PARTISANSHIP AND FISCAL POLICY IN ECONOMIC UNIONS:
EVIDENCE FROM U.S. STATES

Gerald Carlino
Thorsten Drautzburg
Robert P. Inman
Nicholas Zarra

ABSTRACT

Partisanship of state level politicians affect the impact of federal fiscal policy in the U.S. Using data from close gubernatorial elections, we find partisan differences in the marginal propensity to spend federal transfers since the early 1980's: Republican governors spend less. A New Keynesian model of partisan states in a monetary union implies sizable aggregate income effects from these partisan differences. First, the transfer multiplier would rise by 0.60 if Republican governors were to spend as much from federal aid as do Democratic governors. Second, the observed changes in the share of Republican governors imply variation in the fiscal multiplier of 0.40. Local projection regressions support this prediction.

Gerald Carlino
Federal Reserve Bank of Philadelphia
Research Department
Ten Independence Mall
Philadelphia, Pa 19106
jerry.carlino@phil.frb.org

Thorsten Drautzburg
Federal Reserve Bank of Philadelphia
Research Department
Ten Independence Mall
Philadelphia, Pa 19106
tdrautzburg@gmail.com

Robert P. Inman
Department of Finance
The Wharton School
University of Pennsylvania
Philadelphia, PA 19104-6367
and NBER
inman@wharton.upenn.edu

Nicholas Zarra
NYU Stern School of Business
44 West 4th Street
New York, NY
nzarra@stern.nyu.edu

1 Introduction

The United States and many other important democracies are economic unions: collections of politically independent but integrated economies.[1] While the national government funds national public goods and provides for aggregate income and price stability, state and local governments often are required to implement national policies. Examples include health care, national infrastructure, and unemployment and income insurance. The political independence of the subnational governments creates a principal-agent problem between separately elected national and state or provincial governments for these policies. The national government is the "principal" who funds a significant share of services via intergovernmental (IG) transfers. State and local governments act as "agents" who provide the funded services.[2] The agency problem is well documented: States spend much of IG transfers (the "flypaper effect"), but do not necessarily spend transfers as intended by the federal government.[3] We first document that the flypaper effect varies by political party. Second, we show that the partisan differences matter for the impact of national stabilization policies on the aggregate economy.

The importance of IG transfers has grown over time, and particularly during severe downturns. Figure 1 illustrates the growth of IG transfers in the U.S. since 1902 from 0.01% of GDP to as much as 3.7% of GDP in the aftermath of the Great Recession. In 2019, U.S. federal transfers to state and local governments were 14.8% of the federal budget. Temporary increases in federal aid often occur during recessions (see the shaded recession bands in Figure 1). For example, the Great Depression saw the introduction of nationally funded but state administered transfer programs to lower income households and the unemployed. The importance of intergovernmental aid as a stimulus for the macro-economy was also evident

---

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

[2]There is a large literature on the theory of optimal intergovernmental transfers beginning with Musgrave (1959), Oates (1972), and summarized in Inman and Rubinfeld (2020, ch. 8). Lockwood (1999) provides the contemporary analysis of transfer policies as an exercise in mechanism design.

[3]See Nicholson-Crotty (2004) for examples of states diverting IG funds.

in the 2009 American Recovery and Reinvestment Act (ARRA) as a response to the Great Recession. ARRA reserved $318 billion of its $796 billion in aggregate economic stimulus for allocation by U.S. states and localities. The U.S. government's response to the economic decline caused by the Covid-19 pandemic has also allocated significant federal transfers to the state and local public sector.[4]

Figure 1: U.S. federal intergovernmental transfers to states since 1902
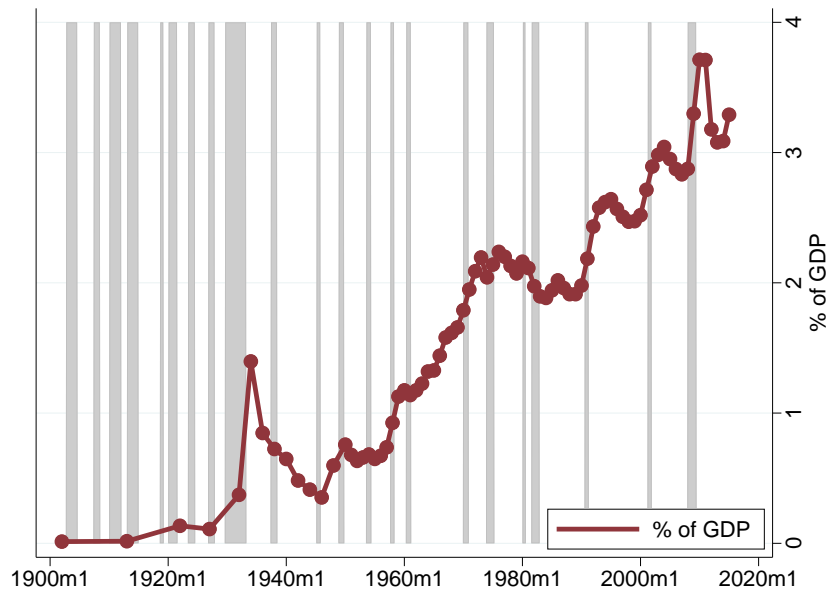
Source: U.S. Census for grant data, Ramey and Zubairy (2014) for historical GDP data, and authors' calculations. Gray shaded areas indicate NBER recession dates.

Recent theoretical work has stressed the importance of IG transfers for aggregate fiscal policy in currency unions, see Gali and Monacelli (2008), Ferrero (2009), and Farhi and Werning (2017). Because of free trade, state stabilization policies are likely to have significant consumption spillovers; see Carlino and Inman (2013) and Auerbach et al. (2019). This leads to an inefficient under-provision of union-wide expansionary policies if left to state governments. Central government borrowing to finance IG transfers is one policy response. Intergovernmental transfers from the central to union-member governments play two po-

---

[4]While we focus on the U.S. in our paper, IG transfers and the resulting agency problem matter in other non-unitary governments, for example the European Union. Similar to the U.S., the European Council allocates significant transfers to member states. Similar to the U.S., EU transfers are also diverted to member states' own priorities; see Ivanova et al. (2017).

tentially important roles: First, to provide income insurance for residents in response to country-specific shocks, and second, to help stabilize the union-wide economy in response to both country specific and union-wide shocks.

Understanding how IG transfers are allocated to state governments and how states then allocate those transfers is essential for predicting the effects of transfers on the aggregate economy. As politically independent agents, elected state officials may choose to allocate transfers in ways counter to the intention of national policy-makers. Partisan differences are the explanation we study here. Consider the recent expansion of Medicaid. Republican politicians at the state level often blocked the Medicaid expansion that formed part of the Democratic healthcare reform bill; see Washington Post (2013) and also Kaiser Family Foundation (2019). Such decisions are not limited to Republicans: Democratic governors refused funding for a Republican approved federal education program promoting sexual abstinence (Raymond et al., 2008). We argue here that the partisan affiliation of governors has become important for federal transfer programs, particularly since the presidency of Ronald Reagan in the 1980s.

Our paper makes two contributions. First, we show that the so-called flypaper effect measuring the budgetary impacts of federal aid for state and local government spending varies by partisanship.[5] Estimates of these impacts are essential for the implementation of federal fiscal policies using state and local governments. When averaged across parties, our results are consistent with current estimates of the flypaper effect in the literature. The familiar approaches to estimating this effect conceal large partisan differences, however. We estimate the impact of state-level partisanship on the implementation of national government policies paid for by IG transfers. Given state-level budgetary institutions, we focus on the the political party of the governor as our measure of partisanship. Central to our analysis

---

[5]Gramlich (1977), Hines and Thaler (1995), and Inman (2009) survey this extensive literature. The literature's concludes that federal money "sticks where it hits": A federal dollar given directly to a household, say through tax relief, leads to about $.04 to $.10 in additional state and local spending. In contrast, a dollar given directly to a state and local government has been estimated to lead on average to $.25 to even more than $1.00 increase in state or local spending.

is the identification of partisan differences in state responses to national transfers – that is, the marginal propensity to spend from federal aid (MPS) under Democratic or Republican governors. We identify partisan differences in spending of IG transfers using panel 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 city spending. We find statistically significant and economically important differences: Democrats favor spending, while Republicans favor tax relief. Our estimated partisan differences in the MPS emerged during and following the tenure of President Reagan, a Republican, and parallel the national increase in partisan polarization of U.S. politics documented by McCarty et al. (2016) and Azzimonti (2018). In contrast to the post-Reagan era, we find no evidence of partisan difference in the pre-Reagan era with its lower levels of polarization, a result consistent with evidence surveyed in Potrafke (2018).

Our second contribution is to quantify the macroeconomic effects of these partisan differences in MPS. We focus on the federal multiplier of IG transfers on GDP. This IG multiplier matters for national policymakers hoping to stimulate the economy via IG transfers. The IG multiplier is the product of two effects: First, the MPS from IG aid and, second, the effect of changes in state government spending or tax relief on aggregate GDP. Our state level estimates yield the first effect. To quantify the second effect, we use a macroeconomic model consistent with recent estimates of aggregate fiscal multipliers. Our causal state-level estimates parameterize the partisan fiscal rules for a representative Democratic and Republican governors in the macro model. The model features states in a monetary union and shares the many New Keynesian features of the models in Nakamura and Steinsson (2014), Brueckner et al. (2019), 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 endogenous labor supply and capital accumulation. The model is flexible enough to give a role for Democratic policies, which are estimated to favor increased spending, including federal and state transfers to lower income households, as well as Republican policies, which

favor tax relief for households.

Without partisan differences in governor allocations (pre-1980) the aggregate impact multiplier of federal IG transfers is 0.85. Allowing for partisan differences with half the states assigned the policy preferences of a Republican governor and half of a Democratic governor reduces the impact multiplier to about 0.5. The reason for the decline is initially lower state spending and thus lower aggregate demand in Republican states. Finally, we vary the partisan division among states to match that of U.S. states from 1983 to 2014. The model predicts that the aggregate impact multiplier falls as the share of Republican governor increases. We validate the model's prediction for U.S. data using a local projection time series regression.

Our paper is one of several to estimate the aggregate effect of federal aid. Chodorow-Reich (2019) provides a detailed review of estimates of the aid multiplier. Based upon studies evaluating the impact of ARRA intergovernmental aid on local jobs and income, he concludes the best estimate of aid's impact on the aggregate economy, if deficit-financed, is a national multiplier of 1.7. Our multiplier estimates are typically smaller, in part because IG transfers are eventually tax financed. Our focus, however, is less on the level of the IG multiplier, but rather on estimating and quantifying on how partisan differences matter for its relative impact. Overall, our analysis points to the potential importance of partisan differences in policy-makers' preferences as a new source of heterogeneity in macroeconomics, along with that for households and firms.

2 Estimating Partisan Differences in Aid Allocation

2.1 Specification: Governors and the Allocation of Aid

State and local governments receive federal IG aid in one of four ways: (1) lump-sum aid with no constraints on purpose (e.g., General Revenue Sharing); (2) lump-sum aid for spending on a specific policy objective (e.g., ESEA Title I aid for the education of lower

income children), (3) matching aid paying a share of program expenses up to a cap on total aid (e.g., Water and Sewer Facilities Assistance), and finally (4), open-ended matching aid with no limit on assistance (e.g., Medicaid). Each of the first three forms of assistance provides a fixed sum of funding, with or without programmatic restrictions on how the money may be spent. While efforts are often made by the funding agency to enforce spending restrictions – known as maintenance of effort provisions – such constraints are very difficult to enforce and binding only for new programs with no prior state or local spending.[6] Without enforcement, the recipient government is free to use any lump-sum categorical grant as it wishes within its budget, allocations known as the "fungibility" of aid. We assume fungibility and aggregate all assistance in the first three categories into a single lump-sum transfer called hereafter IG aid (denoted as IG). Open-ended matching aid, of which Medicaid is the only significant example, pays a fixed share of the costs of allowed state spending at a matching rate m; such assistance is effectively a price subsidy. Our empirical analysis will focus on the impact of lump-sum IG aid.

The institutional features of state budgeting motivate our econometric specification. First, the central role of the state reversion (status quo) and the use of contining resolutions if no budgetary agreement can be reached between the governor and and the state legislature leads us to specify our regression for government spending relative to last year's budget, and thus to focus on changes in spending; see Persson and Tabellini (2000, Chapter 2). Second, governors are in a strong bargaining position: Governors have the right to propose a budget, and, in most states, have the power to veto or change individual budget items. These institutions, plus governors' appointment powers, underlies our specfication of the governor as the decisive political agent for state budgets; see Barrilleaux and Berkman (2003) and Kousser and Phillips (2012). Third, registered voters from the Democratic and Republican parties choose party candidates for governor from a set of "citizen-candidates"

---

[6]While maintenance of effort is easily monitored with programs that directly reimburse a share of state spending as for open-ended matching grants such as Medicaid, even these funds have been found to be diverted; see Nicholson-Crotty (2004). For programs that provide a fixed allocation of funding as for IG aid, most of program aid is diverted to other spending or to general tax relief; see Carlino and Inman (2016).

6

wishing to represent the party in the general election; see Besley and Coate (1997).[7] If candidates are policy motivated, rather than just office-motivated, and if the registered voters in the party primaries care about the governor's policies when in office, the convergence result of Downs (1957) is overturned and the winning candidate will typically not represent the preferences of the state-wide median voter (Wittman, 1983).[8] We recognize the dominant position of the governor in budgeting, and the potential importance of her partisan decided preferences, by including an indicator variable for the governor's party affiliation as an explanator of IG aid's impact of changes in state spending. Finally, we explicitly allow for the possibility of asymmetry in the impact of increases and decreases in IG aid on spending to accommodate the possibility of "habit formation" in observed preferences for state services or tax relief, coming from either divided government or constituent preferences.[9]

Equation (2.1) provides the core specification for changes in log expenditures $\Delta \ln E_{s,t}$ in state s in fiscal year t in response to changes in (ln) IG aid in state s and year t. The effect of aid may differ when aid increases ($\Delta \ln IG_{s,t}^{+} = \max\{0, \Delta \ln IG_{s,t}\}$) or decreases ($\Delta \ln IG_{s,t}^{-} = \min\{0, \Delta \ln IG_{s,t}\}$). It may also differ by governors party affiliation as Republican ($Rep_{s,t-1} = 1$) or Democratic ($Rep_{s,t-1} = 0$):

$$\Delta \ln E_{s,t} = (\gamma_{0,+} + \gamma_{r,+} \times Rep_{s,t-1})\Delta \ln IG_{s,t}^{+} + (\gamma_{0,-} + \gamma_{r,-} \times Rep_{s,t-1})\Delta \ln IG_{s,t}^{-}$$

$$+ \mu_0 + \mu_r \times Rep_{s,t-1} + \text{fixed effects} + \epsilon_{s,t}. \tag{2.1}$$

---

[7]The citizen-candidate specification ties party policy directly to the preferences of the candidate chosen to represent the party, an outcome described by Lee et al. (2004) as "complete" divergence of the politician's preferences from contemporaneous pressures of the electorate. They reject the alternative of no divergence, or the median voter result, using data on U.S. Representatives' policy choices.

[8]Party voters trade off the benefits of holding office against those of having a candidate who will implement policies closer to their preferred allocations; see Alesina and Spear (1988) and Harrington (1992).

[9]We leave unspecified the exact source of spending asymmetries. There are at least two potential explanations. First, constituent preferences may reveal a form of habit formation where past state spending comes to be seen as an "entitlement" by Democratic constituents and past levels of after-tax incomes as an entitlement by Republican constituents. Entitlements then play the role of a "minimum bundle" which must be protected against IG cuts (with borrowed funding, for Democrats) or enhanced with IG increases (with tax cuts, for Republicans). Second, divided government with a governor veto and a status quo reversion means the Democratic governor can protect against spending cuts when IG aid is cut, and the Republican governor can protect against big spending increases when IG aid is increased. We do not test specifically for one explanation or the other but our extensive specification of fixed effects controls for a possible role of lagged spending and taxation or a history of divided governments.

Our aim is to recover the average MPSs for Democratic and Republican governors separately for when aid is increased or decreased. We lag the governor's party affiliation by one year as state budgets are decided one year prior to their implementation. Finally, $\epsilon_{s,t}$ is the error term, which is allowed to be correlated across states and time. The μ_0 coefficient is the average spending growth under Democratic governors, and $\mu_0 + \mu_r$ is the average spending growth under Republican governors, unrelated to changes in IG aid. Under our preferred fixed effects specification, μ_0 and μ_r are unidentified, however. While the growth rates are nominal, the fixed effects always include time dummies, so that estimated growth rates are real changes. A Democratic governor's MPS elasticity equals $\gamma_{0,+}$ when IG aid is increased ($\Delta IG_{s,t}^+ > 0$) and equals ($\gamma_{0,-}$) when IG aid is decreased ($\Delta IG_{s,t}^- < 0$). A Republican governor's MPS elasticity is measured relative to that of the Democratic governor – that is, $\gamma_{0,+} + \gamma_{r,+}$ for an increase in aid and $\gamma_{0,-} + \gamma_{r,-}$ for a decrease in aid. We conjecture that partisan differences are non-zero. Since the Republican party is often associated with lower taxes and spending cuts and the Democratic party with spending increases, we expect $\gamma_{r,+} < 0$ and $\gamma_{r,-} > 0$; see Besley and Case (2003).

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

2.2 Estimation

OLS estimation of equation (2.1) may lead to inconsistent estimates of the causal effects of partisanship: that is, γ_r^+ and γ_r^- may not be consistently estimated. The governor's party is not assigned randomly, but may be correlated with unobserved events that simultaneously affect state spending. For example, an economic shock such as a local recession may shift election outcomes towards more fiscally conservative (Republican) candidates, at the same time that the state is forced to cut expenditures and the federal government increases IG aid.[10] The OLS estimator would then yield a downard bias to the coefficient $\gamma_{r,+}$ that is unrelated to the underlying causal effect of partisan preferences. Political shocks, such as scandals, may have similar effects, if governors use fiscal policy to minimize the consequences of their misdeeds.

While our fixed effects control for some unobservables, they will do so only imperfectly. To address this issue, we use a regression discontinuity design (RDD) based on close elections. In standard RDD, the identifying assumption is that, in close elections, the election outcome is unrelated to the governor's policy preferences or the state of the economy. For example, when elections are close, say they are decided by a margin of victory (MOV) of 2pp (51 vs. 49 percent), small exogenous events, such as weather on election day or a clumsy TV appearance, could decide who wins the election. RDDs based on close elections, identify differences in intercepts by assuming that the winning candidate (and her partisan preferences) has been quasi-randomly assigned and is therefore independent of any unobservables.[11] Since we want to identify differences in slopes (MPS), we will need a stronger assumption. We assume that, conditional on close elections, the party affiliation of the governor is jointly independent of the change in IG transfers and of shocks to expenditures. To ensure that IG transfers used in our estimation are exogenous to the party affiliation of the governor, we

---

[10]See Peltzman (1992), who shows that Democratic governors are particularly harmed by large spending increases in re-elections.

[11]See Imbens and Lemieux (2008) generally, and Lee et al. (2004) and Ferreira and Gyourko (2009) for applications similar to ours.

exclude welfare transfers from our measure of IG, as previously discussed.

While the regression discontinuity allows us identify the partisan differences, it does not allow us to identify all preference parameters. Under the assumption that IG aid and remaining unobservables are independent from the party affiliation of the governor conditional on close elections, the partisan difference in the MPS is identified. But selection – e.g., Democrats being elected more frequently in periods of higher IG increases and higher expenditure growth – could make the baseline MPS asymptotically biased. We therefore focus on the partisan differences. We will, however, use the point estimates as a plausible (exogenously specified) benchmark to compute the implied levels of Democratic and Republican MPS's for policy simulations.

Our preferred specification identifies discontinuities in marginal effects rather than discontinuities in average effects as in standard RDDs. To control for selection, we include the winning governor's MOV and that MOV interacted with the governor's political party, and changes in IG.[12] In addition to the simple equation (2.1), we, therefore, estimate the following (baseline) specification:

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

To choose the MOV cutoff when estimating (2.2), we balance the the gains in precision from a larger cutoff against the possibility of reduced bias from a smaller cutoff. Minimizing the estimated root mean sequared error leads us to prefer a MOV below 10pp as the cutoff (55% vs 45%).[13] For robustness, we also estimate the more parsimonious specification without

---

[12]See Caetano et al. (2017), who analyze a similar setting where the treatment effect could be zero on average, even though the treatment effect may be nonzero for certain groups. In our preferred specification, the average spending effect is captured by fixed effects. Here, partisan differences emerge conditional on changes in IG.

[13]Specifically, we use cross-validation to compute the root-mean-squared error (RMSE) of fitting eq. (2.2) to our data: We estimate (2.2) first leaving one state out at a time, and then one year out at a time. We then

MOV controls with governors elected with a MOV of up to 5pp.[14] We cluster standard errors by state and year.[15]

For ease of interpretation, it is useful to transform our estimates of $\gamma_{r,+}, \gamma_{r,-}$ from elasticities to dollar coefficients. To do so, we could simply use the average ratio of expenditures to IG transfers. However, this ratio varies across states. Instead, we transform the left-hand-side variable directly. 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. 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 the scaled variable approach to yield more precise estimates, as measured by smaller standard errors.

2.3   Data and Sample

The model is estimated using panel data encompassing fiscal and political outcomes for the fiscal years, 1983 to 2014. The year 1983 is the first fiscal year for state governments to respond to new fiscal federal policies following the election of Ronald Reagan as president, typically viewed as the start of polarization in U.S. politics; see McCarty et al. (2016) and Azzimonti (2018) for rising polarization in national politics and Shor and McCarty (2011) for U.S. states. Our sample includes all states except those states with large sovereign wealth funds financed through severance taxes. These states have the luxury of treating ΔIG as a change in wealth, rather than income. We therefore exclude from the analysis Alaska and Wyoming and (after the 2009 fracking boom) North Dakota.[16] State fiscal data are from the *Annual Survey of State and Local Government Finances*, U.S. Census of Governments. Data for governors political party and terms of office are from *Book of States*, Council of State

---

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 preferred cutoff is 10pp.

[14]We also considered a specification with a third order MOV polynomial. The point estimates were consistent to our baseline specifications, but less precise.

[15]We use the `reghdfe` package for `Stata` by Correia (2016).

[16]We drop these states starting in the year that they instituted their wealth fund: Wyoming (1975), Alaska (1976), and North Dakota (2009). Only these states have severance tax revenue shares $\geq 20\%$.

Governments. Economic data are for calendar years from the *Regional Economic Accounts*, U.S. Bureau of Economic Analysis.

Table 1: Variable Means and Significance of Partisan Differences: 1983-2014

	Full sample	Sample with close elections			Dem=Rep *t*-stat by FE		
	(1) All	(2) ≤10pp	(3) Dem≤10pp	(4) Rep ≤10pp	(5) None	(6) St+Yr	(7) 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	.	.

Significance of partisan differences for terms of governors decided by MOV≤ 10*pp*, 1983-2014. Shares and ratios in percent. All growth rates are real per capita. Tests for significance of partisan differences are from regressions explaining each variable by a dummy variable indicating if the governor is Republican (omitting independents), and including linear MOV controls and no fixed effects (col. 5), or state and year fixed effects (col. 6), or state and (year×region) fixed effects (col. 7). *t*-statistics are based on standard errors clustered by state and year.

Table 1 summarizes the data used in our analysis, and importantly, provides tests for the identifying assumption that the measured economic, political, and fiscal attributes of our sample states are similar between states electing a Democratic or Republican governor in a close election (MOV≤ 10*pp*). Column (1) presents the full sample's mean for each variable and then the means for the variable for a sample of close elections (Col. (2)) and for the sample of "close elections" divided between states that elected a Democratic governor (Col. (3)) or a Republican governor (Col. (4)). A statistical comparison of sample means between states with closely elected Democratic and Republican governors for each variable does not allow us to reject the null hypothesis of equal means as reported by the t-statistic for the two-sided test for equality of means; see Cols. 5 (no fixed effect controls), Col. 6 (year and state fixed effect controls), and Col. 7 (state and region x year fixed effect controls). We conclude that the sample of close elections is balanced by all plausibly important economic,

political, and fiscal covariates with the elected governor's party. We will therefore interpret any estimated policy differences in an elected governor's allocation of $\Delta \ln IG$ to $\Delta \ln E$ to be due to the partisan preferences of the elected governor as chosen by his or her party.

3   Estimates of state-level partisanship

3.1   Graphical analysis

In Figure 2 we illustrate the working of our RDD. Governors are grouped into subsamples by their MOV, with all winning Democratic governors collected in positive MOV bins and all Republicans in negative bins. Bins have a width of one percentage point. A positive MOV equal to 2 corresponds to the subsample of Democratic governors winning their election by a margin of more than 1pp, but no more than 2pp (i.e., within a 51% to 49% margin). Each panel shows the estimated MPS for an increase in IG aid (γ^+) from each bin as an elasticity, with the ± 1.65 standard error band for each estimate shown as the shaded area.[17] Panel (a) omits fixed effects, panel (b) includes party-specific fixed effects.

Both panels in Figure 2 show a clear break in the estimated MPS elasticities as the MOV approaches zero (tied elections): The Republican governors' estimated MPS elasticities are close to zero, while closely elected Democratic governors have an estimated MPS elasticity near 0.25. The difference between these elasticities at zero identifies the partisan difference. Fitting a linear regression to the binned elasticities in panel (a) yields a difference of 0.26 (a Democratic intercept of 0.15 and a Republican interceptof -0.11): For a 1% increase in IG transfers, Democrats increased expenditure growth by 0.26pp more than Republicans. The results with fixed effects are very similar. Away from close elections, there are no observed differences in MPS elasticities: When the MOV approaches 10pp in absolute terms,

---

[17]The +2pp to +3pp bin contains an influential observation: Ann Richards, a Democratic governor of Texas in the early 1990s. While governors Richards remains in the sample, we have removed Democratic governor Bob Wise of West Virginia. Under the tenure of these two governors, their states experienced particularly high growth in both IG aid and expenditures. Without either, the elasticity would also be around 0.3 also in the +3pp bin. Adding Bob Wise, the elasticity is +0.94 (no FE) and +0.86 (with FE).

the estimated MPS tend to be positive with insignificant differences for Republicans and Democrats, as the overlap of the confidence intervals for large MOVs indicate. In this case, a MOV far from the cutoff suggests that, on average, the winning party picked a position close to the state's median voter's, nonpartisan, preferences and was able to defeat an ideological (partisan) opponent by a large margin; see Alesina and Spear (1988).

Figure 2: Regression discontinuity in slopes: 1983–2014.

(a) Without fixed effects

(b) With party × (year, state) fixed effects
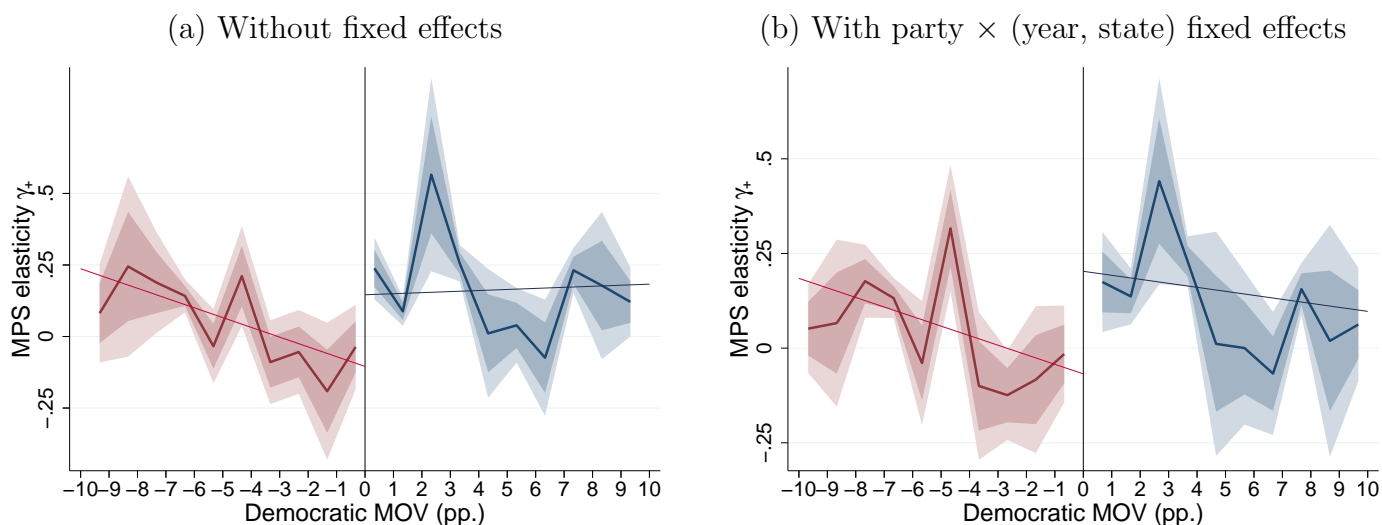
Shown are the estimated MPS elasticities for each 1 percentage point MOV bin along with their 68 and 90 percent confidence bands, based on standard errors clustered by year and state. Specification based on eq. (2.1) controls without (panel a) or with (panel b) fixed effects. Overlaid are linear regressions with each bin's estimated MPS regressed on the MOV for each bin, with MPS estimates weighted by the inverse of their squared standard errors.

Governors' responses to cuts in transfers also show significant partisan differences, but of opposite sign.[18] Without fixed effects, the Republican MPS elasticity in response to a cut in IG aid is to cut spending and is about .2 larger than the MPS of Democratic governors. With party by year and party by state fixed effects, the difference in MPS elasticities is .3 and again for larger cuts in spending by Republican governors. But away from the point of zero MOV the MPS elascities are similar, as for spending increases with increases with IG aid. When partisan preferences are decisive, Republican governors cut their expenditures more than do Democratic governors for the same cut in IG transfers.

[18]See Figure B.2 in the Online Appendix.

That only Democrats increase their expenditure growth in times of high transfer growth is central to our policy analysis – and drives our results. Figure 3 shows this by reporting estimates for a RDD in means for samples with high and low transfer growth: The dark lines show the point estimates and 90% confidence intervals (dashed bands) for the mean expenditure growth conditional on IG increases below the 75th percentile for various MOV bins, while the thin line and its 90% confidence interval (shaded band) show the same for the mean expenditure growth conditional on IG increases above the 75th percentile. The left panel is without fixed effects, while the right panel includes state and year fixed effects.[19] The two confidence intervals for Republican governors (negative MOV) lie on top of each other, suggesting their their spending does not respond to changes in IG transfers. In contrast, for Democratic governors, expenditure growth is significantly higher when IG growth is high rather than low. For a MOV near zero, the difference is about 7pp without fixed effects and 2.5pp with fixed effects.

Finally, Figure 3 provides insight for when significant partisan differences are most likely to be observed. For our sample, it will be for large spending increases typically observed following a major federal IG policy initiative. The mean percentage increase in IG aid for observations in the upper 25th percentile was 18.9%, or $114 (in 2010 dollars) per resident for those years and states. For the remainder of the sample, the percentage increase in aid was 4.6 percent, or $25 per resident. Thus, we find significant partisan effects just when increases in aid are large and likely to be most economically significant. Importantly for the analysis of Section 4, among these high aid observations are the major IG aid policies designed to respond to major recessions.[20]

---

[19]Without fixed effects, we estimate the plots within a MOV of ±6.5pp, based on cross-validation of equation (2.2) without fixed effects. Here, the fixed effects cannot be party specific, because the part-specific means would otherwise be unidentified. Figures B.3 and Figure B.4 in the Appendix also shows the analogous with a cutoff at the 50th percentile, and also includes the binned observations.

[20]Specifically, the Emergency Jobs Act of 1983, the Jobs and Growth Tax Relief and Reconciliation Act of 2003, and the American Recovery and Reinvestment Act of 2009.

Figure 3: Average expenditure Growth by Election MOV, conditional on IG increases: 1983-2014.

(a) Without fixed effects

(b) With year and state fixed effects
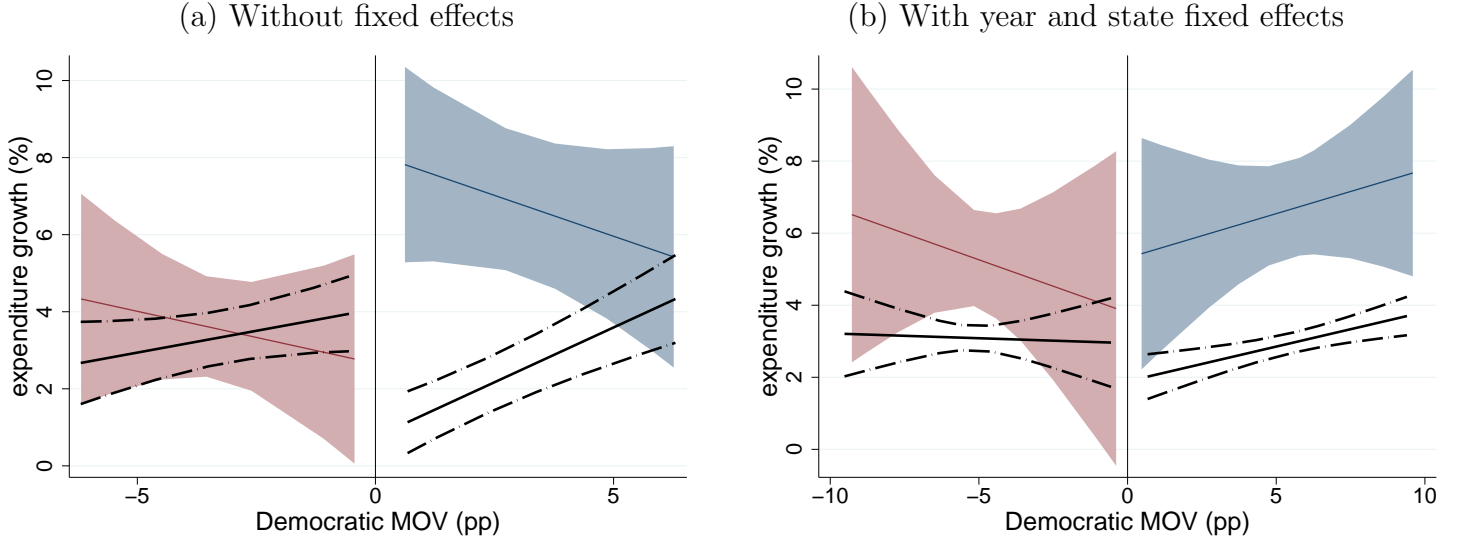
Shown are the predicted mean expenditure growth for a given MOV with its 90th percentile confidence band, first for the observations with IG increases above the 75th percentile (thin line, shaded band) and then for observations with IG increases below the 75th percentile (dark line, dashed band). Coefficient standard errors clustered by year and state.

3.2 Estimates of Partisan MPS

Tables 2 and 3 provide estimates of partisan differences in MPS: The first three columns contain the result for the full model from eq. (2.2) for MOV margins of 10pp with linear MOV controls, and for the full sample with third order MOV polynomial controls. Columns (4) and (5) are for eq. (2.1) without MOV controls for samples with a MOV of less than 4pp or 5pp. For comparison, column (6) contains results for elections that are not close (MOV>10pp). Table 2 uses the scaled expenditure growth as the regressand, so that coefficients have the interpretation of a dollar-for-dollar MPS. Table 3 shows elasticity estimates.

In Table 2, we show estimates of the impact of $\Delta IG_{s,t}$ on $\frac{E_{s,t-5}}{IG_{s,t-5}}\Delta E_{s,t}$ for increases and decreases in aid conditional on the state governor's political party. The estimated coefficients for "IG incr." and "IG decr." measure the MPS of Democratic governors to increases and decreases in aid, while the coefficients for "Rep x IG incr." and "Rep x IG decr." measure the partisan difference in MPS when a state's elected leadership switches from Democratic to

Table 2: MPS Dollar Estimates for Non-Welfare IG Transfers: 1983-2014.

MOV cutoff	with MOV terms as in eq. (2.2)			without MOV terms as in eq. (2.1)		
	(1) ≤10pp	(2) ≤10pp	(3) ≤10pp	(4) ≤5pp	(5) ≤4pp	(6) >10pp
IG incr.	1.260*	1.351***	1.411**	1.698***	1.678***	0.741***
	(0.63)	(0.48)	(0.65)	(0.31)	(0.32)	(0.24)
Rep x IG incr.	-1.661*	-1.919**	-1.510	-1.803***	-2.142***	-0.117
	(0.96)	(0.78)	(1.03)	(0.61)	(0.61)	(0.27)
IG decr.	-0.322	0.271	-0.641	0.125	-0.047	0.368
	(0.63)	(0.76)	(0.70)	(0.67)	(0.54)	(0.32)
Rep x IG decr.	3.541***	2.996**	3.020***	2.193**	2.186***	0.663
	(0.90)	(1.26)	(0.93)	(0.91)	(0.63)	(0.54)
Republican Gov.	0.167**		0.173**			
	(0.08)		(0.07)			
R-squared	0.46	0.53	0.56	0.64	0.67	0.46
R-sq, within	0.11	0.10	0.11	0.14	0.12	0.04
Observations	636	634	634	313	259	872
States	47	47	47	43	41	48
Years	32	32	32	32	32	32
State FE	Yes	By party	Yes	By party	By party	By party
Year FE	Yes	By party	By region	By party	By party	By party
MOV controls	Linear	Linear	Linear	No	No	No

Standard errors clustered by state and year in parentheses. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < .01$, **: $p < .05$, *: $p < .1$.

Republican. From Section 2, Democratic (liberal) governors are expected to spend more than Republican (conservative) governors of any increase in aid and to cut less of any decrease in aid. Table 2 confirms those predictions for a variety of RDD and fixed effects specifications.

The first three columns in Table 2 present results for governors who won in elections with a MOV of up to 10pp. Following eq. (2.2), these specifications include linear MOV controls. Column (1) includes state and year fixed effects, column (2) includes party-specific state and year fixed effects, and column (3) includes state fixed effects and U.S. Census region by year fixed effects. The coefficient estimates imply that Democratic governors increase state spending by between \$1.260 and \$1.411 for a \$1 increase in IG aid. Our focus is, however, on the partisan differences, estimated as the coefficient for Republican governor (Rep_{t-1}) interacted with increases or decreases in aid. Switching governors from a Democratic governor to a Republican governor lowers the MPS by between \$1.510 and \$1.919. We can reject the hypothesis that the partisan differences are positive at the 10% level in all cases and by the more conventional two-sided test for no differences (either positive or negative) at least at the 15% level in all cases.

While we focus on identifying the partisan difference, if we consider the coefficients for the Democratic baseline at face value, the results are reasonable: We cannot reject the hypothesis that Republican governors have a zero MPS in all three cases (*p*-values between 0.16 and 0.45): Republican governors allocate their increase in IG aid to tax relief or to repaying government debt.[21]

For a \$1 decrease in IG aid, we find the opposite pattern: The MPS, here measuring cuts in spending, is significantly higher under Republican governors than under Democratic governors. The spending cut is between \$2.996 and \$3.541 higher for each dollar cut in IG aid (all statistically significant): Republican governors cut spending relatively much more than Democrats when IG is cut. Taking the Democratic coefficients as our benchmark, Democratic governors are estimated to reduce spending by between -0.641 (i.e., to increase spending) and 0.271 dollars. None of these estimates is statistically different from zero. When Democratic governors lose IG aid, they protect spending either by raising state taxes, by borrowing, or by withdrawing funds from state rainy day funds or pension accounts. Adding the Democratic baseline and the causally identified partisan difference suggests that Republicans cut spending by more than the loss in federal aid, making funds available for tax cuts or debt repayment.

A simpler specification tells the same story. Columns (4) and (5) remove MOV controls, but use only elections won with a MOV of up to 5pp and 4pp, respectively. The estimated partisan difference for IG increases imply that Republican governors spend \$1.803 or \$2.142 less than Democratic governors. Together with the Democratic baseline coefficient near \$1.7, we cannot reject a zero Republican MPS. For IG decreases, the partisan difference is also large and statistically significant. Finally, and consistent with our graphical analysis in Figure 2, when we exclude data on close elections and omit MOV controls in column (6), we find no significant partisan difference following either IG increases or cuts. Governors who win by large margins will likely have adopted policy positions attractive to the state-wide median

---

[21]In an earlier version of this paper (Carlino et al., 2020), we provide direct evidence of relatively lower tax rates and interest payments under Republican governors following IG increases.

voter as well as a fraction (>5%) of the opposing party's voters nearest the median. The coefficient estimates for governors elected by a wide margin suggest non-partisan policies, because they largely reflect median voter preferences in the general election, rather than the partisan preferences we are trying to identify.[22]

Table 3: MPS Elasticity Estimates for Non-Welfare IG transfers: 1983-2014.

	with MOV terms as in (2.2)			without MOV terms as in (2.1)		
MOV cutoff	(1) ≤10pp	(2) ≤10pp	(3) ≤10pp	(4) ≤5pp	(5) ≤4pp	(6) >10pp
IG incr.	0.169**	0.181***	0.189***	0.195***	0.194***	0.091***
	(0.06)	(0.04)	(0.07)	(0.03)	(0.04)	(0.03)
Rep x IG incr.	-0.236**	-0.266***	-0.220*	-0.233***	-0.271***	-0.008
	(0.10)	(0.08)	(0.11)	(0.07)	(0.07)	(0.04)
IG decr.	-0.046	-0.018	-0.081	-0.020	-0.034	0.032
	(0.06)	(0.07)	(0.07)	(0.07)	(0.06)	(0.03)
Rep x IG decr.	0.343***	0.337***	0.313***	0.264**	0.266***	0.083
	(0.06)	(0.10)	(0.09)	(0.10)	(0.07)	(0.05)
Republican Gov.	0.016*		0.018**			
	(0.01)		(0.01)			
Expenditure/IG-rev.	8.90	8.90	8.89	9.01	9.04	8.79
R-squared	0.46	0.54	0.56	0.65	0.69	0.45
R-sq, within	0.11	0.10	0.11	0.13	0.12	0.04
Observations	636	634	634	313	259	872
States	47	47	47	43	41	48
Years	32	32	32	32	32	32
State FE	Yes	By party	Yes	By party	By party	By party
Year FE	Yes	By party	By region	By party	By party	By party
MOV controls	Linear	Linear	Linear	No	No	No

Standard errors clustered by state and year in parentheses. p-values based on t-distribution with degrees of freedom equal to the number of year-clusters. ***: $p < .01$, **: $p < .05$, *: $p < .1$. To compute a dollar-to-dollar MPS, multiply the elasticity by the Expenditure/IG revenue ratio.

Table 3 repeats the analysis of Table 2, but now for a log-linear specification. This specification has the advantage of allowing for variable estimates of partisan differences in MPS conditional upon the relative importance of IG aid to state spending. Table 3's estimates of the impact of changes in aid are qualitatively similar to those reported for the linear specification in Table 2. We confirm that Republican governors spend less of an increase in IG aid than do Democratic governors. Again relative to the Democratic baseline, we fail to reject that the Republican MPS is zero. Also as in Table 2, Democratic governors

---

[22]An F-test that there are no partisan differences for the MOV>10pp sample cannot reject that null hypothesis ($p = .47$). Further, point estimates for increases and decreases in IG aid without partisan interactions are both significant and equal .68 and .71 respectively, estimates well within the range of the familiar (median voter) flypaper effect.

are seen to not cut spending when IG aid is decreased while Republican governors make significant percentage cuts. Estimates of the partisan difference in MPS are robust across all six of the samples and specifications when considering one-sided tests, and only very marginally insignificant with the cubic controls when considering a two-sided test. That the estimated elasticities in Table 3 are all well below 1.0, implies the partisan difference in MPS declines as IG aid becomes relatively more important, but not strikingly so. For example, the implied partisan difference in MPS for a \$1 increase in IG aid as estimated in Table 3, col. (1) for states with the median level of IG aid is \$2.33, while that for states in the upper 25 percentile of IG aid is \$2.23. These magnitudes are broadly similar to the ones reported in Table 2.

Finally, while the post-Reagan period is our focus, it is instructive to compare estimated values of MPS for the years prior to the Reagan presidency. Below we provide evidence for whether partisan differences in state fiscal allocations vary with increasing national political polarization. The years prior to 1982 had lower levels of polarization, but there remained significant changes in federal IG programs.[23] Here, we focus on the polarization measure of Azzimonti (2018). Denoting polarization by P_t, normalized to have zero mean and unit standard deviation, we estimate:

$$
\frac{E_{s,t-5}}{IG_{s,t-5}} \Delta \ln E_{s,t} = \underset{[0.24]}{(1.27} + \underset{[0.30]}{0.68} P_{t-1} + (\underset{[0.34]}{-0.53} + \underset{[0.45]}{-1.10} P_{t-1}) \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^{+}
$$

$$
+ (\underset{[0.16]}{0.20} + \underset{[0.01]}{-0.10} P_{t-1} + (\underset{[0.22]}{0.81} + \underset{[0.30]}{0.58} P_{t-1}) \times Rep_{s,t-1}) \Delta \ln IG_{s,t}^{-}
$$

$$
+ \text{MOV} \times \text{IG} \times \text{Polarization} \times \text{party interactions} + \text{fixed effects} + \epsilon_{s,t}. \quad (3.1)
$$

The underlying sample period is 1968 to 2014 for all elections ($N = 2,226$ observations); standard errors clustered by state and year are in parentheses. The results here for a linear MOV provide a conservative estimate of the effects of polarization.

The estimates in equation (3.1) imply that before the 1980s, when polarization was

---

[23]Table B.1 in the Online Appendix provides estimates of eq. (3.1) using different measures of national polarization. The results are similar to those reported here. Measures if polarization in state legislatures are available only for a few years of our sample; see Shor and McCarty (2011).

relatively low, estimated partisan differences in MPS were insignificant. Prior to 1980, P_t averaged minus one standard deviation. This implies a point estimate for the partisan difference in MPS for an IG increase of 0.57 $(= -0.53 + (-1) \times (-1.10))$ with a standard error of 0.59. For IG cuts, the point estimate of partisan differences is 0.23 with a standard error of 0.39. We conclude that the national trend towards increased political polarization helps to explain the estimated partisan differences in the post-Reagan years and it motivates the start date of our main sample in 1983.[24]

3.3 Discussion

The estimated post-Reagan partisan differences in MPS are large, statistically significant, and likely to be lost in the typical approach to estimating the response of state and local governments to changes in IG aid, which omits partisan interactions. A simple OLS regression for the (1983-2014) sample including year-party and state-party fixed effects to measure the marginal spending impact of new IG aid without partisan effects or allowing for asymmetric changes has an estimated impact of \$.72 (s.e. = .15) for \$1 of aid. From Table 2, a sample weighted average over governorships yields a MPS for increases in IG aid ranging from \$.48 to \$.96 and for decreases in IG aid of \$1.1 to \$1.8. The overall weighted average of MPS across both increases and decreases in aid ranges from .75 to 1.18. These average estimates are well within the range of most flypaper studies, as reviewed by Hines and Thaler (1995) and Inman (2009).

Our estimates unravel the previous average estimates of MPS from the flypaper literature to reveal an important new margin of agent heterogeneity, partisanship, that must be respected when designing fiscal policies in economic unions. The importance is particularly evident when the central government uses IG transfers to member states to stimulate the macro economy: This stimulative effect is the product of how much state spending or tax relief the federal government induces for each federal dollar spent and the effect of the state

---

[24]Our estimates are in line with the survey by Potrafke (2018) on the effects of growing partisanship on state policies.

spending or tax relief on the economy. Our estimates above yield the first component. For the second component, we generally need a model. However, when demand effects dominate, we can use a approximation suggested by Wolf (2020) – called "demand equivalence" – for the impact of fiscal policy on the aggregate economy. This simple calculation reveals directly the likely impact of IG transfers on aggregate income and, in the process, the potential importance of partisan differences.

By demand equivalence, the aggregate effect of fiscal policy will equal the multiplier for federal fiscal policy times the impact of any transfer shock to individual agents – here state governors – on agents' economic behaviors, here the governor's MPS. An increase in IG aid stimulates state consumption by the governor's MPS which, when multiplied by the multiplier for federal spending policy, equals the (demand equivalent) change in aggregate income. For example, a political realignment of governorships from that in 1983 of 70% Democrat/30% Republican to 33% Democrat/67% Republican in 2018 – the maximal realignment in our sample – and assuming an aggregate fiscal policy multiplier of .80 Ramey (2011) will reduce the aggregate fiscal multiplier for IG transfers by .57, perhaps as much as 50 percent.[25] While our structural model of the aggregate economy in Section 4 features supply effects that invalidate the exact demand equivalence specified by Wolf, we still confirm the large potential importance of partisanship on the aggregate policy impacts of IG aid.[26]

---

[25]Based on our estimated partisan differences between Republican and Democratic MPS's in Table 2, col. 2 of -1.92, the implied demand equivalent multiplier for IG transfers in 1983 will equal $.80 \times (.70 \times MPS_D + .30 \times (MPS_D - 1.92))$ while that in 2018 will equal $.80 \times (.33 \times MPS_D + .67 \times (MPS_D - 1.92))$. The difference in the two multipliers will then equal $.57 = .80 \times .37 \times 1.92$, where .37 is the percentage higher share of Democratic governors in 1983 and 1.92 is the partisan increase in MPS by switching from a Republican to Democratic governor. In our simulation model, we benchmark the two MPS's for an increase in IG aid at 1.92 for Democrats and 0.0 for Republicans. For this benchmark the implied IG multiplier in 1983 is 1.07 and that in 2018 is .50.

[26]The structural model, through the state budget constraint, implies that changes in IG aid may impact state taxes and state debt as well as state spending. An empirical analysis, like that in Tables 2 and 3, for taxes and debt is more challenging as it requires estimates of changes in tax rates, tax bases (i.e., tax mix) and fees, as well as changes in the market value of outstanding state debt. The required data are not available. For the structural model, therefore, we estimated reduced form equations for changes own state revenues and the stock of (price deflated) government debt; see the Calibration Table, Table 4 below. An earlier version of this paper provides evidence of lower tax rates (where available) and lower interest payments under Republican governors following an increase in IG transfers; see Carlino et al. (2020). Implied behavior in our structural model is consistent with these estimates.

4 Partisan states in a macroeconomic model

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

We model a New Keynesian economy of states (regions) within a monetary union, similar to Nakamura and Steinsson (2014) and Auclert et al. (2019).[27] Its New Keynesian nature gives a role to both demand-side and supply-side policies. Firms set prices in monopolistically competitive markets subject to nominal rigidities and some households live hand-to-mouth. These features give rise to an aggregate demand channel for fiscal policy. Capital accumulation, endogenous labor supply, and state-level distortionary taxes imply a potentially important role for supply-side policies. We discipline the relative strength of these channels by calibrating the model to match the federal government consumption multiplier in Ramey (2011) We calibrate our fiscal experiment to the IG portion of the 2009 U.S. stimulus bill.

4.1 Environment

There are two states, inhabited by representative households and intermediate firms. The home state is of size n, while the foreign state is of size $1-n$, $n \in [0, 1]$. 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 common monetary authority. Except for policy-makers' preferences and possibly their size, the home (H) and foreign (F) states are symmetric. We thus focus our discussion on the home state. As needed, we denote

---

[27]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. Unlike Auclert et al., we have no 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.

variables pertaining to the foreign state by an asterisk.[28]

Households   There is a unit measure of households in each state, 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 credit constrained, has no savings, and consumes their income every period. Households have identical utility over consumption, leisure, and state government services:

$$u(C_t, N_t, G_{st,t}) = \ln C_t - \kappa_N^i \frac{N_t^{1+1/\epsilon_N}}{1 + 1/\epsilon_N} + v((1 - \phi_K)G_{st,t}), \qquad (4.1)$$

where C is an aggregate consumption good, N is labor supply, and $(1 - \phi_K)G_{st,t}$ is state government expenditure on services other than infrastructure. ϕ_K is the share of state spending allocated to public capital. The Frisch elasticity of labor supply, ϵ_N, is common across households. The household's preferences for leisure is governed by κ_N^i; κ_N^i differs by type of household ($i \in \{c, u\}$ for constrained and unconstrained). While state public goods impact household welfare, preferences for $G_{st,t}$ are separable and thus do not affect our positive analysis below.[29]

Households pay proportional federal and state labor income tax rates τ_t^f and τ_t^{st} respectively on their labor income $W_t N_t$, receive transfers Tr_t, and have profit income Pr_t derived from firm ownership. Only unconstrained households can hold nominal bonds B_t or private capital K_t. Households adjust their use of capital services by varying the rate of utilization ν_t, which incurs a resource cost of $\kappa(\nu_t)K_{t-1}$. The price index for both consumption and investment goods is P_t. The budget constraint for unconstrained agents is:

$$P_t(C_t^u + I_t^u + \kappa(\nu_t)K_{t-1}^u) + B_t^u \leq (1 - \tau_t^f - \tau_t^{st})W_t N_t^u + r_t^k \nu_t K_{t-1}^u + B_{t-1}^u R_{t-1}^n + Pr_t + Tr_t^u.$$
$$(4.2)$$

---

[28]Online Appendix C provides a full set of derivations and model equations.

[29]Public investment $\phi_K G_{st,t}$ will have supply side effects through its impact on firm productivity, however.

Unconstrained agents also have access to complete markets, via Arrow-Debreu securities, which are omitted for simplicity. The budget constraint is similar for constrained households, but with $B_t^c = K_t^c = 0$ and without Arrow-Debreu securities. Constrained agents receive transfers Tr_t^c. In our simulations below, transfer payments to stimulate the economy are targeted only towards constrained agents $(dTr_t^c > 0; dTr_t^u = 0)$.

Household demand for consumption and investment is characterized by nested CES preferences over varieties produced at home and abroad. The weight on home goods is ϕ_H, and the elasticity of substitution between home and foreign bundles is η. The price index P_t is the cost-minimizing price index over home and foreign bundles. Capital accumulation is subject to quadratic adjustment costs in the rate of investment.

Firms.   Each state has a measure of intermediate goods producers $z \in [0, 1]$. Each produces its variety using a Cobb-Douglas technology, employing aggregate of utilization-adjusted capital and of labor:

$$y_{h,t}(z) = A_t \times (\nu_t K_{t-1})^\alpha N_t(z)^{1-\alpha}. \tag{4.3}$$

Firms perceive cost shares of capital and labor of α and $1 - \alpha$, respectively. A_t depends on state public infrastructure subject to a congestion externality as in Barro and Sala-I-Martin (1992) and Drautzburg and Uhlig (2015). The equilibrium shares of public infrastructure, private capital, and labor are $\zeta, (1 - \zeta)\alpha$ and $(1 - \zeta)(1 - \alpha)$, respectively. Firms face a constant elasticity of demand (θ) and set prices in monopolistic competition subject to a Calvo-friction: With probability ξ, the firm cannot reoptimize in a given quarter and its prices rise at the rate of trend inflation. Without frictions, firms would set a constant markup $\frac{\theta}{\theta-1}$ over marginal cost.

State governments.   States adjust their transfer payments to households, government consumption and public 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.

Transfer payments to households and local governments are important in state budgets. We assume that states spend a fraction ϕ_{tr} of new IG aid on state transfers, $Tr_{st,t}$.

$$Tr_{st,t} = \psi_{IG}\phi_{tr}\left(\frac{IG_t}{P_t} - \overline{IG}\right) + \overline{Tr}_{st} \tag{4.4}$$

Here, bars indicate steady state values of transfers and IG revenue.

The remaining IG revenue is spent on government consumption and investment $G_{st,t}$, of which a fraction $1 - \phi_{tr}$ goes towards public services, which affects household utility.

$$G_{st,t} = \psi_{IG}(1 - \phi_{tr})\left(\frac{IG_t}{P_t} - \overline{IG}\right) + G_{st,t}^x. \tag{4.5}$$

$G_{st,t}^x$ is exogenous ("pre-transfer") government consumption plus public investment. State demand for $G_{st,t}$ and household demand have the same CES structure, with a home share ϕ_H and price elasticity η. States invest the fraction ϕ_K of $G_{st,t}$ in public capital ($K_{st,t}$), where $K_{st,t}$ evolves as:

$$K_{st,t} = (1 - \delta_G)K_{st,t-1} + \phi_K G_{st,t}. \tag{4.6}$$

δ_G is the rate of depreciation. Public capital (infrastructure) increases A_t for firm production of intermediate goods.

We assume that states adjust distortionary taxes to keep debt stable. As we discuss below, states smooth tax rates, and gradually adjust labor income tax rates in response to their debt burden and level of net expenditure. Denoting trend inflation by $\bar{\bar{\Pi}}$, our baseline tax rule therefore takes the following form:

$$\tau_{st,t} = \rho_\tau \tau_{st,t-1} + (1 - \rho_\tau)(\bar{\tau}_{st} + \psi_{st,b}\left((R_{t-1}^n - 1)B_{st,t-1} - (\bar{R}^n - 1)\frac{\bar{b}_{st}}{\bar{\bar{\Pi}}}P_t\right)$$

$$+ \psi_{st,E}(P_t(G_{st,t} - \bar{G}_{st}) + P_t(tr_{st,t} - \overline{tr}_{st}) - (IG_t - P_t\overline{IG})))$$

$$(4.7)$$

$B_{st,t}$ is nominal state debt, whereas b_{st} is real debt. States change tax rates according to the rate of adjustment ρ_τ, and $\psi_{st,b}$ determines how states adjust tax rates to increased interest cost of outstanding debt. Similarly, $\psi_{st,E}$ determines how states adjust rates to increased expenditures net of IG revenue. Note that federal transfers to states, unlike expenditures and state transfers to households, are in nominal dollars.

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 IG_t and equal across states. The total federal allocation is thus $nIG_t + (1-n)IG_t = IG_t$:

$$(nP_t + (1-n)P_t^*)G_{f,t} + IG_t + Tr_{f,t} + R_{t-1}^n B_{f,t-1} = \tau_t^f(nW_tN_t + (1-n)W_t^*N_t^*) + B_{f,t}$$

$$(4.8)$$

$Tr_{f,t}$ denotes (nominal) federal transfers net of lump-sum taxes. $B_{f,t}$ denotes federal nominal debt. Variables with an asterisk refer to the foreign state.

IG_t transfers follows an exogenous AR(1) process with persistence ρ_{IG}, calibrated for persistence of ARRA spending. The federal government finances its expenditures by taxation of labor income, lump-sum taxes, or temporary borrowing. Federal labor income taxes finance $1 - \gamma^f$ of government consumption and IG transfers every period (out of steady state), and the government levies constant lump-sum taxes (or transfers) to balance the federal budget. 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):

$$R_t^n = \left(\frac{\bar{\bar{\Pi}}}{\beta} \right)^{\rho_r} \left(\left(\frac{\Pi_t^{agg}}{\bar{\bar{\Pi}}} \right)^{\psi_{r\pi}} \left(\frac{Y_t^{agg}}{\bar{Y}} \right)^{\psi_{ry}} \right)^{1-\rho_r}, \qquad (4.9)$$

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

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 federal (defense) spending multipliers, which we take to be 0.8 for surprise spending increases, following Ramey (2011). We also match other moments and parameter estimates from the literature, as detailed below. Importantly, we calibrate the two states to have the estimated partisan propensities to spend IG transfers.

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 Coenen et al. (2012) use in their SIGMA model of the U.S.

Table 4: Calibrated parameters

	Parameter	Value	Source
firms & households	Persistence of nominal prices ξ	0.85	Match defense multiplier (Ramey, 2011)
	Share of credit constrained households μ	0.4	Auclert et al. (2019),Coenen et al. (2012)
	Price elasticity of demand across states η	2	Nakamura and Steinsson (2014)
	Price elasticity of demand within states θ	7	Nakamura and Steinsson (2014)
	Demand share of home state ϕ_H	$\frac{2}{3} + \frac{n}{3}$	Nakamura and Steinsson (2014)
	Cost share of private capital α	0.2	Labor income share of 0.66
	Annual interest rate	4%	Share of investment in GDP
	Share of public capital in production ζ	0.02	Optimal steady state
	Depreciation rate of private capital (annual)	10%	Leeper et al. (2017)
	Investment adjust costs κ_I	5	Leeper et al. (2017)
	Elasticity of capacity utilization $\frac{\kappa''(1)}{\kappa'(1)}$	0.2	Leeper et al. (2017)
	Frisch elasticity of labor supply ϵ_N	2	Leeper et al. (2017)
federal policy	Interest rate smoothing ρ_r	0.75	Galí (2008)
	Reaction to inflation $\psi_{r,\pi}$	1.5	Galí (2008)
	Reaction to output $\psi_{r,y}$	$\frac{1}{8}$	Galí (2008)
	Annual inflation rate $\overline{\Pi}$	2%	Inflation target
	Federal tax adjustment γ_f	0.8	Tax IRF to defense spending
	Federal government consumption and investment $\frac{\bar{G}_f}{\bar{Y}}$	0.12	Data
	Persistence of defense shock ρ_G	0.89	Match duration of IG
states	State tax persistence ρ_τ	0.8	Online Appendix Table C.1
	Reaction of state taxes to debt $\psi_{st,b}$	0.01	Online Appendix Table C.1 & determinacy
	Reaction of state taxes to net expenditure $\psi_{st,E}$	0.1	Online Appendix Table C.1 & determinacy
	State government consumption and investment $\frac{\bar{G}_{st}}{\bar{Y}}$	0.08	Data
	Investment share out of state G_{st}	0.2	Data
	Transfer share out of state spending	0.38	Data
	Republican MPS ψ_{IG}	0	Consistent with Table 2 column (2)
	Democratic MPS ψ_{IG}^*	1.92	Implied by ψ_{IG} and Table 2 column (2)
IG aid	Standard deviation of IG shock ω_{IG}	0.24	2009 IG shock size
	Persistence of IG shock ρ_{IG}	0.89	2009 stimulus duration
	Federal IG transfers $\frac{\overline{IG}}{\bar{Y}}$	0.02	Data

Trade elasticities and the degree of home bias determine the importance of trade adjustments when relative prices change between states. 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 also follows Nakamura and Steinsson (2014) when the home state is of size $n = 0.1$. We adjust the home bias with the size of the state to also match the home bias of the larger state 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 cost share of capital (α) equals .2 and, together with θ implies a labor income share of .66. The share of public and private investment pin down the annual interest rate and, as in Drautzburg and Uhlig (2015), the share of public infrastructure in production. The remaining parameters are taken from Leeper et al. (2017). See Table 4 for details.

Federal policy rules. We calibrate a monetary policy rule as in Galí (2008), with a persistence of $\rho_r = 0.75$, and coefficients on inflation and output of $\psi_\pi = 1.5$ and $\psi_y = \frac{1}{8}$. The steady state inflation rate is 2%.

The federal government adjusts labor tax rates to pay for expenditures, as in Nakamura and Steinsson (2014). But 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 .05pp over the first year following a 1.0% of GDP increase in government spending. We thus assume that the federal government adjusts labor income taxes to pay for a fraction $1 - \gamma_f$ of changes in current expenditures. We calibrate $\gamma_f = .8$ to match the response of tax rates to surprise defense spending shocks in Ramey (2011). For our policy comparison, the persistence of defense spending shocks (ρ_G) is set at .89 to match the duration of IG aid. The steady state share of GDP of federal government consumption and investment ($\frac{\bar{G}_f}{\bar{Y}}$) is set equal to .12.

State policy rules. We estimate state tax policy as a dynamic process with a positive rate of autocorrelation and taxes positively correlated with increases in interest paid on debt and with expenditures net of IG transfers. In contrast, we do not find correlations that suggest states stabilize their budgets throught adjustments in overall expenditures or state transfers.[30] In our baseline revenue rule, we thus use an estimated annual persistence of the tax rate (ρ_τ) equal to .80, converted to a quarterly frequency of 0.95, and scale up the correlations on interest payments and net expenditures by the same factor to achieve

---

[30]See Online Appendix at Table C.1.

determinacy, yielding $\psi_{st,b} = 0.01$ and $\psi_{st,E} = 0.10$. The steady state share of GDP of state government consumption and investment ($\frac{\bar{G}_{st}}{\bar{Y}}$) is equal to .08 with the investment share of spending (ϕ_K) equal to .20 and the state transfer share of spending (ϕ_{tr}) equal to .38.[31]

The central parameters in our calibration of state fiscal policy are the marginal propensities to spend of Democratic and Republican governors, as estimated in column (2) of Table 2. Only the estimated difference between partisan propensities is causally identified, however. We must therefore benchmark one of the two MPSs. We do so by setting the Republican governor's MPS from new IG aid to zero, a result consistent with our point estimates. The resulting MPS for a Democratic governor is therefore 1.92.[32] We designate the home state to be run by a Republican governor, and the foreign state to be run by a Democratic governor; therefore for increases in IG transfers, $\psi_{IG} = 0.0$ and $\psi_{IG}^* = 1.92$.[33] Our analysis will also perform two counterfactuals. First, we eliminate partisan differences in state responses to aid ($\psi_{IG} = \psi_{IG}^* = 1.92$). Second, we vary the share (n) of the national economy run by Republican (n; $\psi_{IG} = 0.0$) or Democratic (1-n; $\psi_{IG}^* = 1.92$) governors to illustrate how shifting partisan control of state governments affects the impact of federal IG aid on the aggregate economy. Otherwise, states are identical and of equal size.[34]

Shocks to federal transfer policy. 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 billion (Carlino and Inman, 2016), or 2.2% of GDP at the time. This yields a shock standard deviation ω_{IG} of $100 \times (1 - \rho_{IG}) \times 0.022 \approx 0.24$: IG

---

[31] The average share of transfers to households and municipalities is 38%. We assume that municipalities spend their transfers on goods that are economically equivalent to consumption of the constrained agents.

[32] Although our estimate is for a year-over-year increase in IG aid, we apply them more broadly to business-cycle increases. This is consistent with Table B.2 in the Online Appendix which shows that the partisan MPS differences are stable for multi-year increases.

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

[34] To quantify the uncertainty surrounding our estimates of partisan differences, we compute confidence intervals using the Delta method, based on the numerical derivative of the model and the asymptotic standard error of the estimated partisan difference.

transfers rise initially by 0.24% of GDP, after a one standard deviation shock. For ease of comparison, we impose the same process for the federal government spending process. The steady state share of IG transfers in aggregate income is .02.

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 the initially partisan Republican governor then behaves as her Democratic counterpart, consistent with our estimated insignificant partisan differences when polarization was at its lower, pre-Reagan era levels. Second, we set the partisan differences at the level prevailing in the Reagan era and vary the share (n) of the national population living in the Republican state to compute how IG transfer multipliers would have changed over time as a function of the changing partisan composition across U.S. governors.

Dynamics following a shock to federal transfers. Figure 4 shows the responses of federal and state fiscal policies and output to a federal IG transfer shock. The three panels on the left of Figure 4 show federal variables, the panels in the center outcomes in the Democratic state, and the panels on the right outcomes in the Republican state. To isolate the causal effect of partisanship identified in the state-level analysis, all panels show two scenarios: First, the solid lines with squares shows the baseline scenarios arising from our partisan differences in the MPS. Second, the dashed lines show the case without partisan estimated differences, when the Republican has the same MPS as the Democratic governor. The difference between these two scenarios is the effect of the identified partisan MPS differences. The thin solid line with the surrounding band shows the point estimate of these differences with its 90% confidence interval.

The top panels in Figure 4 show the IG transfer shock and the state spending responses.

The top left panel shows the increased federal transfers. Transfers initially increase by 0.24% of GDP and have a half-life of six quarters. Since transfers are exogenous, they are the same in both scenarios of partisanship. The top middle panel shows the spending response in the Democratic state. It always has an MPS of 1.92; spending increases by 0.47% of GDP

Figure 4: Impulse Responses: Spending, Taxes and Output.
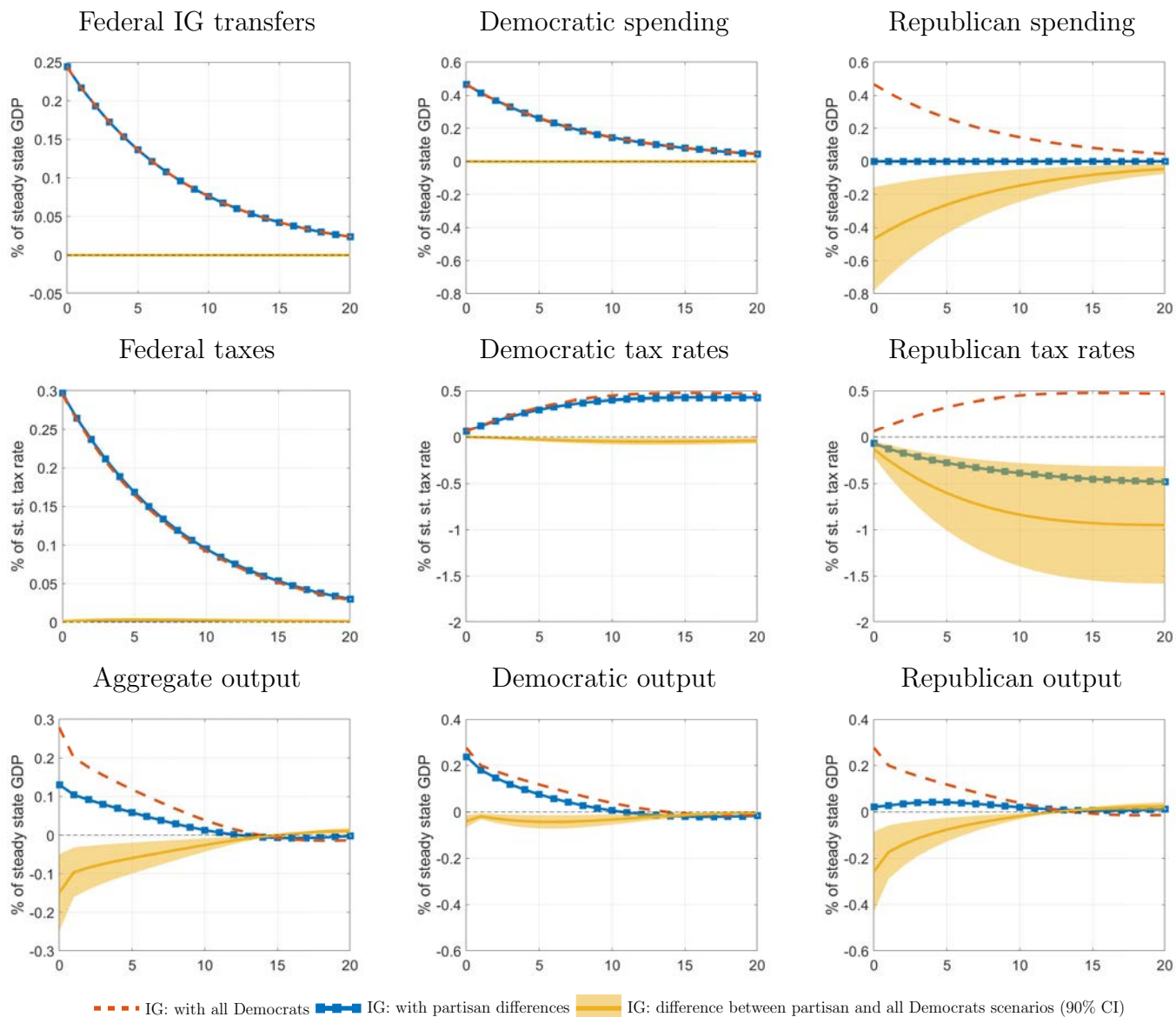
Impulse responses (relative to steady state) to IG transfer shock shown for two scenarios: (1) with partisan differences (solid line with squares) with one state run by a Democratic governor (middle column) and the other by a Republican governor (right column), and (2) when both states have the preferences of Democratic governors (dashed line). The thin solid with its 90th percentile confidence interval as the shaded area shows the difference in responses between the two scenarios.

33

(=1.92×0.244%) on impact. It then declines as the shock to IG aid gradually declines over time; the solid and dashed lines overlap in this case. The top right panel shows the spending response in the Republican state with either the baseline MPS of zero (solid line with squares) or with the counterfactual Democratic MPS of 1.92. Since spending depends only on IG transfers and the MPS, it is zero in the baseline scenario, and equal to that in the Democratic state without partisan differences. The narrow line with its 90% confidence band shows the estimated (negative) partisan effect on spending in the Republican state (top row, right panel). Republican state spending is statistically significantly lower (below zero) than with a Democratic MPS.[35] Given an increase in IG aid, Democrats increase spending.

The center rows of Figure 4 shows the path of taxation. To finance the IG transfers, federal taxes are increased by .3% of their steady state value, or by .09pp in absolute terms; center row, left panel of Figure 4. With an MPS greater than unity in Democratic states, Democratic states must increase state labor income taxes to pay for increased spending above the $1 of new IG aid. As a result, state labor income tax rates must also rise, rising by up to .43% of the steady state rate (or 0.02pp) after 14 quarters and remaining at the new higher rate until after the 20-quarter horizon shown in the graph (solid line with squares; middle row, center panel). The tax increase is only slightly higher when policy in the Republican state changes (dashed line). Since Republican states do not increase spending but rather allocate their increases in IG transfers to tax cuts (given their tax rule), state tax rates fall, shown as the declining line with squares (center row, right panel). However, when the Republican state is assigned the MPS of Democratic states, state labor income taxes will now need to rise, not fall, along the "Democratic" dashed line. Again the estimated partisan difference in tax responses is shown by the narrow line with its 90% confidence band in the center row, right panel. The estimated level of Republican state taxation is shown as significantly lower than that under Democratic policies state and centered at -.95% of steady

---

[35]See Figure C.1 in the Online Appendix for the paths of state government consumption, investment, and transfers. With a Democratic MPS, state government consumption and investment rises by 0.29% of GDP and state transfers rise by 0.18%, adding up to the overall 0.47% increase.
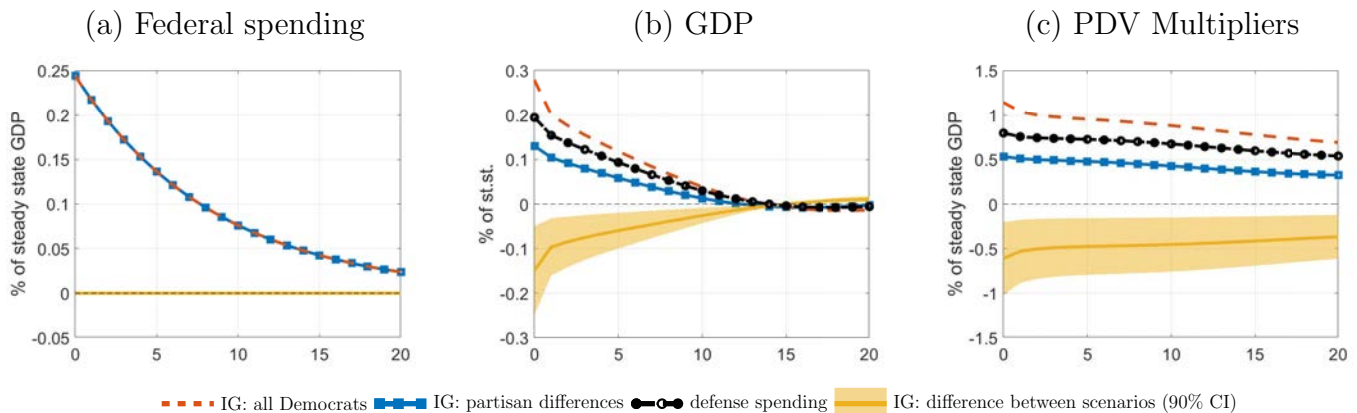
state taxes, after 20 quarters. The shaded 90% confidence interval for this difference is (-.32% to -1.58%). Given an increase in IG aid, Republican states cut taxes.

The difference in fiscal policies across Republican and Democratic states lead to significant differences in the paths of state output and in aggregate national output. The bottom middle panel of Figure 4 shows the path of Democratic state output to rise by .28% on impact and still by .14% after five quarters when only Democratic states raise state spending (solid line with squares). Republican states, which do not raise state spending but instead cut taxes, have much more modest gains in state output, shown as the solid line with squares in the bottom right panel. If, however, Republican states were to spend IG transfers as do Democratic states (dashed line) they would also enjoy significant gains in state output. The end results is significantly lower output gains in Republican states because of their decision to allocate IG transfers to tax cuts rather than spending: Output increases by .26% of GDP less than with a Democratic MPS, with a 90% confidence interval of (-.09%, -.43%).

Finally, note the nation as a whole and also Democratic states enjoy less output gains because Republican states do not spend IG transfers. The higher dashed line in the bottom left panel of Figure 4 shows national output gains if all states spent IG transfers as do Democratic states. It implies an output gain of 0.28% on impact – which would be equally shared by the two states, as the dashed lines in all bottom panels show. National output gains with partisan differences are less than half as high: Output grows by only 0.13% (line with squares; bottom left) on impact. Output is persistently lower with the partisan differences in spending. This is because of calibrating the model to have an impact (defense spending) multiplier of 0.8 and the fact that spending aggregated across the two (equal-sized) states is only half as high with partisan policies. To a lesser extent, there are also spillovers: Output is lower with the partisan spending policies not only in the Republican state, where it is .26% lower (solid line with band; bottom right panel), with a 90% confidence interval of (-.09%, -.43%). It is also lower by .04% in the Democratic state (thin solid line with band; bottom middle panel) as a result of lower spillover demand for its exports.

Comparing multipliers. Given partisan differences, how much does the federal government stimulate the economy for each dollar of new IG aid? And how does the impact of IG aid compare to that of new defense spending as estimated by Ramey (2011)? How does the IG multiplier change as preferences of state policymakers change? We follow Mountford and Uhlig (2009) and analyze present discounted value (PDV) multipliers, defined as the ratio of the PDV of output relative to the PDV of federal transfers. Figure 5 first replicates the federal spending and aggregate output from Figure 4, and then also shows how the resulting PDV multipliers evolve over time. The impact multiplier is just the ratio of the impact GDP response to the impact spending impulse. For longer horizons, the multiplier is the ratio of the discounted sums of the GDP response and the spending impulse for each horizon.

Figure 5: Impulse Responses and Multipliers for IG Transfers and Defense Spending

(a) Federal spending   (b) GDP   (c) PDV Multipliers

- - - - IG: all Democrats   IG: partisan differences   defense spending   IG: difference between scenarios (90% CI)

Panel (a) shows identical paths of federal IG transfer aid and defense spending by quarter. Panel (b) shows the simulated paths for national GDP for (i) defense spending (dark, dashed line with circles), (ii) IG aid with one Republican and one Democratic state (dark line with squares; partisan differences), and (iii) IG aid with Democratic policies in both states (dashed line; no partisan differences). The line thin within panels (b) and (c) show the differences in simulated GDP (panel b) and PDV multipliers (panel c) between the scenarios when states are run by one Republican and one Democratic governor (partisan differences) and when states implement Democratic policies (no partisan differences).

Figure 5 compares the response of the economy to a defense spending shock to responses for an equivalent IG shock. Spending, shown in the left panel, follows the same path for both the shock to IG spending and federal defense spending. The middle panel shows that if policymakers in all states implement Democratic spending policies, aggregate output rises

initially by 0.28% (dashed line). This contrasts with defense spending (dark line with circles), which would lead to an increase of 0.2% on impact. Intuitively, output rises more with IG spending when all states implement Democratic policies, because they spend some funds of their own, in addition to federal spending. However, if half the states are Republican and do not spend from IG aid (solid line with squares), output rises by only 0.13% on impact.

Partisan differences can change the relative effectiveness of different federal policy instruments. We calibrate the defense spending multiplier to be 0.8 on impact (dark line with circles; right panel of Figure 5). If all states follow Democratic spending policies, the aggregate multiplier for IG transfers is 1.14.[36] But with partisan differences – half Republican, half Democratic states – the resulting IG multiplier is 0.53 on impact, 61 cents lower per dollar spent than with no partisan differences (thin line with 90% confidence band). Compared to having all Democratic governors, partisan differences significantly lower the IG multiplier for all 20 quarters as shown. On impact, the 90% confidence interval for this differences ranges from -$.20 to -$1.02. With only Democratic governors, federal IG spending stimulates the economy more than defense spending. But with the partisan differences, shocks to defense spending have a greater multiplier, both on impact and over 20 quarters.

Table 5 compares short-run and long-run multipliers for IG transfers and federal defense spending for three scenarios: (i) when states spend on transfers, consumption, and investment (panel a); (ii) when federal tax rates are adjusted as slowly as state tax rates, so that the federal spending must be initially largely debt financed (panel b), as, for example, in Chodorow-Reich (2019); and (iii) when states spend only on state consumption (panel c). Given our calibration, the federal defense multiplier is, by construction, 0.80 on impact. The IG impact multiplier when all states have Democratic governors (top panel) is higher, at 1.14. However, allowing for partisan differences – again, half Republican, half Democratic states – lowers the impact multiplier to 0.53. With states spending only on government con-

---

[36]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, total government spending including spending by states exceeds the federal government spending. Multipliers larger than unity in our analysis do not imply crowding in of private activity, but rather state activity.

sumption (panel c), the impact IG multipliers is larger, as state government consumption does not discourage work as do transfer payments to constrained households in panel (a).[37] Long-run multipliers are only slightly lower than the impact multipliers.

Table 5: Impact and Long-Run (PDV) Multipliers: Defense Spending vs. IG Transfers.

(a) Baseline

| Multiplier horizon | ZLB duration | IG increase | | | Comparison: |
		Partisan spending	All Democrats	Δ (s.e.) col. (3) − col. (2)	Fed defense C&I
Impact	0	0.53	1.14	-0.61 (0.25)	0.80
Long-run	0	0.51	1.04	-0.53 (0.21)	0.76
Impact	10	0.86	2.03	-1.17 (0.47)	1.24
Long-run	10	0.88	2.06	-1.18 (0.48)	1.27

(b) More Debt Financing

| Multiplier horizon | ZLB duration | IG increase | | | Comparison: |
		Partisan spending	All Democrats	Δ (s.e.) col. (3) − col. (2)	Fed defense C&I
Impact	0	0.72	1.33	-0.61 (0.25)	0.98
Long-run	0	0.71	1.25	-0.53 (0.22)	0.96
Impact	10	0.92	2.08	-1.15 (0.47)	1.30
Long-run	10	0.95	2.11	-1.17 (0.47)	1.33

(c) State Spending on State Consumption Only

| Multiplier horizon | ZLB duration | IG increase | | | Comparison: |
		Partisan spending	All Democrats	Δ (s.e.) col. (3) − col. (2)	Fed defense C&I
Impact	0	0.77	1.61	-0.84 (0.34)	0.80
Long-run	0	0.73	1.48	-0.75 (0.30)	0.76
Impact	10	1.15	2.61	-1.46 (0.59)	1.24
Long-run	10	1.17	2.63	-1.46 (0.59)	1.26

All results based on the calibration of the economy as in Table 4.

Finally, anything that increases the effectiveness of demand side policies in the model or the MPS in the data increases the IG multiplier, and also the importance of partisan differences. Table 5 illustrates this in two ways. First, if we hold the nominal interest fixed for ten quarters (ZLB duration), demand side (spending) policies become more important, and short-run effects of supply side policies less important. The impact of fiscal policy on the

---

[37]Our results are intuitive in the light of our calibration that gives an important role to demand side features of our model, given the "demand equivalence" in Wolf (2020). Of our simulations, the results in Panel (c) comes closest to meeting the assumptions needed for Wolf's equivalence result. States now provide only the consumption good and do not provide public infrastructure and targeted transfers as in panels (a) and (b) with supply side effects. The multiplier differences shown in Panel (c), col. 3 (without ZLB) for partisan spending ($\frac{1}{2}$ Democrats, $\frac{1}{2}$ Republicans) and no partisan spending (all Democrats) are very close to the implied difference from Wolf approximation of -.77: $(= [.5 \times 1.92 + .5 \times 0.0] \times .80 - [1.0 \times 1.92 - 0.0 \times 0.0] \times .80)$.

aggregate therefore rises; see, for example, Christiano et al. (2011).[38] Here, the IG multipliers also rise, as does the importance of partisan differences. For example, it increases from -0.61 to -1.17 on impact in panel (a). Second, all multipliers are larger still when federal fiscal policy is initially debt financed. But because the multiplier here increases due to supply side policies (initially lower taxes), the partisan difference is unchanged.

Figure 6: Share of Republican Governors and Simulated IG Multiplier, 1983–2019.

(a) Republican Share   (b) Impact Multiplier
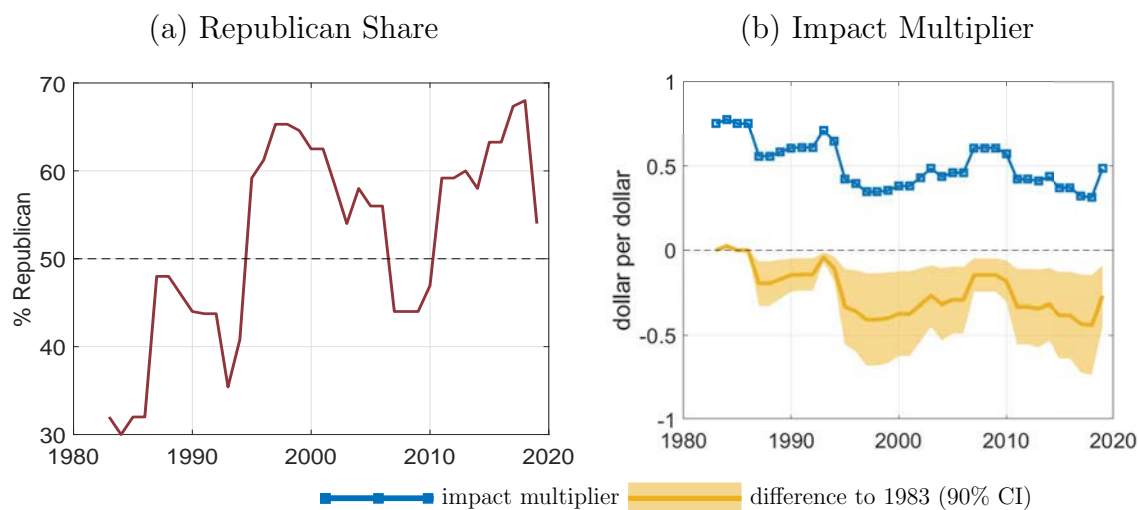
Panel (a) shows the share of Democratic and Republican governors who are Republican, by year. Panel (b) shows the simulated impact multiplier for IG transfers for the baseline specification of Table 4, where the solid dark line traces the value of the simulated aggregate multiplier and the thin line shows the changes in the multiplier as the share of Republican governors increases above its lowest share of 30 percent in 1983. As the Republican share rises from .30 in 1984, the impact multiplier declines, shown as the thin line, largely below zero, and its 90th percentile confidence band for the multiplier's predicted decline.

Our results illustrate the importance for macro policy of knowing who is running the states, Republicans or Democrats. The fraction of states run by Republicans has varied significantly over our sample period; see Figure 6. Panel (a) shows the fraction of states governed by Republicans over the post-Reagan period, omitting the rare independent governors. This Republican fraction ranges from a low of 30% just after Reagan took office to a high of 67% during Trump's presidency. Using these values to calibrate n in our model translates to sizable differences in the impact IG multiplier, shown in panel (b) of Figure 6. The transfer

---

[38]We implement the ZLB 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.

multiplier peaks during the periods of low Republican governorships, specifically during Reagan's first term and Clinton's and Obama's first terms. Allowing for estimation uncertainty of partisan effects yields the confidence interval for the impact multiplier as the shaded band in panel (b). Moving from the low 1983 share to the high 2018 Republican share lowers the multiplier by -.45, with a 90% confidence interval of (-.15, -.75), very similar to the "demand equivalence" estimate of -.51 at the end of Section 3.

5 Model validation in aggregate time series

While state partisan preferences have a significant policy impact in our structural model, it is valuable to see if there is direct statistical evidence for partisan effects as well. We do so by estimating multipliers to IG transfer shocks in a model that includes the share of Republican governors.[39]

We use the surprise component of federal IG transfers as the transfer shock, treating it as exogenous to other current shocks. Identification of the IG shock is similar to the approach proposed by Blanchard and Perotti (2002) for government purchases and adopted to IG transfers in Carlino and Inman (2016): We assume a decision lag in fiscal policy, so that unexpected changes in fiscal policy are contemporaneously unaffected by changes in current GDP. We view this assumption as reasonable, since the NIPA data used here excludes an important automatic stabilizer, the unemployment insurance program. We also include a rich set of controls to ensure that the transfer shocks are unexpected by economic agents.

Our estimating equation applies the local projection approach of Jordà (2005):

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

---

[39]We could also test the analogous prediction that, on impact, Democratic-governed states have higher levels of economic activity – but that after a few quarters, growth in Republican states is higher. In line with this prediction, we estimate in the state panel data that the change in the employment-to-population ratio is lower under marginally elected Republican governors than under Democratic governors: If IG aid increases by 1pp, the employment to population ratio drops by 0.04pp to 0.05pp relative to the Democratic-run state, depending on the specification, see our Online Appendix. Table B.3 columns (1) to (5) for results on current employment changes, and columns (6) and (7) for future employment changes.

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

where $\mathbf{x}_{t-\ell}$ includes lags of GDP, federal expenditures, state and local expenditures, federal tax revenue net of transfers, and IG 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. Since Ramey (2011) and Leeper et al. (2013) have documented the importance of accounting for agents' information set when estimating fiscal multipliers, we use survey expectations to proxy for agents' information: specifically, one-quarter ahead inflation and output growth expectations, one-quarter ahead expectations of both federal and state and local government purchases, and three-quarter ahead government purchase expectations. All expectation measures are from the Philadelphia Fed's *Survey of Professional Forecasters* (SPF). We also interact the expectation measures with the share of Republican governors. We use quarterly data from 1981q1 to 2018q3: It is the longest sample for which all SPF series are available, and largely coincides with our post-Reagan sample. Our focus is on $\beta_{Rep,h}$, an estimate of the response at horizon h of GDP to the IG shock as a function of the share of Republican governors.

Figure 7, panel (a) shows the estimate for the impact of an IG shock worth 1% of GDP on impact changes when the share of Republican governors is one standard deviation above average. Specifically, it shows $\beta_{Rep,h}$ times a 10.8pp higher Republican share (one standard deviation) and converted to a share of GDP, along with the 67% and 90% confidence intervals. As predicted by the structural model, on impact and for the first four quarters afterwards, a surprise increase in IG aid lowers the GDP response significantly when the Republican share is above average. But after two to three years, the GDP effects are estimated to be significantly higher when the Republican share of governors is larger. We also estimate eq. (5.1) with the IG level on the left-hand-side, to compute the corresponding change in the cumulative multiplier; see Figure 7, panel (b). The cumulative multiplier is also estimated to be significantly lower on impact and for the following four quarters when the Republican

41

share of governors is above average. The confidence interval for the cumulative multiplier is centered near zero after about 10 quarters.[40]

Figure 7: GDP response and IG Multiplier by Republican Share, 1981q4–2018q3.

(a) GDP response

(b) Cumulative IG multiplier
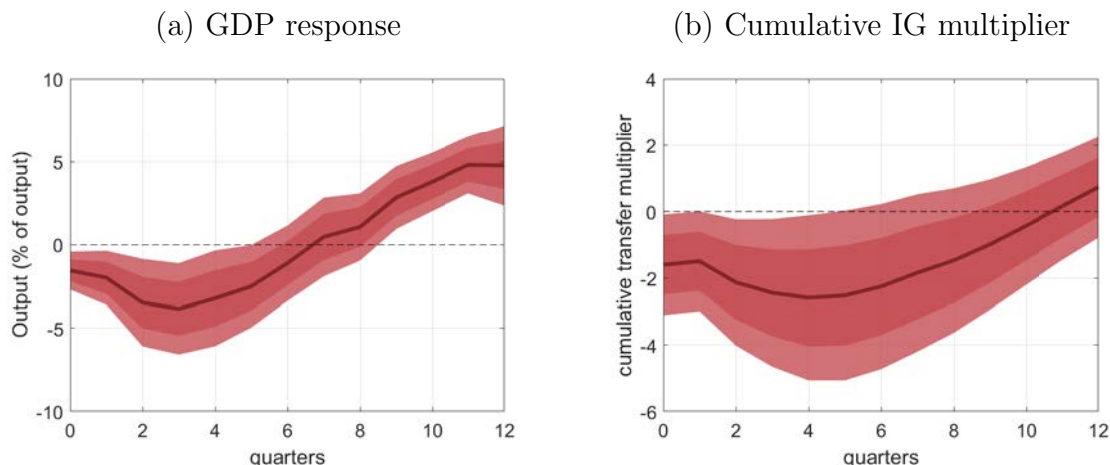
Estimated GDP response and cumulative multiplier (dark lines) and their 67th percentile and 90th percentile confidence bands, based upon Newey-West heteroskedasticity and autocorrelation robust standard errors with two lags above response horizon. Shown are the changes in the IG Transfer multipliers for when the share of Republican governors is increased by one standard deviation (10.8pp) above the sample (1981–2018) average.

The point estimate of the aggregate multiplier difference here is large. Centered near -2 for a 10.8pp. increase in the share of Republican governors, the change is much larger than our model simulations. In the baseline calibration of the model, the change in the impact multiplier is much smaller, only about -0.3 for a 30pp change in the share of Republican governors, see Figure 6. In terms of the time series estimates in Figure 7, this point estimate would be towards the upper end of the 90% confidence interval. The structural model's predicted effect would rise, however, if we calibrated the model with all state spending to be allocated to state consumption, and would about double for a ZLB; see Table 5. Overall, the time series evidence points to a potentially bigger role for partisan differences than in our baseline calibration. If anything, our simulated partisan differences seem a conservative estimate for the importance of partisan politics for the administration of macroeconomic policy through state governments.

---

[40]Table D.1 and Figure D.1 in the Online Appendix show additional coefficient estimates and IRFs, with similar results for more parsimonious specifications in longer samples.

6   Conclusion

While well understood for the implementation of micro-economic policy in fiscal unions, IG aid as a tool for macro-economic stabilization policy has only recently received serious scholarly attention, primarily because of the importance of such aid in the U.S. government's response to the Great Recession. The American Recovery and Reinvestment Act of 2009 allocated $318 billion to state and local governments. The current Covid Recession has also called for significant assistance to state and local government: Over the past year (2020), the U.S. CARES Act plus its Supplemental Covid Relief have jointly allocated $807 billion dollars through state and local governments for increased (state-run) unemployment insurance ($450 billion) and the protection and expansion of state government services ($357 billion). Finally, the European Union in a major break with policy tradition has recently approved an EU funded €750 billion fiscal relief package to be allocated by member states.

States have discretion in how IG aid is finally allocated. State politics, and importantly the partisan preferences of state political leaders, are therefore likely to play a significant role in such spending, specifically whether to state services and transfers or to state tax relief. We have seen that allocation can make a significant difference for the final impact of IG aid on macro-economic performance. Our work provides evidence that because of partisan based state politics, exacerbated by rising national political polarization, Republican governors allocate extra aid almost exclusively to tax relief while Democratic governors choose to (more than) fully spend their increased aid. Our New Keynesian model shows that these partisan choices matter. Because Democratic governors spend their aid and Republican governors provide tax relief, and because the model favors demand side policies over supply side policies for short-term stimulus, the impact multiplier of federal IG aid is significantly larger the greater the share of the nation's governors who are Democrats.

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

References

ADJEMIAN, S., H. BASTANI, M. JUILLARD, F. KARAMÉ, J. MAIH, F. MIHOUBI, G. PERENDIA, J. PFEIFER, M. RATTO, AND S. VILLEMOT (2011): "Dynare: Reference Manual Version 4," Dynare Working Papers 1, CEPREMAP.

ALBOUY, D. (2013): "Partisan Representation in Congress and the Geographic Distribution of Federal Funds," *The Review of Economics and Statistics*, 95, 127–141.

ALESINA, A. AND S. E. SPEAR (1988): "An overlapping generations model of electoral competition," *Journal of Public Economics*, 37, 359–379.

AUCLERT, A., W. S. DOBBIE, AND P. GOLDSMITH-PINKHAM (2019): "Macroeconomic Effects of Debt Relief: Consumer Bankruptcy Protections in the Great Recession," NBER Working Papers 25685, National Bureau of Economic Research.

AUERBACH, A. J., Y. GORODNICHENKO, AND D. MURPHY (2019): "Local Fiscal Multipliers and Fiscal Spillovers in the United States," Working Paper 25457, National Bureau of Economic Research.

AZZIMONTI, M. (2018): "Partisan conflict and private investment," *Journal of Monetary Economics*, 93, 114–131.

BARRILLEAUX, C. AND M. BERKMAN (2003): "Do Governors Matter? Budgeting Rules and the Politics of State Policymaking," *Political Research Quarterly*, 56, 409–417.

BARRO, R. J. AND X. SALA-I-MARTIN (1992): "Public Finance in Models of Economic Growth," *Review of Economic Studies*, 59, 645–661.

BESLEY, T. AND A. CASE (2003): "Political Institutions and Policy Choices: Evidence from the United States," *Journal of Economic Literature*, 41, 7–73.

BESLEY, T. AND S. COATE (1997): "An Economic Model of Representative Democracy," *The Quarterly Journal of Economics*, 112, 85–114.

BLANCHARD, O. AND R. PEROTTI (2002): "An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output," *The Quarterly Journal of Economics*, 117, 1329–1368.

BRUECKNER, M., E. PAPPA, AND A. VALENTINYI (2019): "Local Autonomy and Government Spending Multipliers: Evidence from European Regions," CEPR Discussion Papers 14106, C.E.P.R. Discussion Papers.

CAETANO, C., G. CAETANO, AND J. ESCANCIANO (2017): "Over-Identified Regression Discontinuity Design," Unpublished, University of Rochester.

CARLINO, G., T. DRAUTZBURG, R. INMAN, AND N. ZARRA (2020): "Partisanship and Fiscal Policy in Economic Unions: Health Spending in US States." Federal Reserve Bank of Philadelphia.

CARLINO, G. A. AND R. P. INMAN (2013): "Local deficits and local jobs: Can US states stabilize their own economies?" *Journal of Monetary Economics*, 60, 517 – 530.

———— (2016): "Fiscal Stimulus in Economic Unions: What Role for States?" *Tax Policy and the Economy*, 30, 1–50.

CHODOROW-REICH, G. (2019): "Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?" *American Economic Journal: Economic Policy*, 11, 1–34.

CHRISTIANO, L., M. EICHENBAUM, AND S. REBELO (2011): "When Is the Government Spending Multiplier Large?" *Journal of Political Economy*, 119, 78–121.

COENEN, G., C. J. ERCEG, C. FREEDMAN, D. FURCERI, M. KUMHOF, R. LALONDE, D. LAXTON, J. LINDÉ, A. MOUROUGANE, D. MUIR, S. MURSULA, C. DE RESENDE, J. ROBERTS, W. ROEGER, S. SNUDDEN, M. TRABANDT, AND J. IN'T VELD (2012): "Effects of Fiscal Stimulus in Structural Models," *American Economic Journal: Macroeconomics*, 4, 22–68.

CORREIA, S. (2016): "Linear Models with High-Dimensional Fixed Effects: An Efficient and Feasible Estimator," Working Paper.

DOWNS, A. (1957): "An Economic Theory of Political Action in a Democracy," *Journal of Political Economy*, 65, 135–150.

DRAUTZBURG, T. AND H. UHLIG (2015): "Fiscal Stimulus and Distortionary Taxation," *Review of Economic Dynamics*, 18, 894–920.

FARHI, E. AND I. WERNING (2017): "Fiscal Unions," *American Economic Review*, 107, 3788–3834.

FERREIRA, F. AND J. GYOURKO (2009): "Do Political Parties Matter? Evidence from U.S. Cities," *The Quarterly Journal of Economics*, 124, 399–422.

FERRERO, A. (2009): "Fiscal and monetary rules for a currency union," *Journal of International Economics*, 77, 1 – 10.

GALÍ, J. (2008): *Monetary Policy, Inflation, and the Business Cycle: An Introduction to the New Keynesian Framework*, Princeton University Press, chap. 3.

GALI, J. AND T. MONACELLI (2008): "Optimal monetary and fiscal policy in a currency union," *Journal of International Economics*, 76, 116–132.

GRAMLICH, E. M. (1977): *The political economy of fiscal federalism*, Heath, chap. Intergovernmental Grants: A Review of the Empirical Literature.

HARRINGTON, J. (1992): "The Role of Party Reputation in the Formation of Policy," *Journal of Public Economics*, 49, 107–121.

HINES, J. R. AND R. H. THALER (1995): "The Flypaper Effect," *Journal of Economic Perspectives*, 9, 217–226.

IMBENS, G. W. AND T. LEMIEUX (2008): "Regression discontinuity designs: A guide to practice," *Journal of Econometrics*, 142, 615 – 635, the regression discontinuity design: Theory and applications.

INMAN, R. AND D. L. RUBINFELD (2020): *Democratic Federalism: The Economics, Politics, and Law of Federal Governance*, Princeton University Press.

INMAN, R. P. (2009): "Flypaper Effect," in *The New Palgrave Dictionary of Economics*, ed. by S. N. Durlauf and L. E. Blume, London: Palgrave Macmillan UK.

IVANOVA, I., T. MURPHY, M. STEFANOV, E. RAUCH, M. FIANS, M. GRGURIC, B. JAKOB, R. KAYIBANDA, K. MAKSINEN, A. NYHOLM, D. S. FOLTYNOVA, AND M. ZAGORDO (2017): "Youth unemployment – have EU policies made a difference?" European Court of Auditors Special Report.

JORDÀ, O. (2005): "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95, 161–182.

KAISER FAMILY FOUNDATION (2019): "Medicaid Waiver Tracker: Approved and Pending Section 1115 Waivers by State," https://www.kff.org/medicaid/issue-brief/medicaid-waiver-tracker-approved-and-pending-section-1115-waivers-by-state/.

KLARNER, C. (2015): "State Partisan Balance Data, 1937 - 2011," Harvard Dataverse.

KOUSSER, T. AND J. PHILLIPS (2012): *The Power of American Governors: Winning on Budgets and Losing on Policy*, Cambridge University Press.

LEE, D. S., E. MORETTI, AND M. J. BUTLER (2004): "Do Voters Affect or Elect Policies? Evidence from the U. S. House," *The Quarterly Journal of Economics*, 119, 807–859.

LEEPER, E. M., N. TRAUM, AND T. B. WALKER (2017): "Clearing Up the Fiscal Multiplier Morass," *American Economic Review*, 107, 2409–2454.

LEEPER, E. M., T. B. WALKER, AND S. S. YANG (2013): "Fiscal Foresight and Information Flows," *Econometrica*, 81, 1115–1145.

LOCKWOOD, B. (1999): "Inter-regional insurance," *Journal of Public Economics*, 72, 1–37.

MCCARTY, N., K. POOLE, AND H. ROSENTHAL (2016): *Polarized America: The Dance of Ideology and Unequal Riches*, Polarized America, MIT Press.

MOUNTFORD, A. AND H. UHLIG (2009): "What are the effects of fiscal policy shocks?" *Journal of Applied Econometrics*, 24, 960–992.

MUSGRAVE, R. (1959): *The Theory of Public Finance: A Study in Public Economy*, McGraw-Hill.

NAKAMURA, E. AND J. STEINSSON (2014): "Fiscal Stimulus in a Monetary Union: Evidence from US Regions," *American Economic Review*, 104, 753–792.

NICHOLSON-CROTTY, S. (2004): "Goal Conflict and Fund Diversion in Federal Grants to the States," *American Journal of Political Science*, 48, 110–122.

OATES, W. E. (1972): *Fiscal Federalism*, Harcourt, Brace, Jovanovich.

PELTZMAN, S. (1992): "Voters as Fiscal Conservatives," *The Quarterly Journal of Economics*, 107, 327–361.

PERSSON, T. AND G. TABELLINI (2000): *Political Economics. Explaining Economic Policy*, MIT Press.

POTRAFKE, N. (2018): "Government ideology and economic policy-making in the United States – a survey," *Public Choice*, 174, 145–207.

RAMEY, V. A. (2011): "Identifying Government Spending Shocks: It's all in the Timing," *The Quarterly Journal of Economics*, 126, 1–50.

RAMEY, V. A. AND S. ZUBAIRY (2014): "Government Spending Multipliers in Good Times and in Bad: Evidence from U.S. Historical Data," NBER Working Papers 20719, National Bureau of Economic Research.

RAYMOND, M., L. BOGDANOVICH, D. BRAHMI, L. J. CARDINAL, G. L. FAGER, L. C. FRATTARELLI, G. HECKER, E. A. JARPE, A. VIERA, L. M. KANTOR, AND J. S. SANTELLI (2008): "State refusal of federal funding for abstinence-only programs," *Sexuality Research & Social Policy*, 5, 44.

SHOR, B. AND N. MCCARTY (2011): "The Ideological Mapping of American Legislatures," *The American Political Science Review*, 105, 530–551.

U.S. CENSUS BUREAU (2006): *Government Finance and Employment Classification Manual*, https://www2.census.gov/govs/pubs/classification/2006_classification_manual.pdf.

WASHINGTON POST (2013): "Millions will remain uninsured because of blocked Medicaid expansion in states," November 15.

WITTMAN, D. (1983): "Candidate Motivation: A Synthesis of Alternative Theories," *American Political Science Review*, 77, 142–157.

WOLF, C. (2020): "The Missing Intercept: A Demand Equivalence Approach," University of Chicago.