_{0,\mathfrak{s},m} + \gamma_{r,\mathfrak{s},m} \times Rep_{s,t-1})\Delta \ln IG^{\mathfrak{s}}_{s,t} \times MOV_{s,t-1} \\ &+ (\beta_{0,m} + \beta_{r,m} \times Rep_{s,t-1})MOV_{s,t-1} + \mu_0 + \mu_r \times Rep_{s,t-1} + \text{fixed effects} + \epsilon_{s,t}. \quad (2.3)\end{aligned}$$

$\Delta E_{s,t}$ is (log) expenditure growth. $IG^{+}_{s,t}$ denotes increases in IG transfers, and $IG^{-}_{s,t}$ denotes transfer cuts. In extensions, we replace $\Delta E_{s,t}$ with revenue or other outcomes. $Rep_{s,t-1}$ is a dummy for Republican governors at the time the state budget was passed. $MOV_{s,t-1}$ is the corresponding margin of victory. Here, $\Delta \ln IG^{+}_{s,t} \equiv \max\{0, \Delta \ln IG_{s,t}\}$ and $\Delta \ln IG^{-}_{s,t} \equiv \min\{0, \Delta \ln IG_{s,t}\}$. $\epsilon_{s,t}$ is the error term, which can be correlated across states and time. For statistical inference, we therefore cluster the standard errors by state and year, using the `reghdfe` package for `Stata` (Correia, 2016). Fixed effects always include either state or party by state fixed effects as well as either year, broad census region by year, or party by year fixed effects. In what follows, we focus on the estimates of $\gamma_{r,+}$ and $\gamma_{r,-}$, that is, on how the pass-through elasticity changes when the governor is Republican rather than a Democrat. If Republicans have a lower MPS out of positive IG growth than Democrats, then $\gamma_{r,+}$ is negative.

To estimate the our baseline equation, we choose a cutoff to trade off bias and variance of the estimates. Specifically, we use cross-validation to compute the root-mean-squared error (RMSE) of fitting equation (2.3) to our data. We find that a 10pp cutoff yields the minimum RMSE.[3] The specification with linear MOV controls is similar to that in Caetano et al. (2017). For robustness, we also estimate two additional specifications: A richer specification with third-order MOV polynomials in the full sample and a more parsimonious specification without MOV controls with governors elected with a MOV of up to 5pp.

Our baseline specifications allow for a Texan Republican to differ from one in California – and for Democrats to act, on average, differently during the Obama presidency than during the Trump presidency. Formally, we use party×state and party×year fixed effects. This captures concerns that southern Democrats or New England Republicans are, on average, different from their average party colleague. In addition, it controls for the possibility that a president of one party may strategically direct IG transfers more so to states run by fellow Democrats or Republicans – or that governors decline transfers offered by a president of

[3] Specifically, we estimate (2.3) first leaving one state out at a time, and then one year out at a time. We then compute the root-mean-squared-error (RMSE) for omitted observations. We repeat this process for MOV cutoffs on a one percentage point grid, and choose the cutoff that minimizes the RMSE averaged across leaving out state and leaving out years. With fixed effects, the resulting cutoff is 10pp. Without fixed effects, and thus a larger effective sample size, the resulting cutoff is 6.5pp.

the other party. For example, Republican governors turned down funds during the recent Medicaid expansion. Our fixed effects thus isolate the within-state variation in political outcomes and the between-state variation in IG transfers and fiscal policy.

For ease of interpretation, it is useful to transform γ_p from an elasticity to a dollar coefficient. To do that, we may simply use the average ratio of expenditures to IG transfers. This ratio, however, also varies between states. Instead of converting the elasticity to dollars after running the regression, we can also transform the left-hand-side variable directly. Using the scaled variable $\frac{E_{s,t-5}}{IG_{s,t-5}}\Delta E_{s,t}$ on the LHS has the virtue that it reflects heterogeneity in the ratio of expenditures to transfers across states in our estimation sample. This is similar to regressing the (real, per capita) dollar change in expenditures on the (real, per capita) dollar change in transfers, but we have found it to be more robust.

2.3 Data sources

We construct a panel data set encompassing fiscal and political outcomes in U.S. states from 1963 to 2014, supplemented with macroeconomic indicators. Our main data source on governors is the Council of State Government's Book of States. For state fiscal data, we use the U.S. Census Bureau's State and Local Government Finance historical database for 1958 to 2006 by fiscal year. The data for 2007-2014 come from the Census Bureau's Annual Surveys of State and Local Government Finances.

We also use data on the economic characteristics such as GDP and population from the U.S. Bureau of Economic Analysis' Regional Economic Accounts by calendar year. To merge the data set, we line up state fiscal years with the calendar years straddling the end of the previous fiscal year and the beginning of the current fiscal year, to best reflect states' contemporaneous information. In addition, we use annual and quarterly National Income and Product Account (NIPA) data: We use the GDP deflator to deflate all nominal variables at the state level. And we use quarterly data on GDP, state aid, and other variables to test the time series predictions of our model. Appendix A provides a detailed discussion and specific variable definitions.

2.4 Sample selection

We merge our data to reflect the predominant state fiscal year definition. Almost all state fiscal years run from July through June. In our analysis, we relate the expenditures in a given fiscal year to the political majorities in the previous fiscal year because the state budgets are passed in the six months leading up to the new fiscal year.

For our main analysis, we begin our estimation sample in the (state) fiscal year of 1983. This fiscal year is the first fiscal year that states knew the Reagan policies: Reagan took office in 1981 and the first new federal fiscal year in his presidency began in September 1, 1981. As indicated, the fiscal years begin in July in most states, whereas the federal fiscal year begins on September 1. States could react to the 1981 federal budget during their budget deliberations for FY 1983 that took place in the first half of 1982. Some of our results depend on excluding the pre-Reagan years, and we analyze this time-dependence explicitly below.

We drop states that have large sovereign wealth funds financed through severance taxes. Spending by these states follows the predictions of the permanent income hypothesis more closely, contrary to the notion of the flypaper "anomaly" (Hines and Thaler, 1995) that we are interested in. States that have sovereign wealth funds have explicit requirements on revenues and expenditures. For example, the Alaska Constitution mandates that at least 25% of oil revenue is deposited in its wealth fund. Such fiscal rules and the potential to use these funds to smooth expenditures or taxes may create problems for our model. We thus drop the states starting in the year that they instituted their wealth fund: Wyoming (1975), Alaska (1976), and North Dakota (2009).[4]

As discussed above, when using the specification (2.3) with linear MOV interactions, we find that a cutoff of 10pp minimizes the RMSE when using fixed effects. Without fixed effects, a cutoff of 6.5pp minimizes the RMSE. When using our parsimonious specification,

[4]Only these states receive 20% of their revenue from severance taxes. Our main results are robust to including these states.

equation (2.3) without MOV controls, we use MOV cutoffs of up to 5pp, with 4pp as our default. A 4pp MOV corresponds to a 52.0% Democratic vote share with the remaining 48.0% going to the Republican candidate, if no votes were cast for independent candidates. Figure A.1 in the Appendix shows the number of marginally elected governors by year. Even for the 4pp MOV cutoff, all years have marginally elected governors from both parties.

2.5 Descriptive statistics

The descriptive statistics for the variables used in our analysis are displayed in Table 1. Besides providing context for the regression analysis, it allows us to consider the assumption that states with narrowly elected governors are similar. Intuitively, we are assuming that, conditional on close elections, the unobserved state characteristics, as well as the (observed) IG revenue are identically distributed on both sides of the chosen cutoff. If these or other observables are very differently distributed for closely elected Republicans than for closely elected Democrats, this difference would cast doubt on the assumption that unobservables are about the same for both parties.

Specifically, Table 1 provides information on our full sample since the Reagan era, the sample with close elections, and the close election sample split by governor's party. It also provides t-statistics that evaluate the assumption of equal sample means for closely elected governors – without any fixed effects, with state and year fixed effects, and with state and census region×year fixed effects. Note that our preferred fixed effects, which are party×year, state fixed effects, eliminate any mean differences between party and are thus omitted. The table uses the 10pp MOV cutoff and controls for linear MOV in the regressions used to compute the t-statistic for the sample means. About 42% (636 out of 1508) of all state-years have governors elected with a MOV of 10pp or less. Table A.1 in the Appendix uses the 4pp MOV cutoff and does not use controls. The table displays results similar to those in Table 1. For the 4pp MOV, the corresponding number is 18%. For comparison, the median absolute MOV is 13pp. Each table covers variables entering our regression analysis in the top part,

Table 1: Sample means of main variables and the significance of partisan differences in close election samples, 1983–2014.

	Full sample	Sample with close elections			Dem=Rep t-stat by FE		
	1983-2014	Within 10pp.	Dem<10pp.	Rep<10pp.	None	St+Yr	St+Reg×Yr
Expenditure growth	2.6	2.6	2.8	2.5	0.7	-0.7	-0.2
Net general rev gr	2.2	2.6	2.6	2.6	0.3	-0.6	-0.2
Income sales tax rev gr	2.1	2.4	2.3	2.5	0.5	0.1	0.1
Tax rev growth	2.0	2.3	2.2	2.4	0.6	0.1	0.1
IG growth	3.3	3.3	3.2	3.4	0.8	-0.7	-1.4
IG increases	5.0	4.9	4.9	4.9	0.7	-0.3	-1.0
IG decreases	-1.6	-1.6	-1.7	-1.5	0.8	-0.9	-1.2
IG growth excl welfare	2.2	2.1	2.2	2.0	0.9	-1.2	-1.4
IG incr excl welfare	4.9	4.9	4.9	4.9	1.2	-0.1	-0.7
IG decr excl welfare	-2.8	-2.8	-2.7	-2.8	0.2	-2.1	-1.8
Prior exp growth	2.9	2.5	2.5	2.5	-1.6	-0.5	0.1
Prior IG growth	3.3	3.3	2.3	4.3	-0.9	0.1	0.6
Prior IG growth excl welfare	2.7	3.1	1.5	4.5	-1.5	0.0	0.6
Republican incumbent share:	48.0	42.4	45.9	39.1	-0.1	-1.7	-1.3
Dem share in legislature	55.9	56.6	55.3	57.6	1.4	0.6	-1.4
Observations	1508.0	636.0	298.0	338.0	0.5	.	.

Shares and ratios in percent. All growth rates are real per capita. t-statistics based on standard errors clustered by state and year after removing no fixed effects, or state and year fixed effects, or state and broad census-region×year fixed effects. t statistics are based on regression with governor dummies and linear MOV controls.

and provides additional state characteristics in the bottom part.

Our main takeaway from the table is that the differences between Democratic and Republican governors in the growth rates of expenditures, various revenue components, and IG growth are generally small and insignificant. Expenditure growth averages 2.6% (real, per capita) in our sample, and is only marginally higher under Democrats than Republicans: 2.8% vs. 2.5%. This difference is not statistically significant, with t-statistics of 0.7 without fixed effects or -0.7 and -0.2 with fixed effects and MOV controls. The differences between all other growth rates tend to have small absolute t-statistics. In particular, IG growth excluding welfare averaged 2.1% in 10pp MOV sample, and 2.2% under Democrats vs. 2.0% under Republicans. Depending on fixed effects, the difference has t-statistics of 0.9, -1.2, and -1.4. For IG increases, again excluding welfare, the sample means are 4.9% for both Democrats and Republicans, and the t-statistics range from -0.1 to 1.2.

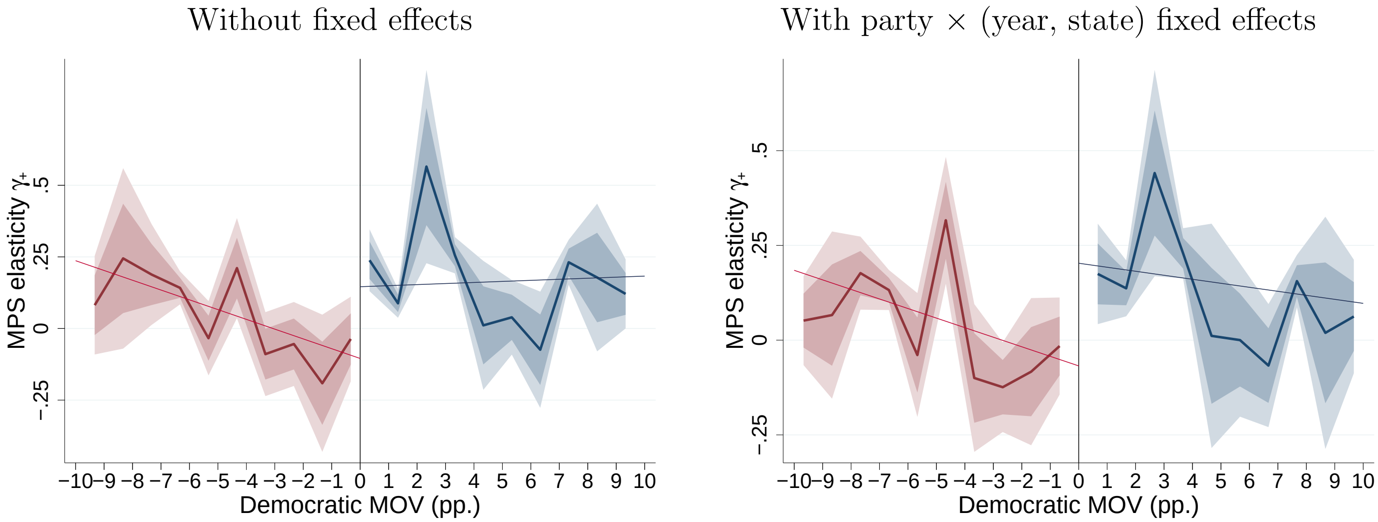
The sample also appears largely balanced for other state characteristics. Consider the 10pp MOV sample. (Our findings are similar when we use the 4pp MOV cutoff.) Without controlling for fixed effects, growth rates in the year prior to the term of the closely elected governor have t-statistics between -1.6 (prior expenditure growth) and 1.4 (Democratic share in the legislature). There are fewer Republican incumbents when Democratic governors are in power, but the t-statistic is only -0.1. Republican governors tend to be in power when the share of Democratic legislatures is slightly higher (57.6% compared to 55.3% under Democrats), and are slightly overrepresented in our sample (156 vs 113 state-years), but the t-statistics are small, at 1.4 and 0.5. Controlling for fixed effects, the t-statistics tend to be smaller in absolute value and without a consistent pattern for the different sets of fixed effects. Nevertheless, we verify the robustness of our results by augmenting our parsimonious regression with interaction terms for various state characteristics.

3 Estimates of state-level partisanship

We now turn to the results of the estimating equation. We begin with the expenditure side of the budget, before turning to tax revenues and debt payments.

3.1 Expenditure side

Graphical analysis. We begin our analysis by showing how the propensity to spend out of transfer increases varies by MOV – see Figure 2. Both panels in the figure report coefficients from estimating γ_+ in equation (2.3) without MOV terms, separately for each one percentage point MOV bin. The left panel is without fixed effects, while the right panel controls for fixed effects. The figure shows a plot of the estimated coefficients along with ± 1 and ± 1.65 standard error bands. Both panels clearly show that the MPS jumps when the MOV turns positive. Focusing on panel (a) first, Republicans have a marginal propensity to spend slightly below zero near the cutoff, whereas Democrats have a marginal propensity to spend near 0.25. Fitting linear regressions to the binned elasticity estimates yields a Republican intercept of -0.11, and a Democratic intercept of 0.15. The 0.26 difference in the MPS elasticity estimates has a causal interpretation, given our identifying assumptions. It implies that if a Democratic governor receives 1pp higher IG growth, her expenditure growth is 0.26pp higher than if she were a Republican governor. Controlling for fixed effects, in panel (b), the picture changes only slightly: The MPS of marginally elected Republicans is -0.06, whereas that of Democrats is 0.20. The difference is, again, 0.26.[5]



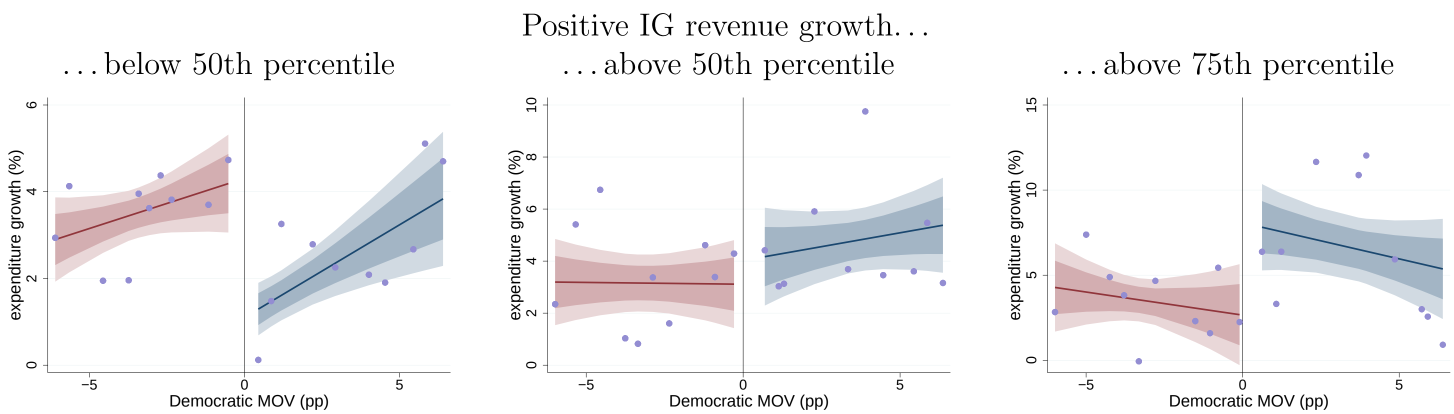
The plots show the estimated marginal propensity to spend (MPS) elasticity along with ±1(±1.65)s.e. clustered by year and state for each 1pp MOV bin. The standard errors are computed pointwise by estimating (2.3) without MOV controls with party×year and party×state fixed effects (or without any fixed effects) and the slope coefficients and intercepts are interacted with dummies for each MOV bin. We drop the term of Bob Wise (D, WV, 2001-2005). Overlaid are linear regressions weighted by the inverse squared s.e.

Figure 2: Illustrating our regression discontinuity in slopes, 1983–2014: Republican governors pass less of IG increases on to spending.

The response of states to transfer cuts also shows partisan differences. Figure C.2 shows the results. We find that the MPS tends to be somewhat higher under Republican governors than under Democrats. At face value, this suggests that Republican governors cut their expenditure more than Democrats would for the same cut of IG transfers. One interpretation of the difference in partisan biases may be that Republicans shy away from creating new programs – and thus have a lower MPS out of IG increases – but cut existing programs more

[5]Note that the +2pp to +3pp bin contains an influential observation: Ann Richards, a Democratic governor of Texas in the early 1990s. We have removed, however, the former Democratic governor of West Virginia, Bob Wise. Without both of them, the elasticity would also be around 0.3 also in the +3pp bin. Adding Bob Wise, the elasticity is +0.94 (no FE) and +0.86 (with FE).

readily than Democrats. The magnitude of the partisan difference is similar to the one we found for IG increases. Since they are more common, we focus our analysis on such increases. But our estimates imply qualitatively similar results (of opposite sign) for transfer cuts.



±1 (±1.65) standard errors, based on coefficient standard errors clustered by year and state. No fixed effects. All observations receive equal weights within the shown MOV range.

Figure 3: Expenditure growth binned RDD plot by IG transfer growth, 1983–2014: Democratic governors raise expenditure as IG transfers rise, while Republican governors do not.

While we interpret our results as implying partisan MPS differences, one can also visualize our results differently using plots more familiar from the RDD literature. For most of our analysis, we assume a continuous, linear relationship between IG growth and expenditure growth for governors of either party – and focus on differences in slopes. Here, we relax that assumption of linearity and consider discrete changes in expenditure growth as a function of partisanship at different levels of IG growth. To do that, we run an RDD, which estimates partisan differences in intercepts, separately for subsamples that condition on high or low IG growth. Figure 3 shows such more traditional plots. We estimated the plots within a MOV of ±6.5pp, based on cross-validation of equation (2.3) without controlling for fixed effects.[6] Comparing the three panels confirms that spending increases relatively more under Democrats when IG growth is higher than under Republicans. Specifically, the panels show that spending under closely elected Republicans is largely invariant to the level of IG growth.

[6]The results that control for fixed effects are similar: See Figure C.3 in the Appendix.

In contrast, spending increases under Democrats with the level of IG growth. Compare the results for the sample with below median but positive IG growth in the left panel with the results for above median IG growth. This comparison shows that Democratic spending at the cutoff rises from an estimated 1.5% to 4%, whereas Republican spending at the cutoff falls from 4% to slightly less than 4% as we condition on above median IG growth. If IG growth is in the top 75th percentile, shown in the right panel, Democratic expenditure growth is around 8% at the cutoff, compared to Republican IG growth below 4%. The fact that Republican spending is, if anything, decreasing in IG growth, while Democratic spending is increasing suggests that the Democratic is, indeed, considerably higher than that of Republicans. This validates our assumption of a difference in slopes, which we quantify in our regression analysis.

Regression estimates. In this subsection we present the main findings for the partisan differences in MPS. Table 2 shows the main regression results. Columns (1) through (4) use the specification with linear MOV controls from (2.3) for 10pp MOV. The columns differ according to which fixed effects are included in the regressions: Party×state and party×year fixed effects are our baseline, column (1). To show that our results are not driven by over-differencing, we also use the coarser state and year fixed effects in column (2), commonly used in the public finance literature (e.g., Besley and Case, 2003). To control for regional economic spillovers, we also report estimates with state and census region×year fixed effects as an additional robustness check in column (3). Column (4) has no fixed effects. To show that our results do not hinge on the linear MOV specification, column (5) shows the results with cubic MOV controls and party specific fixed effects with data on all elections. Columns (6) to (8) show that we obtain the same results in the more parsimonious version of equation (2.3) without MOV controls. The columns differ in the MOV cutoff, which ranges from 5pp to 3pp. Column (9) shows the estimates without MOV controls excluding close elections to show that the results are, indeed, driven by close elections.

Table 2: Estimates of state-level partisan MPS elasticities out of non-welfare IG growth: 1983 to 2014.

	with MOV terms					without MOV terms			
MOV cutoff	(1) ≤10pp	(2) ≤10pp	(3) ≤10pp	(4) ≤10pp	(5) ≤100pp	(6) ≤5pp	(7) ≤4pp	(8) ≤3pp	(9) >5pp
IG incr.	0.181***	0.169**	0.189***	0.200**	0.174**	0.195***	0.194***	0.109	0.096***
	(4.16)	(2.66)	(2.84)	(2.43)	(2.37)	(5.90)	(5.37)	(1.29)	(4.25)
Rep x IG incr.	-0.266***	-0.236**	-0.220*	-0.287**	-0.196**	-0.233***	-0.271***	-0.243**	0.001
	(-3.49)	(-2.45)	(-2.03)	(-2.74)	(-2.27)	(-3.40)	(-3.88)	(-2.38)	(0.03)
IG decr.	-0.018	-0.046	-0.081	0.168**	0.078	-0.020	-0.034	-0.021	0.014
	(-0.26)	(-0.81)	(-1.25)	(2.53)	(1.07)	(-0.27)	(-0.56)	(-0.21)	(0.71)
Rep x IG decr.	0.337***	0.343***	0.313***	0.230***	0.183*	0.264**	0.266***	0.241	0.082***
	(3.33)	(5.53)	(3.58)	(2.82)	(1.71)	(2.69)	(3.58)	(1.39)	(2.84)
Republican Gov.	0.000	0.016*	0.018**	0.024***					
	(0.00)	(1.91)	(2.35)	(4.20)					
Expenditure/IG-rev.	8.90	8.90	8.89	8.90	8.83	9.01	9.04	8.92	8.80
R-squared	0.54	0.46	0.56	0.18	0.42	0.65	0.69	0.71	0.43
R-sq, within	0.10	0.11	0.11	0.18	0.08	0.13	0.12	0.06	0.05
Observations	634	636	634	636	1497	313	259	169	1187
States	47	47	47	47	48	43	41	32	48
Years	32	32	32	32	32	32	32	32	32
State FE	By party	Yes	Yes	No	By party	By party	By party	By party	By party
Year FE	By party	Yes	By region	No	By party	By party	By party	By party	By party
MOV controls	Linear	Linear	Linear	Linear	Cubic	No	No	No	No

Estimated using equation (2.3), with and without MOV controls. t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$. To compute a dollar-to-dollar MPS, multiply the elasticity by the Expenditure/IG revenue ratio.

The first row of Table 2 shows the MPS elasticities for Democratic governors and the second row shows the partisan difference. For IG increases, the estimates of the Democratic baseline MPS elasticity in columns (1) through (5) range from 0.169 to 0.20. The results in row 2 show that the MPS elasticity is estimated to be 0.196 to 0.287 lower under Republican governors than under Democratic governors. These results are close to those found in the graphical analysis. The fixed effects ensure that the differences are not driven by strategic funding of governors of a certain party, common macro policies, or omitted region-specific economic forces.

The estimated partisan differences are economically and statistically significant. The t-statistics are all above two. The economic significance is seen most easily by converting the MPS elasticity into levels. This is accomplished by multiplying the difference in elasticity by the average ratio of expenditures to non-welfare IG, which is 8.89. An elasticity difference of 0.22 corresponds to an MPS that is 1.96 lower (0.22×8.89). While only the partisan difference has a causal interpretation, the estimates are consistent with Republicans having an MPS near zero, since the estimated elasticity difference is in the same order of magnitude as the Democratic baseline MPS elasticity: For example, in column (3), the baseline coefficient is of 0.189 and the Republican difference is -0.22. The full Republican effect is thus of -0.031 – or virtually zero. In the case of transfer cuts, we also estimate large, statistically significant partisan differences, shown in row 4. Republican governors have an MPS elasticity out of transfer cuts that is higher than that of Democrats. The estimated differences in columns (1) through (5) range from 0.183 to 0.343. They imply that Republicans cut expenditures relatively more than Democrats do, following cuts in IG transfers.

The empirical estimates are consistent with the existing literature on the flypaper effect. We can use the estimated elasticities presented in Column 1 of Table 2 to compute an average MPS elasticity. If we assume equal shares of Democratic and Republican governors, the average MPS elasticity is $0.181 - \frac{1}{2}0.266 = 0.048$. This yields an average dollar MPS of 0.43 (0.048×8.9) – well within the 0.25 to 1.04 range of estimates in the survey by Hines and Thaler (1995). The model also fits reasonably well: With fixed effects, the within-model R^2 varies between 0.08 and 0.18, with 0.10 for our baseline specification in column (1).

The variation in the data that drives our results comes, indeed, from closely elected governors. Partisan differences are smaller among governors elected with a wide margin. To show this, columns (6) to (8) show the results for MOV cutoffs below 5pp to 3pp, compared to results based on a MOV above 5pp in column (9). The estimates shown in columns (6) through (9) are based on the version of equation (2.3) that omits MOV terms from the regressions. Considering close elections, the differences in estimated MPS elasticity range between -0.233 and -0.271 and are similar to the elasticities estimated when MOV terms are included in the regressions and we use a higher cutoff. However, as column (9) shows, the differences in MPS elasticities between Republican and Democratic governors disappear when we exclude close elections characterized as a MOV greater than 5pp.

The same factors that explain partisan differences in the first place may also explain the smaller partisan differences outside close elections. Downs (1957) model of politics would suggest no partisan differences at all, and perhaps at least smaller partisan differences among closely elected governors. However, consider the case of candidates running for the party nomination as "citizen-candidates," as specified by Besley and Coate (1997). The primary voters may be selected such that they nominate conservative Democrats into conservative leaning states. Unless offset by an equal shift in the Republican party, this would explain the weaker partisan difference outside close elections. Another explanation lies in the incentives for governors as officeholders. In competitive states, a governor may maximize chances of reelection or future bids for office by scoring points with their ideological base. Otherwise rent-seeking politicians may increase their utility of holding office by implementing more centrist policies. Here, this would mean using IG transfers in a less partisan way. This would be rational, for example, if this increased their chances of reelection or of running for other offices such as the U.S. Senate or the presidency.

One possible issue with transforming our MPS elasticities to a dollar-per-dollar MPS is that the ratio of total state expenditures to non-welfare IG received by a state varies widely across states. Among the 47 states in our baseline sample in columns (1) to (4) of Table 2, the 25th percentile is 7.5 while the 75th percentile is 10.2. To provide direct dollar estimates that still take this heterogeneity into account, we transform expenditure growth prior to the estimation by multiplying it with the state-specific expenditure to non-Welfare IG ratio. We use a five year lag to ensure that the ratio is predetermined. This gives us a weighted dollar MPS estimate, which may be a more representative average effect for our sample. This then allows us to compare the dollar-per-dollar MPS out of IG transfers exclusive of welfare with that for total IG transfers.

When using scaled state-level total expenditure growth in the analysis, we find that dollar MPS are similar to those based on converted elasticities, but slightly smaller. Table 3 reports the estimated dollar-for-dollar MPS. Columns (1) through (5) are the expenditure-weighted counterparts to the same columns in Table 2. In Column (6) we report the results for IG excluding welfare transfers. While the first six columns are for IG excluding welfare,

Table 3: Dollar-for-dollar MPS estimates for non-welfare IG and total IG: 1983 to 2014.

	IG excluding transfer for welfare						Total IG transfers		
	With MOV controls					No MOV	With MOV controls		No MOV
	(1) ≤10pp	(2) ≤10pp	(3) ≤10pp	(4) ≤10pp	(5) ≤100pp	(6) ≤4pp	(7) ≤10pp	(8) ≤100pp	(9) ≤4pp
IG incr.	1.351***	1.260*	1.411**	1.469*	1.354*	1.678***	1.068**	1.095***	1.285***
	(2.84)	(2.00)	(2.17)	(1.82)	(1.96)	(5.19)	(2.47)	(3.03)	(6.06)
Rep x IG incr.	-1.919**	-1.661*	-1.510	-1.905*	-1.318	-2.142***	0.220	0.523**	0.262
	(-2.47)	(-1.73)	(-1.47)	(-1.84)	(-1.59)	(-3.49)	(0.72)	(2.35)	(0.41)
IG decr.	0.271	-0.322	-0.641	1.595**	0.694	-0.047	-0.697	-0.850*	-1.446***
	(0.36)	(-0.51)	(-0.92)	(2.42)	(0.91)	(-0.09)	(-1.18)	(-1.77)	(-2.90)
Rep x IG decr.	2.996**	3.541***	3.020***	2.202**	2.228*	2.186***	2.480***	1.845**	2.310***
	(2.38)	(3.95)	(3.25)	(2.08)	(1.97)	(3.45)	(3.09)	(2.64)	(2.87)
Republican Gov.	0.000	0.167**	0.173**	0.213***			0.000		
	(0.00)	(2.19)	(2.37)	(3.61)			(0.00)		
R-squared	0.53	0.46	0.56	0.19	0.42	0.67	0.55	0.47	0.71
R-sq, within	0.10	0.11	0.11	0.19	0.09	0.12	0.17	0.17	0.19
Observations	634	636	634	636	1497	259	634	1497	259
States	47	47	47	47	48	41	47	48	41
Years	32	32	32	32	32	32	32	32	32
State FE	By party	Yes	Yes	No	By party	By party	By party	By party	By party
Year FE	By party	Yes	By region	No	By party	By party	By party	By party	By party
MOV controls	Linear	Linear	Linear	Linear	Cubic	No	Linear	Cubic	No

Estimated using equation (2.3), with and without MOV controls. t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

columns (7) to (9) report estimates for overall IG for comparison. For non-welfare IG, the dollar partisan differences are large and robust across the first six specifications reported in Table 3: Per dollar received, they range from \$1.32 to \$2.14 following IG increases. Compared to the MPS elasticity estimates, the level MPS estimates are less precise than the elasticities estimates and sometimes are marginally insignificant.

While we focus our attention on the partisan difference, it is worth noting that the level estimates are reasonable in light of the literature. Consider column (1) of Table 3. First, note that the effect sizes are slightly smaller than the converted elasticity based on the same column in Table 2: As the third row of column (1) of Table 3 shows, the partisan difference is -1.92, whereas the converted elasticity difference shown in Table 2 is -2.37 (-0.266×8.9). Second, assuming equal shares of Republican and Democratic governors yields an average MPS of 0.39 ($1.351 - \frac{1}{2}1.919$). This level MPS estimate is slightly below the 0.43 estimate based on elasticities and also lines up with the literature (Hines and Thaler, 1995).

The pattern for overall IG and non-welfare IG is consistent with our argument about identification that matching grants, which dominate welfare IG transfers, bias partisan difference downward. For the same specification and sample period, we find that the partisan difference is lower with overall IG transfers than with non-welfare IG transfers: In the baseline specification shown in Table 3, this difference drops from -1.919 (column (1) for non-welfare IG) to -0.697 (column (7) total IG). In the full sample with a third order MOV polynomial, the partisan difference drops from -1.318 (column (5)) to -0.850 (column (8)). As column (7) shows, without MOV controls, the partisan difference also drops in the 4pp MOV sample. This later finding is consistent with our identifying argument for total IG transfers, the partisan difference is biased down because of matching of spending with IG revenue. Matching of federal funds with state funds is important for welfare IG – with recent Medicaid matching rates between 50% and 71%, and even higher matching rates for CHIP payments. Matching plays little role aside from welfare transfers. If there is any matching in the remaining IG transfers, our simple model suggests it should bias the estimated partisan differences downward. Our estimates would then be a lower bound.

Polarization and time-variation. A large literature (e.g., Azzimonti (2018) and McCarty et al. (2016)) has documented an increase in political polarization in the U.S. This motivates the question whether the partisan differences that we estimate are a fixture of the political landscape, or have changed over time. To address this question, we extend our sample period back to 1968. Previewing our results, we find that partisan differences at the state level have risen along with the polarization of federal policymakers.

In addition to extending our sample period, we need a richer regression specification to capture variation in partisanship. Specifically, we connect the measured polarization at the federal level to partisan MPS differences for state governors, using the following specification:

$$\begin{aligned}
\Delta E_{s,t} =& (\gamma_{0,+} + \gamma_{0,+,p} P_{t-1} + (\gamma_{r,+} + \gamma_{r,+,p} P_{t-1}) \times Rep_{s,t-1}) \Delta \ln IG^{+}_{s,t} \\
&+ (\gamma_{0,-} + \gamma_{0,-,p} P_{t-1} + (\gamma_{r,-} + \gamma_{r,-,p} P_{t-1}) \times Rep_{s,t-1}) \Delta \ln IG^{-}_{s,t} \\
&+ \sum_{\mathfrak{s} \in \{-,+\}} (\gamma_{0,\mathfrak{s},m} + \gamma_{0,\mathfrak{s},m,p} P_{t-1} + (\gamma_{r,\mathfrak{s},m} + \gamma_{r,\mathfrak{s},m,p} P_{t-1}) \times Rep_{s,t-1}) \Delta \ln IG^{\mathfrak{s}}_{s,t} \times f(MOV_{s,t-1}) \\
&+ (\beta_{0,m} + \beta_{0,m,p} P_{t-1} + (\beta_{r,m} + \beta_{r,m,p} P_{t-1}) \times Rep_{s,t-1}) f(MOV_{s,t-1})
\end{aligned}$$

$$+ (\mu_0 + (\mu_r + \beta_{r,p} P_{t-1}) \times Rep_{s,t-1}) + \text{fixed effects} + \epsilon_{s,t}, \tag{3.1}$$

where P_{t-1} is one of three national indicators of polarization. Since the polarization measure is national, its direct effect is absorbed in the year fixed effects. $f(MOV_{s,t-1})$ is a polynomial in terms of the MOV (without intercept). In the linear case, it is just the MOV itself. While we include most terms in this equation to guard against misspecification, our focus is on $\gamma_{r,+,p}$, which captures how the partisan MPS difference has changed with polarization. Since we standardize the measured polarization, the coefficient corresponds to the effect of a one standard deviation increase in polarization on the partisan difference. Our three measures of partisanship are: The mean political difference in the House, and the mean difference in the Senate, both taken from McCarty et al. (2016), and the news-based historical partisan conflict index from Azzimonti (2018), smoothed by averaging two years. We use expenditure growth scaled with the ratio of expenditures to non-welfare IG to obtain level MPS estimates.

Table 4 shows the estimates of equation (3.1) for the extended sample period, from 1968 to 2014. In the regressions, we always use party×year and party×state fixed effects, for different cutoffs and with and without MOV controls. We use scaled expenditure growth so that the coefficients are expressed in terms of the MPS in levels. Since below, in our national model in section 4, we are interested in how partisan differences have changed for all governors, and not just those elected marginally, columns (1) through (4) of the table are based on all observations. The columns differ in the interaction terms and MOV controls. Column (5) uses the 10pp MOV cutoff with linear MOV controls, and columns (6) through (8) use the 4pp MOV cutoff without MOV controls. Last, columns (1) through (6) use the polarization indicator from Azzimonti (2018), while columns (7) and (8) use the measures for the House and Senate, respectively.

We find that partisan differences among state governors have risen along with increased polarization. Column (1) of Table 4 shows the full-sample regression without any controls for close elections and without polarization interactions. Unsurprisingly, the partisan difference

Table 4: Dollar-for-dollar MPS estimates and political polarization: Various MOV cutoffs, 1968 to 2014.

	Polarization control							
	None	News-based historical partisan conflict					House	Senate
MOV cutoff	(1) 100pp	(2) 100pp	(3) 100pp	(4) 100pp	(5) 10pp	(6) 4pp	(7) 4pp	(8) 4pp
Pos IG growth	0.965***	1.006***	1.268***	1.449***	1.344***	1.319***	1.228***	1.351***
	(5.44)	(5.82)	(5.38)	(3.27)	(2.92)	(2.96)	(3.03)	(3.32)
Rep gov x Pos IG growth	-0.269	-0.283	-0.527	-1.208*	-1.305*	-1.342**	-1.329**	-1.529**
	(-1.67)	(-1.65)	(-1.57)	(-1.84)	(-1.93)	(-2.19)	(-2.19)	(-2.52)
Control x Pos IG growth		0.419*	0.681**	1.325***	1.295***	1.217***	0.440	0.768***
		(1.86)	(2.24)	(3.30)	(4.12)	(5.17)	(1.23)	(3.05)
Rep gov x Control x Pos IG growth		-0.703**	-1.104**	-2.038***	-1.996***	-1.998***	-1.017*	-1.346***
		(-2.24)	(-2.43)	(-3.14)	(-3.30)	(-5.54)	(-1.95)	(-3.47)
Neg IG growth	0.224*	0.216*	0.195	0.071	-0.030	0.006	-0.108	-0.056
	(1.83)	(1.68)	(1.24)	(0.14)	(-0.06)	(0.01)	(-0.15)	(-0.08)
Rep gov x Neg IG growth	0.801***	0.748***	0.810***	1.486**	1.460**	0.737	1.119	1.101
	(4.67)	(3.88)	(3.76)	(2.53)	(2.02)	(0.94)	(1.41)	(1.40)
Control x Neg IG growth		0.000	-0.093	-0.097	-0.182	-0.478	-0.383	-0.395
		(0.00)	(-0.43)	(-0.20)	(-0.47)	(-0.95)	(-0.65)	(-0.71)
Rep gov x Control x Neg IG growth		0.295*	0.580*	1.090	1.043	1.450**	1.302*	1.192
		(1.85)	(1.95)	(1.37)	(1.59)	(2.12)	(1.76)	(1.65)
R-squared	0.43	0.43	0.43	0.44	0.52	0.65	0.64	0.64
R-sq, within	0.06	0.06	0.07	0.08	0.10	0.11	0.08	0.08
Observations	2226	2226	2226	2226	961	390	390	390
States	50	50	50	50	50	47	47	47
Years	47	47	47	47	47	47	47	47
MOV controls	No	No	Linear	Cubic	Linear	No	No	No

Estimated using equation 2.3 in column (1), and equation (3.1) otherwise. Columns (1), (2) and (6) through (8) exclude MOV controls. Party by year and party by state fixed effects. The LHS is scaled with the lagged non-welfare IG to expenditure ratio in each state to yield dollar estimates. t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

is only 0.269 lower, about one eighth of our Reagan-era estimate that controls for close elections. If we add the smoothed and average measure of partisanship based on Azzimonti (2018), however, we find economically and statistically significant interaction term (rows 3 and 4 of column (2)): A one standard deviation increase in polarization is associated with an increased partisan difference of \$0.703 per dollar received. Since polarization has been 1.42 standard deviations higher in the 2000s than in the five years preceding the Reagan era, 1978 to 1982, this corresponds to an increase in partisan differences between MPS of about 1.0 dollars per dollar received (0.702×1.42).[7]

[7] At the same time, the baseline (Democratic) MPS, which is identified only under stronger assumptions, increased by only 0.4 times the increase in partisan differences. Since the partisan difference is almost twice as high, this result implies that the MPS varies only little with polarization, when averaged across Democrats and Republicans and they govern similar numbers of states.

Controlling for close elections points to a larger effect of polarization: In column (3), where we control for the linear effects of the MOV and its interactions in the full sample, the partisan difference rises in magnitude from -0.703 to -1.104. Controlling for cubic MOV terms brings the effect of a one standard deviation increase in polarization on partisan MPS difference to -2.038 (column (4)). As shown in column (5), this partisan difference is very similar in the 10pp MOV sample with only linear MOV terms and interactions, and also in the parsimonious model without MOV terms but with a 4pp MOV cutoff (columns (5) and (6)). Using polarization measures for the U.S. House or U.S. Senate also implies that partisan differences have risen significantly with polarization (columns (7) and (8)).

A non-Downsian model of politics may explain why partisan differences have increased with polarization. Wittman (1983) and Harrington (1992) show that what pulls party-voters away from the preferences are party voters' preferences for non-income policies. The further apart party voters' preferences for non-income policies are, the greater will be the preference separation of the candidates for governor and thus the larger the partisan differences between the preferences of elected governors.

Other sources of exogenous variation. We can use the exogenous variation in severance taxes as a way to further confirm our finding of state-level partisan differences in fiscal policy responses to IG transfers. Typically, severance taxes are imposed on the extraction of natural resources within a specific taxing jurisdiction. These taxes are often imposed by oil producing states, such as Oklahoma and Wyoming. But, severance taxes are often imposed on the extraction other natural resources, such as gas, coal, and timber. Severance tax revenue is largely driven by changing prices of natural resources, particularly oil prices, and may therefore be considered another source of exogenous variation. Specifically, we consider whether there are partisan differences in the MPS out of severance tax revenue increases. In this analysis, we focus on 21 states[8] with severance tax revenue accounting for at least 1%

[8] States ever in the sample are: AL, AK, AR, CO, FL, KS, KY, LA, MN, MS, MT, NM, ND, NV, OK, OR, SD, TX, UT, WV, and WY.

of overall revenue five years ago and use data on all elections for the period 1983 - 2014.[9]

Similar to our finding that Republican governors spend less of IG transfers, we also find that Republican governors spend less of the increase in severance tax revenue compared to Democratic governors. For brevity, we focus here on the results for regressing expenditure growth on state fixed effects, denoted $\hat{\mu}_s$, and severance tax revenue growth with partisan interactions. This yields the following estimate:

$$\Delta \ln E_{s,t} = \hat{\mu}_s - \underset{[0.01]}{0.01} Rep_{s,t-1} + \underset{[0.14]}{0.633} \Delta \text{SevTax}_{s,t-1} - \underset{[0.08]}{0.252} \times Rep_{s,t-1} \Delta \text{SevTax}_{s,t-1}. \quad (3.2)$$

When severance tax revenue increases by 1pp of overall revenue, expenditure growth is - 0.252pp lower under a Republican governor than under a Democratic governor. Table C.1 in the Appendix also includes additional specifications.

Extensions We also analyze the expenditure by end use, for those categories that we could match across the two vintages of the government budget data, comprising about two thirds of total expenditure. The results suggest that the different propensities to spend apply to a large range of end uses, with the exception of infrastructure spending. Specifically, we find marginal propensities to spend that are significantly lower for Republicans than Democrats in Education (accounting for 32.7% of expenditures), Natural Resource Management (2.4%), and Housing and Community Development (0.7%). Republicans also spend significantly less for unclassified expenditures, which account for 34.0% of the total. See Table C.3. In contrast, we find only insignificant different for public welfare, highway spending, and other, smaller categories. While this might be expected for welfare spending, given that we exclude welfare IG, it may indicate that partisan differences are less pronounced for some uses.[10]

[9] In the appendix, we also analyze expenditure growth stimulus in the aftermath of the recession (2009-2012) relative to the preceding 4-year period. The regression also shows the same qualitative partisan difference (Table C.2). Other papers in the literature (see Chodorow-Reich, 2019) use IV strategies to analyze state spending and transfers without regard to partisanship. However, we find that the instruments are weak once we split the sample by party.

[10] In several extensions, we also investigate the role of institutional details, timing, politics, and the interaction with economic factors. See Tables C.4 through C.9 in the Appendix.

3.2 Revenue and debt

Since Republican governors have a lower MPS out of federal transfers, some other components of their state budgets need to adjust. While the Census Bureau cautions that the budget identity does not hold exactly in the data set used in the analysis, we provide evidence of relatively lower taxes under Republican governors than under Democratic governors during periods of IG inflows. For debt, which is not well-measured in our data, we also find results that suggesting some of the IG funding may be used to reduce either debt levels or interest rates. However, the estimates are noisier than our results for expenditures. This may be because, unlike expenditures, neither tax revenue nor our preferred measure of debt (interest payments on debt) are direct choice variables for the state government. We thus also consider data on changes in statutory tax rates, which supports the analysis in our baseline results.

Table 5: Partisan determinants of tax revenue growth and changes in future interest on debt by state governments: 1983 to 2014. Dollar-for-dollar estimates

	Income and sales tax revenue				Future interest payments			
	With MOV controls			No MOV	With MOV controls			No MOV
	(1) ≤10pp	(2) ≤10pp	(3) ≤100pp	(4) ≤4pp	(5) ≤10pp	(6) ≤10pp	(7) ≤100pp	(8) ≤4pp
IG incr.	0.147	0.058	0.427	0.518***	55.987	70.475*	-12.799	99.186**
	(0.39)	(0.13)	(1.04)	(3.01)	(1.18)	(1.84)	(-0.41)	(2.56)
Rep x IG incr.	-0.716	-0.325	-0.923*	-0.880***	-63.503	-120.526**	-14.332	-141.322**
	(-1.45)	(-0.60)	(-1.77)	(-3.07)	(-1.02)	(-2.30)	(-0.27)	(-2.10)
IG decr.	-1.061***	-1.035*	-1.121**	-0.363	-10.725	51.839	64.894	-112.492**
	(-2.93)	(-1.84)	(-2.45)	(-0.81)	(-0.13)	(0.75)	(1.52)	(-2.16)
Rep x IG decr.	1.645***	1.517**	1.603***	0.794	53.893	33.245	-41.378	169.804**
	(3.24)	(2.16)	(2.92)	(1.34)	(0.56)	(0.39)	(-0.62)	(2.21)
Republican Gov.	0.000	0.053			0.000	11.414**		
	(0.00)	(0.79)			(0.00)	(2.20)		
R-squared	0.56	0.56	0.43	0.70	0.31	0.32	0.18	0.54
R-sq, within	0.03	0.02	0.02	0.04	0.02	0.02	0.01	0.06
Observations	634	634	1497	259	634	634	1497	259
States	47	47	48	41	47	47	48	41
Years	32	32	32	32	32	32	32	32
State FE	By party	Yes	By party	By party	By party	Yes	By party	By party
Year FE	By party	By region	By party	By party	By party	By region	By party	By party
MOV controls	Linear	Linear	Cubic	No	Linear	Linear	Cubic	No

Estimated using equation (2.3), with and without MOV controls. t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

We begin by analyzing the partisan determinants of tax revenue growth. Overall, we find evidence of relatively lower taxes under Republican governors following IG increases. The first three columns in Table 5 show the regression results for income and sales tax revenue growth. We also provide estimates of the dollar coefficients directly, by scaling the LHS variable with the 5-year lag of the tax revenue divided by non-Welfare IG revenue. Our baseline estimate, shown in column (1), implies that for each dollar increase in intergovernmental transfers, the tax revenue falls by 72 cents under a Republican governor, compared to a Democratic governor. The point estimates are fairly robust across specifications, ranging between -0.716 and -0.880, depending on the specification. But, the results are statistically significant (and marginally so) only in the full sample with cubic MOV controls and in the 4pp MOV sample without MOV.

For plausible magnitudes of fiscal multipliers, these results indicate reductions in effective tax rates under Republican governors compared to Democratic governors. While small relative declines in Republican tax revenues could be the result of their relatively lower spending in the presence of fiscal multipliers, this is implausible for reasonable parameter values. For example, according to column (1) in Table 3, Republican governors spend \$1.92 less per dollar received, and according to column (2) in Table 5, this is associated with \$0.72 less in revenue. With a unit multiplier and a combined state tax rate of 10% on GDP, only \$0.19 of tax revenue could be explained by such effects. Thus, our results point to lower effective tax rates to explain at least part of the lower Republican tax revenue.

To get directly at a policy instrument chosen by state, we now turn to statutory tax rates. Specifically, we use maximum marginal income tax rates from the NBER TAXSIM database. Table 6 shows the corresponding regression results. In levels, shown in the first three columns, the evidence is suggestive only: The results are only significant for the specifications in columns (2) and (3). The results with MOV controls point towards a reduction in tax rates, but as column (1) shows, the results are insignificant in our baseline specification. However, we observed only a few changes in the statutory level of tax rates during our sample period. We thus analyze only changes in the marginal tax rates. To deal with the sparser data in this analysis, we use a model with fewer fixed effects to avoid overfitting. The results, shown in

Table 6: Partisan determinants of statutory income tax rates: 1983 to 2014.

	Current state marginal tax rate				Changes in marginal tax rate		
	With MOV controls			No MOV	With MOV controls		No MOV
	(1) ≤10pp	(2) ≤10pp	(3) ≤100pp	(4) ≤4pp	(5) ≤6.5pp	(6) ≤100pp	(7) ≤4pp
IG incr.	-1.470	0.187	1.003	0.031	3.472**	5.079***	1.413
	(-0.96)	(0.19)	(1.18)	(0.05)	(2.27)	(3.39)	(1.20)
Rep x IG incr.	-0.860	-4.237*	-3.421*	0.586	-5.779*	-6.387**	-2.517
	(-0.34)	(-1.82)	(-1.86)	(0.36)	(-1.71)	(-2.24)	(-1.41)
IG decr.	-1.631	-4.119	-3.639	-1.316	-7.431**	-3.246	-6.345**
	(-0.51)	(-1.60)	(-1.30)	(-0.64)	(-2.72)	(-0.77)	(-2.28)
Rep x IG decr.	0.514	3.346	4.132	-1.147	9.009**	5.448	5.120
	(0.10)	(1.02)	(1.06)	(-0.24)	(2.14)	(0.84)	(1.57)
Republican Gov.	0.000	-0.818**			0.439	0.000	0.276
	(0.00)	(-2.17)			(0.98)	(0.00)	(0.99)
R-squared	0.92	0.93	0.91	0.96	0.06	0.35	0.06
R-sq, within	0.03	0.07	0.03	0.01	0.06	0.15	0.06
Observations	634	634	1497	259	139	414	88
States	47	47	48	41	36	41	31
Years	32	32	32	32	32	32	32
State FE	By party	Yes	By party	By party	No	By party	No
Year FE	By party	By region	By party	By party	No	No	No
MOV controls	Linear	Linear	Cubic	No	Linear	Cubic	No

Estimated using equation (2.3), with and without MOV controls. t-statistics based on standard errors clustered by state and year. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < 0.1$, **: $p < 0.05$, *: $p < 0.01$.

columns (5) through (7), suggest that Republican governors have smaller increases (or larger decreases) in statutory tax rates than Democratic governors. In the regression summarized in column (5), we include linear MOV controls, but no fixed effects – and thus use the intermediate MOV cutoff of 6.5pp, which we calibrated for the expenditure growth model without fixed effects. In particular, the estimate with MOV controls shown in column (5) suggests that a 1pp increase in IG growth is associated with a 0.06pp cut in states' maximum marginal income tax rate. The effects in the full sample with party by state fixed effects and cubic MOV controls shown in column (6) are very similar. In the most parsimonious specification in column (7), we find effects with a t-statistic of only 1.4. The effect still indicates that a 1pp increase in IG growth is associated with a 0.03pp cut in tax rates.

Finally, we turn to possible debt changes, where we find that Republicans improve the debt position of their state compared to Democrats. Our preferred measure of debt is states' interest payments on debt, since all other debt measures in the Census data are at face value, rather than at market value. Columns (5) through (8) in Table 5 show the results for debt payments in the next fiscal year, in real per capita terms. Only two specifications yield statistically significant results. Our parsimonious specification in column (8) implies that a one percent increase in IG revenue is associated with a reduction of debt payments by $1.41 per capita. This reduction could come from reducing outstanding debt, or rolling it over at lower interest rates.

4 Partisan states in a macroeconomic model

To assess the aggregate effects of the partisan policy rules, we build a macroeconomic business cycle model that features two representative states in a monetary union, endowed with the estimated preferences of Democratic governors and Republican governors, respectively. We use the model to evaluate the effects of a fiscal stimulus through IG transfers, as a function of the partisan difference in the MPS.

The economy is a New Keynesian model of states (regions) within a monetary union, similar to Nakamura and Steinsson (2014) and Auclert et al. (2019).[11] Its New Keynesian nature gives a role to both demand-side and supply-side policies: Firms set prices in monopolistic competition subject to nominal rigidities, and a subset of households lives hand-to-mouth. These features give rise to an aggregate demand channel for policy. Capital accumulation, endogenous labor supply, and distortionary taxes, however, imply a potentially important role for supply-side policies. We discipline the relative strength of these channels by calibrating the model to match federal government consumption multiplier estimated in Ramey (2011). We calibrate our fiscal experiment to the IG portion of the 2009 U.S. stimulus bill.

[11]Similar to Auclert et al. (2019), our model has two regions, each with two types of households who consume two different types of goods, but with added fiscal detail to make the model suitable for the question at hand. Given our focus, we abstract from an explicit model of borrowing constraints and tradable vs nontradable sectors. Compared to Nakamura and Steinsson (2014), our model adds constrained households, as well as state governments, intergovernmental transfers, and a role for productive government spending.

4.1 Environment

There are two states, inhabited by a unit measure of households and intermediate firms. The home state is of size n, while the foreign state is of size $1-n$. The states trade with each other, but households and capital are immobile across states. Each state has its own government, and there is a federal fiscal authority as well as a monetary authority. Except for policy-makers' preferences and possibly their size, the home (HS) and foreign (F) states are symmetric. We thus focus our discussion on the home state. As needed, we denote variables pertaining to the foreign state using an asterisk. Appendix D provides a full set of derivations and model equations.

Households The unit measure of households in each state is divided into constrained and unconstrained households. Unconstrained households have access to complete markets and accumulate private capital and government debt. A fraction $1-\mu$ of households is barred from borrowing and saving and consumes their income every period. Households have identical utility over consumption, leisure, and state government services of the following form:

$$\tilde{u}(C_t, N_t) = \ln C_t - \kappa_N^i \frac{N_t^{1+1/\epsilon_N}}{1+1/\epsilon_N},$$

where C is an aggregate consumption good, N is labor supply, and ϵ_N is the Frisch elasticity of labor supply, which is common across households. κ_N^i governs the preference for leisure, and we allow it to differ by type of household ($i \in \{c, u\}$ for constrained and unconstrained).

Households pay proportional federal and state labor income taxes τ_t^f and τ_t^{st} on their labor income and receive lump-sum transfers and profit income. Only unconstrained households can hold nominal bonds B_t or capital K_t. Households can adjust capital services by varying the rate of utilization u_t, which incurs a resource cost. This yields the following budget constraint for unconstrained agents:

$$P_t(C_t^u + I_t^u + \kappa(u_t)K_{t-1}^u) + K_{t-1}^u\delta + B_t^u \leq (1-\tau_t)W_t N_t^u + r_t^k u_t K_{t-1}^u + B_{t-1}^u R_{t-1}^n + Tr_t + Pr_t$$

Unconstrained agents can also trade a complete set of Arrow-Debreu securities, which are in zero net supply and omitted for simplicity. The budget constraint is similar for constrained households, but with $B_t^i = K_t^i = 0$ and without Arrow-Debreu securities.

Capital accumulation is subject to quadratic adjustment costs in the rate of investment. Capital depreciates at rate δ, and more quickly, when utilization u_t is higher.

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

Households consumption and investment demand is characterized by nested CES preferences over varieties produced at home and abroad. These preferences attach a weight ϕ_H $(1 - \phi_H \equiv \phi_F)$ to home (foreign) goods, and an elasticity of substitution η between home and foreign goods and an elasticity of substitution of θ between different varieties within a state. Consequently, demand for bundles of home and foreign goods is given:

$$C_t^S + I_t^S = \phi_S(C_t + I_t)\left(\frac{P_{S,t}}{P_t}\right)^{-\eta}, \quad S \in \{H, F\}.$$

Here, $P_{S,t}$ is the optimal price index for the bundle purchased from state S, $P_{S,t} = (\int p_{S,t}(i)^{1-\theta} di)^{\frac{1}{1-\theta}}$. P_t is the aggregate price index, $P_t = (\phi_H P_H^{1-\eta} + (1-\phi_H)P_F^{1-\eta})^{\frac{1}{1-\eta}}$. Demand for varieties within each bundle has the same structure, but with elasticity θ over the relative price $\frac{p_{S,t}(i)}{P_{S,t}}$.

Firms. Each region has a unit measure of intermediate goods producers. They produce their variety z using a Cobb-Douglas aggregate of utilization-adjusted capital and labor:

$$y_{h,t}(z) = \bar{A}_t(u_t K_{t-1})^{\alpha} N_t(z)^{1-\alpha}.$$

While firms perceive cost shares of capital and labor of α and $1-\alpha$, respectively, $\bar{A}_t$ depends on public infrastructure that is subject to a congestion externality as in Barro and Sala-I-Martin (1992) and Drautzburg and Uhlig (2015), so that the equilibrium shares of public infrastructure, private capital, and labor are $\zeta, (1-\zeta)\alpha$ and $(1-\zeta)(1-\alpha)$. Firms face iso-elastic demand with elasticity θ and set price in monopolistic competition subject to a Calvo-friction. With probability ξ, the firm cannot reoptimize in a given quarter and its prices rise at the rate of trend inflation $\bar{\Pi}$. Absent these frictions, firms would set a constant markup $\frac{\theta}{\theta-1}$ over marginal cost.

State governments States adjust transfers, government consumption and investment, and labor income tax rates in response to changes in IG transfers. The home and foreign state governments are symmetric, except for the propensity to spend IG transfers. In the home state, the MPS is ψ_{IG}, while it is ψ^*_{IG} in the foreign state.

Since transfer payments are important in state budgets, we assume that states spend a fraction ϕ_{tr} on transfers $tr_{st,t}$. The remainder is spent on government consumption and investment $G_{st,t}$, of which a fraction $1-\phi_K$ goes towards public services.

$$tr_{st,t} = \psi_{IG}\phi_{tr}\left(\frac{IG_t}{P_t} - \bar{I}G\right) + \overline{tr}_{st}$$
$$G_{st,t} = \psi_{IG}(1-\phi_{tr})\left(\frac{IG_t}{P_t} - \bar{I}G\right) + G^x_{st,t}$$
$$G^x_{st,t} = (1-\rho_{st,g})\bar{G}^{st} + \rho_{st,g}G^x_{st,t-1} + \omega_{st,g}\epsilon^x_{st,t}$$

States invest the remainder of $G_{st,t}$ in infrastructure:

$$K_{st,t} = (1-\delta_G)K_{st,t-1} + \phi G_{st,t}. \tag{4.1}$$

State purchases $G_{st,t}$ are the same CES aggregate of home and foreign bundles as private consumption and investment.

To guarantee stable debt, we assume that states adjust distortionary taxes. As we discuss below, there is reduced form evidence that states smooth tax rates, and gradually adjust labor income tax rates in response to their debt burden and level of net expenditure. Our baseline tax rule therefore takes the following form:

$$\tau_{st,t} = \rho_\tau \tau_{st,t-1} + (1-\rho_\tau)(\bar{\tau}_{st} + \psi_{st,b}(R^n_{t-1}-1)B^{st}_{t-1} - (\bar{R}^n - 1)\frac{\bar{b}^{st}}{\bar{\Pi}}P_t)$$
$$+ \psi_{st,E}(P_t G^{st}_t - P_t \bar{G}^{st}_t + P_t(tr_{st,t} - \overline{tr}_{st}) - (IG_t - P_t \bar{I}G))) \tag{4.2}$$

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 are equal to IG_t in each region and follow an exogenous AR(1) process with persistence ρ_{IG}. Federal labor income taxes finance $1-\gamma^f$ of government consumption and IG transfers every period (out of steady state), and the government issues constant lump-sum transfers (or taxes). Out of steady state, the federal government finances the remaining fraction γ^f of expenditures via nominal debt issuance.

Monetary authority. The monetary authority reacts to aggregate inflation and output when setting interest rates. Specifically, it follows a standard Taylor rule, as in Galí (2008):

$$R^n_t = (\bar{\Pi}/\beta)^{\rho_r}\left(\left(\frac{\bar{\Pi}_t}{\bar{\Pi}}\right)^{\psi_{r\pi}}\left(\frac{\bar{Y}_t}{\bar{Y}}\right)^{\psi_{ry}}\right)^{1-\rho_r}, \tag{4.3}$$

where aggregate inflation and output are simply weighted measures of regional consumer price inflation and output ($\bar{\Pi}_t \equiv n\Pi_t + (1-n)\Pi^*_t$ and $\bar{Y}_t \equiv nY_t + (1-n)Y^*_t$).

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).

4.2 Calibration

Since our goal is to evaluate the effectiveness of fiscal policies, we calibrate our model to match estimates of aggregate (defense) spending multipliers, which we take to be 0.8 for surprise spending increases, following Ramey (2011). Otherwise, parameters are similar to the calibrated values in the closely related currency union models of Nakamura and Steinsson (2014) and Auclert et al. (2019), as well as in the estimates of Leeper et al. (2017).

Type distribution, preferences, and technology. To match the defense spending multiplier, our model requires strong Keynesian features. We thus calibrate a high degree of nominal rigidities and a large fraction of high MPC agents, similar to Auclert et al. (2019). Specifically, we pick a persistence of nominal prices of $\xi = 0.85$ and choose a fraction of constrained agents of $1 - \mu = 0.4$. Auclert et al. (2019) choose $\xi = 0.8$ and calibrate $\mu = 0.5$ to match the fraction of the population with credit card debt. Our share of 40% constrained agents is higher than the modal share across seven DSGE models in Coenen et al. (2012), but lower than the 47% share for the SIGMA U.S. model. Constrained agents account for about 35% of aggregate consumption in steady state under our baseline assumption that they also receive an equal share of firm profits.

Our unitary intertemporal elasticity of substitution ε_c corresponds to the calibrated value in Leeper et al. (2017), Nakamura and Steinsson (2014) and Auclert et al. (2019). We set the Frisch elasticity of labor supply to $\varepsilon_N = 0.5$. The Frisch elasticity is lower and in the range of microeconomic studies. It is important to match the multiplier in the presence of distortionary taxes and absent wage setting frictions.

We calibrate elasticities for across home and foreign goods and for individual varieties as in Nakamura and Steinsson (2014): $\eta = 2$ and $\theta = 7$. The home bias in consumption is set similarly to Nakamura and Steinsson (2014) when the home region is of size $n = 0.1$. We adjust the home bias with the size of the region to also match the home bias of the larger region implied by their calibration. This yields $\phi_H = \frac{2}{3} + \frac{1}{3}n$ and $\phi_F^* = \frac{2}{3} + \frac{1}{3}(1-n)$.

The labor income share is 0.66. Together with $\theta = 7$ this implies a cost share of capital of $\alpha = 0.20$. We set the adjustment costs of investment and utilization to $\kappa_I = 5$ and $\frac{a''(u)}{a'(u)} = 0.20$, close to the estimates in Leeper et al. (2017) of 5.46 and $\frac{0.16}{1-0.16} = 0.19$. We calibrate an annual depreciation rate of 8% and an annual nominal interest rate of 4%, as well as an annual inflation rate of 2%. The slightly lower discount rate compared to Nakamura and Steinsson (2014) yields a higher and thus more realistic share of investment in GDP, given the cost share of capital. The share of public capital in production is calibrated by assuming that the provision of public infrastructure maximizes output net of public investment in steady state. This yields $\zeta = \frac{\delta_g K_g}{Y}$, which is about 0.02 in the U.S. data.

Policy rules. We calibrate a monetary policy rule as in Galí (2008), with a persistence of $\rho_r = 0.75$, a coefficient on inflation of $\psi_\pi = 1.5$ and a coefficient on output of $\psi_y = \frac{1}{8}$.

The federal and the state and local government adjust labor tax rates to pay for expenditures, as the federal government does in Nakamura and Steinsson (2014). While Nakamura and Steinsson (2014) assume a balanced budget, we find that this yields too strong a response of the tax rate to a surprise increase in defense spending. For example, Ramey (2011) estimates an average increase of 0.05pp over the first year following a 1% increase in government spending. We thus assume that the federal government adjusts labor income taxes to pay for a fraction $1 - \gamma_f$ of current expenditures, where $\gamma_f = 0.8$ in our baseline calibration.

Since there is no guidance on state and local governments, we turn to empirical correlations to guide the calibration. In the state fiscal data, we find that the tax rate exhibits positive autocorrelation, and increases in the tax rate are positively correlated with increases in the interest paid on debt and with expenditures net of IG transfers. In contrast, we do not find correlations that suggest stabilizing of the state budget through adjustments to overall expenditures or transfers (Table D.2). In our baseline rule, we thus use the estimated persistence of the tax rate of 0.80, converted to a quarterly frequency of 0.95, and scale up the coefficients on interest rate payments and net expenditures by the same factor to achieve determinacy – this requires a reaction that is 4.5 times stronger than in the reduced form regressions in Table D.1.

Alternatively, we consider the same fiscal rule for the state government as for the federal government – with an added small loading of 0.1 on real debt to ensure determinacy. We also consider full adjustment $\gamma_f = 0$ for both the state and the local government tax rates.

Shock process. We calibrate the IG process to the 2009 stimulus package: We choose $\rho_{IG} = 0.89$ to yield a half-life of six quarters, given the duration of the 2009 stimulus of about three years (Drautzburg and Uhlig, 2015, Fig. 1) and a cumulative (non-discounted) value of \$320 bn (Carlino and Inman, 2013), or 2.2% of GDP at the time. This yields $\omega_{IG} = 100 \times (1 - \rho_{IG}) \times 0.022 = 0.24$: IG transfers rise initially by 0.24% of GDP. For ease of comparison, we impose the same process for the federal government spending process.

State government spending goes towards government consumption and investment and targeted transfers in proportions equal to those in the state budget. That is, 38% of spending goes towards transfers to constrained agents, treating transfers to municipalities as equivalent as transfers to persons. Of the remainder, 25% is spent on investment, consistent with NIPA data on the importance of state and local government consumption and investment.

States and their marginal propensity to spend. In our baseline calibration, we calibrate the two representative states to be of equal size ($n = 0.5$). We label the home state to be the "Republican" one, while we identify the foreign state as "Democratic." The states differ only in the pass-through of IG increases to state spending. While only the difference between partisan propensities to spend has a causal interpretation, for illustrative purposes we also use the point-estimate for the Democratic baseline in addition to the estimated Republican-Democratic partisan difference. Specifically, we use the estimates from column (7) of Table 4 as our baseline. Given the evolution of the underlying polarization measure, they imply a Democratic pass-through of 1.59 and a Republican pass-through that is 1.27 lower for the

2000s.[12] These estimates imply an increase in the partisan differences of 0.99, i.e., before the Reagan era the MPS was only 0.28 lower under Republican governors.

Our baseline calibration attributes the unobserved heterogeneity to the governors, assuming that if the partisan composition were to change, the mix of governors would reflect the unobserved heterogeneity in the full sample. If we use, instead, the MOV as a proxy for unobserved heterogeneity, due to, for example, current preferences in the electorate, and removing the effect of the MOV, we find larger effects: Specifically, when we use column (8) of Table 4 to isolate the effect of partisanship, we get a Democratic baseline of \$2.23, while Republican governors spent \$2.09 less per dollar received in the 2000s, compared to just \$0.53 less before the Reagan era.

To reflect the uncertainty surrounding our estimates, we not only compute the results for the point estimates, but also use the Delta method to compute confidence intervals.

4.3 Results

We quantify the role of partisanship on the effects of a surprise increase in IG transfers in two scenarios: First, we illustrate 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 increase changes if one were to reduce the partisan differences to their pre-Reagan era level. Second, we fix the partisan differences at the level prevailing in the 2000s and vary the size of the Republican region to compute how IG transfer multipliers would have changed over time as a function of the changing partisan composition of U.S. governors.

4.3.1 Dynamics following a shock to federal transfers

We first present the responses of the fiscal instruments to the IG transfer shock, shown in Figure 4. The top left panel shows the exogenous increase in IG, which initially increases by

[12] Even though the estimates are literally for annual increase, we interpret them as applying more broadly to business-cycle increases. This is consistent with our finding that similar pass-through differences hold at multi-year horizons; see Table C.4.

almost 0.25% of GDP. The other two panels in the top row show the responses of Republican state spending and Democratic state spending. Here, as well as in all subsequent panels, we distinguish between three cases: (1) The "all Democrats" case, in which the Republican governor shares the Democratic propensity to spend (dashed orange line). (2) The pre-Reagan partisan difference (dotted purple line), and (3) the post-2000 level of partisan differences (solid blue line). For these cases, we focus on our mean point estimate. We also compute the difference between cases (2) and (3), shown as the solid yellow line accompanied by dashed 90% confidence intervals. Since the pre-Reagan partisan differences were small, we focus our discussions on cases (1) and (3), and the effects of reducing partisan differences.

In the "all Democrats" case, the spending in both states increases by 0.36% of GDP in both regions. As spending increases more than IG revenue (and the tax base does not rise enough), state labor income tax rates also rise gradually, flattening out at 0.2% after 10 quarters (or 0.01pp, given the 5% steady state rate). The federal tax rate rises by 0.24%, corresponding to a 0.07pp federal tax rate increase.

In the scenario with the "post 2000 partisanship," the home state has the lower Republican propensity to spend, while the foreign state is unchanged from the previous scenario. Mechanically, the Republican state has lower spending and tax rates. We focus again on the posterior median, shown as the blue solid line. This is evident in the lower Republican spending increase in this scenario, which is only 0.06% of GDP. Tax rates in the Republican region now fall gradually, by almost 0.2% after 20 quarters. While the level of the pass-through is not causally estimated in our regression, we focus on the partisan differences, shown in yellow lines for the mean (solid), along with the 90% confidence intervals (dashed lines). The spending difference is centered around -0.24% of GDP, since the MPS differ by almost one dollar per dollar received, and IG rises by 0.24% of GDP. The confidence interval ranges from -0.06% to -0.42% of GDP. The median difference in income tax rates gradually builds up to -0.5%, with a confidence interval between -0.15% and 0.85%. In contrast, income tax rate increases at the federal level and the Democratic state are virtually unchanged from the first scenario.

Underlying the increases in expenditure are increases in transfers and government consumption and investment, as Figure D.1 in the Appendix shows. In the Republican region, government consumption and investment rises by 0.24% of GDP and transfers rise by 0.15%, adding up to the overall 0.39% increase. Since spending is exogenous, spending by Democrats is just proportionately higher given the estimated partisan differences.

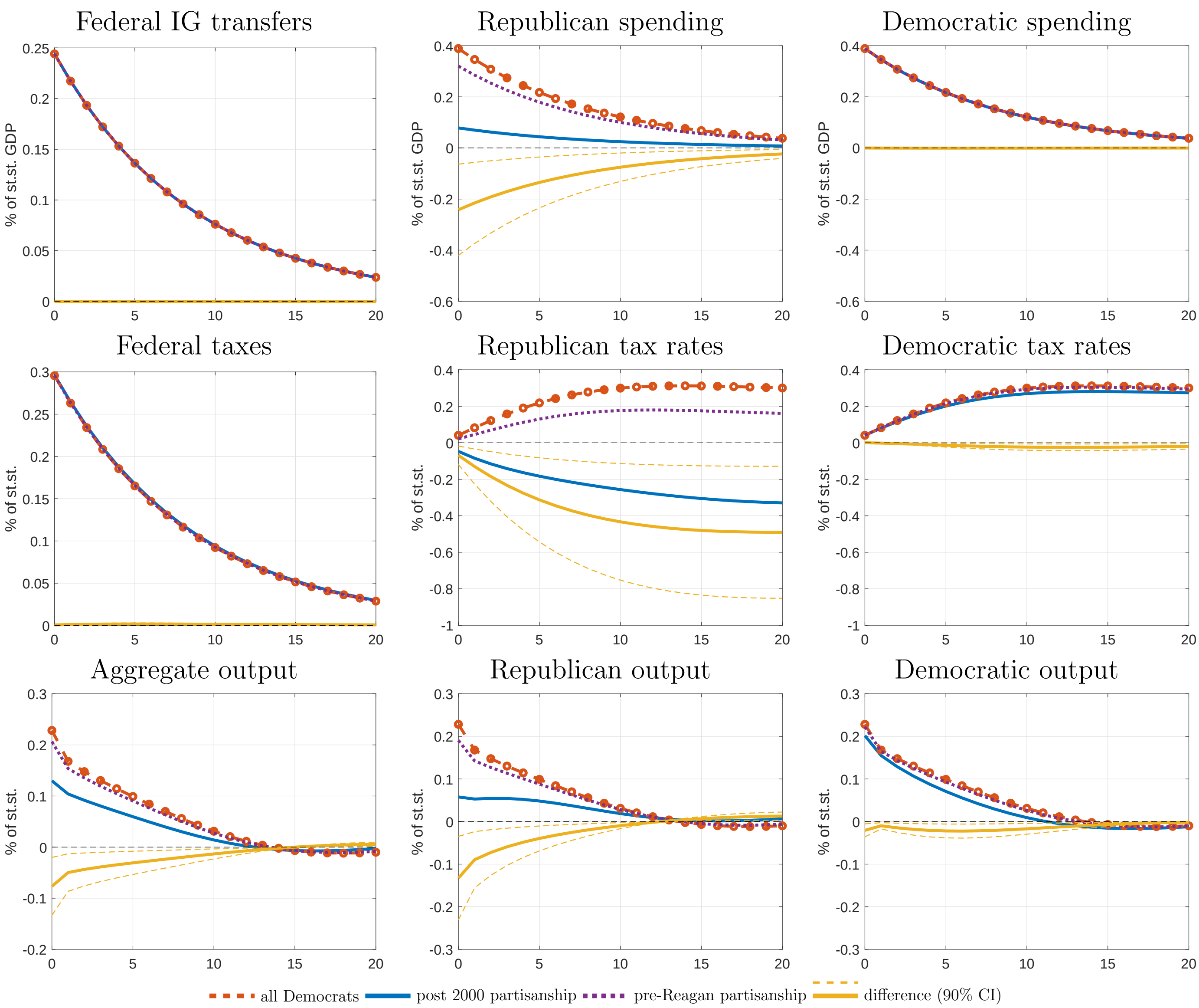


Figure 4: Impulse-responses of state and federal fiscal variables as well as output following a shock to IG transfers.

Next, we describe the overall economic effects of the differences in state fiscal policies. The bottom panel of Figure 4 shows aggregate and state-level output. When policy-makers in both regions behave as the Democratic baseline, output would rise by almost 0.24% on impact in both regions, and gradually reverse to zero. With the estimated partisan difference, output in the Republican region rises by only 0.06% on impact. This corresponds to a difference of GDP, relative to the pre-Reagan era degree of partisanship, of about -0.13% (-0.04%, -0.22%) in the Republican region – with only small effects of around -0.02% on the Democratic region. Correspondingly, the partisan difference lowers aggregate output by about 0.08% at the point estimate relative to the pre-Reagan era estimate, to an increase of 0.12%. Given the capital adjustment costs, the responses and differences in hours worked (not shown), are slightly larger than those for GDP.

The shock also leads to modest producer price inflation in both regions. Underlying the price increases are rising factor costs for both capital and labor. Specifically, absent partisan differences, producer price inflation increases by 0.13pp (at annual rates) in both regions on impact, as the middle row of Figure D.2 in the Appendix shows. With the partisan differences, inflation in the home region runs only at slightly above 0.05pp. Higher GDP and the corresponding increase in aggregate inflation lead the monetary authority to raise interest rates. Absent partisan frictions, this increase peaks at about 0.12pp, but is roughly 0.03pp lower with the estimated partisan differences, see Figure D.3 in the Appendix.

The output response is largely driven by the direct response to spending. This follows from our calibration strategy, which results in an impact multiplier to government spending of 0.8, implying little crowding out in response to a federal spending shock. On impact, consumption is unchanged when policymakers have Democratic preferences, as Appendix Figure D.3 shows. In contrast, it falls in the Republican region, due to lower demand from the constrained agents.

4.3.2 Multipliers

After discussing the response of the economy to IG transfer shocks in detail, we now turn to a measure of how effective fiscal stimulus is: How much does the federal government stimulate the economy for each dollar it spends? How sensitive is this answer to the preferences of state policymakers? To answer this question, we follow Mountford and Uhlig (2009) and analyze present discounted value (PDV) multipliers: The ratio of the PDV of output relative to the PDV of federal transfers. Figure 5 shows these PDV multipliers over time, along with output and spending, which determine the numerator and denominator, respectively.

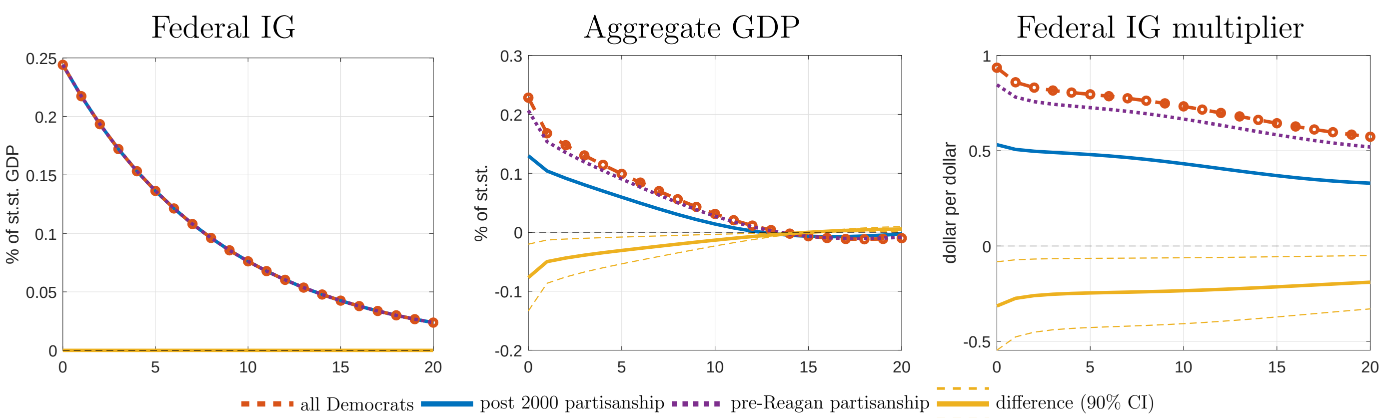


Figure 5: IRFs: Fiscal stimulus, GDP response, and PDV multipliers for IG transfer shocks

Without partisan differences, output rises almost one for one with federal IG spending, resulting in a multiplier just shy of one. With the post 2000 partisan differences, output rises by only 0.13% on impact, following a 0.24% increase in spending relative to GDP. This results in a short-term multiplier of about 0.53. Relative to the small pre-Reagan era partisan difference, the multiplier is 0.31 lower, with a standard error of 0.14. The multipliers and the partisan difference decline only slowly over time and stabilize at a positive long-run multiplier, discussed below. Figure D.4 provides a comparison with a federal defense spending shock and the associated multiplier.

Table 7 compares short-run and long-run multipliers for IG transfers and federal government consumption when states spend on transfers, consumption, and investment in the top panel. The bottom panel considers the case without productive state capital and with states only spending the stimulus on government consumption. Given our calibration strategy, the federal consumption multiplier is, by construction, 0.80 on impact. The pre-Reagan era IG multiplier with the empirical composition (top panel) would have been comparable, at 0.85.[13] However, the increased partisan difference lowers it to 0.53. With states spending only on government consumption (bottom panel), the impact IG multipliers would be larger, because government consumption does not discourage work as transfer payments to constrained households do.

Anything that increases the aggregate multiplier in the model or the MPS in the data increases the IG multiplier – and the implied partisan difference. The table illustrates this in two ways. First, if we hold the nominal interest fixed for ten quarters, demand side (spending) policies become more important, and short-run effects of supply side policies become less important. The government spending multiplier thus rises from 0.80 to 1.24 – see, for example, Christiano et al. (2011).[14] Here, the IG multiplier rises proportionately when spent only on state consumption, and more than proportionately when spent on a mix of consumption, investment, and transfers. In this case, the multiplier difference rises from -0.31 to -0.60. Second, we would observe a similar increase in magnitude if we based our simulation on estimates that remove the effect of MOV terms on policymakers' estimated preferences. Both the MPS and the partisan difference in MPS rise by about 50%, yielding about 50% higher multiplier differences.

Comparing our simulation results with a back of the envelope calculation reveals the important channels in the model. Wolf (2019) shows that, for a class of models, only the aggregate increase in demand and the multiplier matter. Applying this idea to our model yields a partisan effect on the multiplier of -0.4: For each federal dollar spent, the partisan

[13] The federal consumption multiplier is smaller than the IG multiplier because our denominator is the cost of stimulus to the federal government, not overall spending. In response to an IG shock, the total government spending thus exceeds the federal government spending. Multipliers larger than unity thus do not imply crowding in of private activity, as it would for multipliers on overall government spending.

[14] We implement the ELB in our linearized model via monetary policy shocks calibrated to keep interest rates constant for 10 quarters, all revealed at the same time as the fiscal policy shocks.

Table 7: Short-run and long-run PDV multipliers on federal consumption and IG transfers as a function of partisan bias and the duration of the ZLB, for different state spending compositions

(a) States spend on welfare, state consumption, and state investment (baseline)

Multiplier horizon	ELB duration	Federal G increase	IG increase – full sample pre-Reagan	2000s	Δ (s.e.)	IG increase – MOV controls pre-Reagan	2000s	Δ (s.e.)
Impact	0 qtrs	0.80	0.85	0.53	-0.31 (0.14)	1.18	0.68	-0.50 (0.20)
Impact	10 qtrs	1.24	1.47	0.87	-0.60 (0.27)	2.08	1.13	-0.95 (0.39)
Long-run	0 qtrs	0.54	0.26	0.44	0.18 (0.08)	0.05	0.34	0.29 (0.12)
Long-run	10 qtrs	1.00	0.91	0.78	-0.13 (0.06)	1.01	0.80	-0.21 (0.08)

(b) States spend on state consumption only

Multiplier horizon	ELB duration	Federal G increase	IG increase – full sample pre-Reagan	2000s	Δ (s.e.)	IG increase – MOV controls pre-Reagan	2000s	Δ (s.e.)
Impact	0 qtrs	0.80	1.20	0.77	-0.44 (0.19)	1.66	0.97	-0.69 (0.28)
Impact	10 qtrs	1.24	1.91	1.16	-0.75 (0.33)	2.68	1.49	-1.19 (0.49)
Long-run	0 qtrs	0.54	0.41	0.54	0.13 (0.06)	0.26	0.47	0.21 (0.09)
Long-run	10 qtrs	1.00	1.16	0.94	-0.21 (0.09)	1.34	1.01	-0.34 (0.14)

MPS difference of \$0.99 for half the (governor) population corresponds to a difference of \$0.50. With a multiplier of 0.8, this yields a difference in IG multiplier in the order of 0.4. Our model estimate is only 0.31 because of wealth effects on labor supply for the constrained agents (welfare discourages work), productive state spending, and a different financing mix for federal and state expenditures. These features are absent in Wolf (2019). In the model with just state consumption spending, we still have a small discrepancy (-0.44 vs. -0.40) due to the differences in distortionary taxes. While government spending multipliers can be sensitive to distortionary taxation, our results are comparable when taxes are persistent.[15]

Our results suggest that the effects of federal fiscal policy depend on who is running the states. The fraction of states run by Republicans has varied significantly over our sample period: The left panel in Figure 6 shows the fraction of states governed by Republicans, omitting the rare independent governors. This fraction ranges from a low of 30% after Reagan took office to a high of roughly two thirds during Clinton's second term. Using these

[15] We show this by changing only the state and federal government tax rules. Figure D.4 shows multipliers and tax rate changes for the baseline and two scenarios: (1) Applying the federal tax rule also to state tax rates, so that state taxes adjust to cover 20% of the cost of increased expenditures net of IG revenue. (To ensure stability, we also let taxes adjust to state debt increases, with a loading of 0.1.) In this case partisan pass-though differences yield a multiplier difference centered around -0.25, similar to our baseline. (2) With a balanced budget, which is at odds with the typical deficit finance, the multiplier difference is around -0.1.

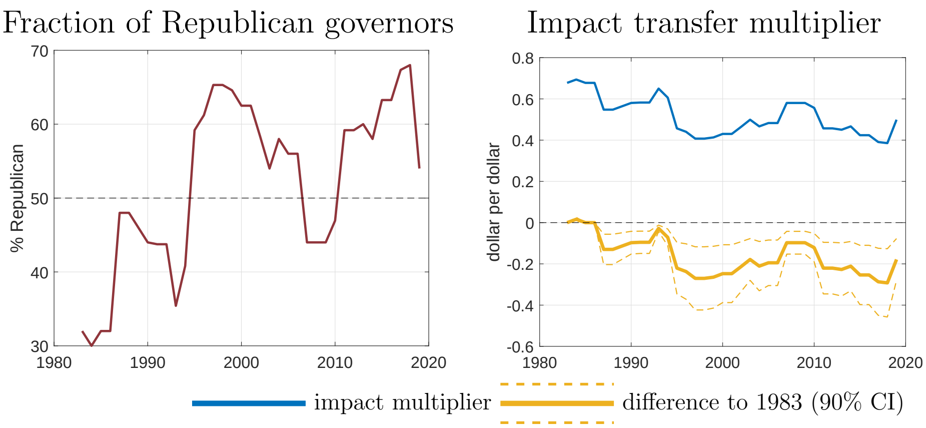


Figure 6: Transfer multipliers for historical shares of Republican vs. Democratic governors

values to calibrate n in our model translates to sizable differences in the impact transfer multiplier, shown in the middle panel of Figure 6. The transfer multiplier peaks during the early Reagan years with values slightly above 0.8 and falls to about 0.6 during Clinton's and Obama's second terms. Taking the estimation uncertainty into account puts the 90% confidence interval for the impact multiplier in 2018 between 0.1 and 0.42.

4.3.3 Economic activity in the data

The model predicts that, on impact, Democratic-governed states have higher levels of economic activity, but that after a few quarters growth in Republican-governed states is higher. In our calibration, the higher growth in Republican regions is because of less mean-reversion. For other parameters, it could also be due to supply side policies. We now turn to this prediction in the data. Below we report results from regression (2.3) with the change in the employment-to-population ratio on the LHS. While this measure is not lined up with the fiscal year, the overall results are consistent with our model results.

First, we find that current activity – straddling the first half of the fiscal year – is lower in Republican-run states following increases in IG. Specifically, columns (1) through (5) in Table C.10 show that the employment-to-population ratio is lower under marginally elected Republican governors than under Democratic governors: If IG increased by 1pp, the employment to population ratio drops by 0.04pp to 0.05pp relative to the Democratic-run state, depending on the specification. The partisan difference does not appear to be associated with public employment (not shown) and appears to be reversed after one year, although the t-statistics for future activity, which are near 1, are inconclusive.

Second, we find that the results for current activity weaken and may be reversed when looking at future activity – straddling the second half of the fiscal year. Columns (6) and (7) in Table C.10 consider future changes in the employment-to-population ratio. Column (6) implies that these changes are an order of magnitude smaller compared to the results for current and insignificant. Column (7) implies that IG transfers increase future employment changes under Republican governors relatively more, and significantly so.

5 Model validation in aggregate time series

In this section, we test the prediction of our model that the IG transfer impact multiplier falls as the share of Republican governors increases. We test this prediction by estimating multipliers to IG transfer shocks in a model that includes the share of Republican governors.

We use the surprise component of intergovernmental transfers as the transfer shock, treating it as exogenous to other current shocks. Identification of the IG shock is similar to the approach Blanchard and Perotti (2002) took for government purchases: We posit a decision lag in fiscal policy, so that unexpected changes in fiscal policy are contemporaneously unaffected by changes in current period GDP. Since the NIPA data used in the time series analysis excludes an important automatic stabilizer, the unemployment insurance program, and some other funds, we view this assumption as reasonable. Below we also describe the rich set of controls that we use to ensure that the changes in IG transfers were unexpected by economic agents at the time.

Our estimating equation builds on the local projection approach (Jordà, 2005):

$$\ln GDP_{t+h} = \alpha_{0,h} + \alpha_{Rep,h} Rep_{t-4} + \beta_{0,h} \ln IG_t + \beta_{Rep,h} \ln IG_t \times (Rep_{t-4} - \overline{Rep})$$

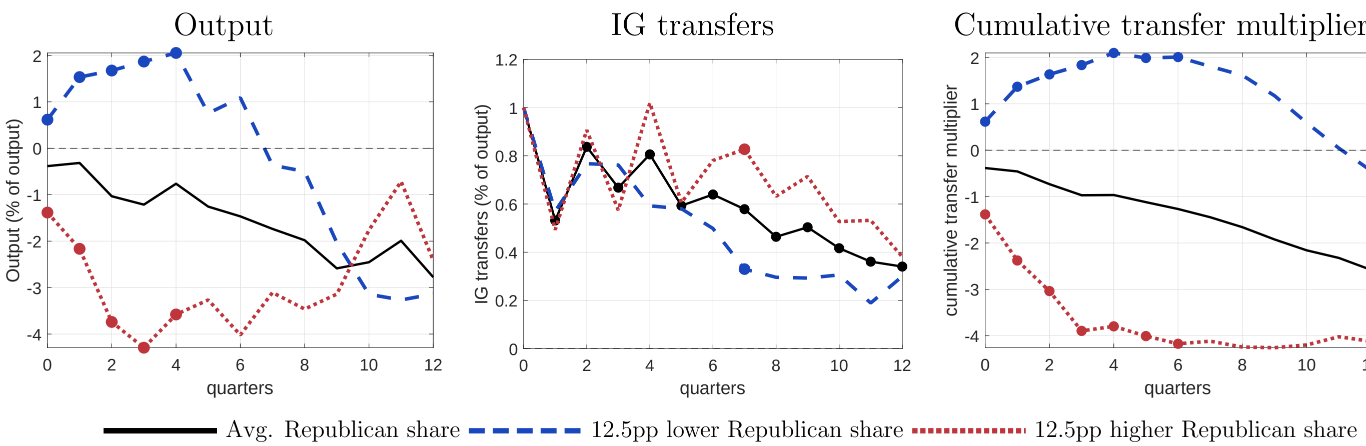
$$+\sum_{\ell=1}^{4}\mathbf{x}'_{t-\ell}\gamma_{0,\ell}+\sum_{\ell=1}^{4}\mathbf{x}'_{t-\ell}\times(Rep_{t-4}-\overline{Rep})\gamma_{Rep,\ell}+u_{t+h}. \tag{5.1}$$

To control for agents' expectations, $\mathbf{x}_{t-\ell}$ includes lags of GDP, federal expenditures, state and local expenditures, federal tax revenue net of transfers, and intergovernmental transfers, all in logs and real per capita terms. We lag the share of Republican governors by four quarters to account for the fact that state budgets are passed one fiscal year in advance, the same as in our panel regressions. We use quarterly data from 1964q1 to 2018q3. Our focus is on $\beta_{0,h}$ and $\beta_{Rep,h}$, which yield the response at horizon h of GDP to the IG shock as a function of the share of Republican governors.

The results from estimating equation (5.1) are shown in Table E.1(a) in the Appendix. As the second row of the table shows, up to four quarters out, the effect of intergovernmental transfers shrinks as the (lagged) fraction of Republican governors increases. This last finding is qualitatively the same as in our structural model.

To interpret the local projection estimates quantitatively, we compute the implied IRFs and the cumulative multiplier. Figure 7 shows the IRFs for aggregate GDP and intergovernmental transfers following an increase in IG equal to 1% of GDP, along with the cumulative multiplier. To compute the multiplier, we estimate (5.1) jointly with GDP and IG on the LHS. The solid black line denotes the baseline with the average Republican share of governors. Dots on the baseline denote that the estimate at the given horizon is significantly different from zero at the 10% level. The dashed line shows the effect when the Republican share is one standard deviation (12.5pp) below average. The dotted lines show the effect when the Republican share is one standard deviation above average. For these responses, dots denote significant partisan differences at the 10% level.

The partisan effects on output are significant up to four quarters out, while the baseline output effect is not significantly different from zero. Partisan effects on IG transfers themselves are largely insignificant, consistent with the notion that state partisan considerations do not influence federal transfers. This lack of partisan effects is intuitive, because transfers largely follow administrative formulas. When the Democratic share of governors is one standard deviation (12.5pp) higher than usual, the estimates imply an impact multiplier of 0.6, which rises up to 2.1 after six quarters, before declining. When the Republican share is average or below, the estimated multiplier is negative.



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

Figure 7: Politics-dependent responses to innovations in IG transfers, 1964q1–2018q3.

Adding agents' expectations as a way to control for anticipation of fiscal policy does not affect our qualitative results. Ramey (2011) and Leeper et al. (2013) have documented the importance of accounting for agents' information set when estimating fiscal multipliers. In Figure E.1 we first add one-quarter ahead inflation and output growth expectations to our baseline model. Second, we also add one-quarter ahead expectations of both federal and state and local government purchases. Third, we also add three-quarter ahead purchase expectations. All expectation measures are from the Philadelphia Fed's Survey of Professional Forecasters. We also interact the expectation measures with the share of Republican governors. In all three cases, we confirm that the impact output effects are lower when a higher share of states is governed by Republicans. We also find that once we control for expectations, the output effects at the two to three year horizon rise as the share of Republicans governors increases – consistent with positive supply side effects of their tax policies.

To test a further prediction of our model, we estimate whether the national government purchases multiplier depends on the share of Republican governors. Our model implies that the share of Republican governors only affects the economy through their use of intergovernmental transfers. When we run the same interacted regression for the government purchases multiplier, we find an insignificant effect of the interaction term; see Table E.1(c). This shows that our finding is not an artifact of the Republican share of governors being a proxy for some underlying determinant of federal purchases, policy, or the economy more broadly.

6 Conclusion

States are often charged with implementing national policies in economic unions such as the U.S. But states and their governors have their own agendas, which may conflict with the national policy objectives. In this paper, we show that federal spending policies that give transfers to states have disparate impacts across states, depending on the partisan affiliation of their governors. State-level partisanship effects do not average out across states but can have aggregate effects. We quantify this for the case of stimulus policies.

Our results have important implications for fiscal stimulus, such as for the economic recovery from the coronavirus pandemic. We find that traditional government stimulus through direct purchases or transfers targeted to credit constrained households stimulate the economy more than transfers to the states in the short run. If the scale of fiscal stimulus requires involving the states, policymakers should design the state fiscal stimulus package carefully. Our results suggest that there may be less partisan disagreement about certain uses, such as infrastructure. Infrastructure spending may thus be an effective stimulus.

While we focus on fiscal stimulus in the U.S., similar lessons may apply more broadly. In the U.S., federal policymakers should consider partisan preferences when designing spending programs that rely on the states. One example may be welfare reform. Other economic unions, such as the European Union, also face conflicts over the use of funds (Ivanova et al., 2017). They may also need to pay attention to agency conflicts with member states.

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