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The local fiscal multiplier of intergovernmental grants: evidence from federal medicaid assistance to states

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Abstract
Advocates of Medicaid expansion argue that federal Medicaid assistance to states fosters economic activity, generating positive local multiplier effects. Furthermore, during economic downturns, Congress regularly tweaks federal match rates for state Medicaid spending—including during the COVID-19 public health emergency—in order to assist states. Despite heavy reliance on Medicaid funding formulas, identifying the economic effect of these federal transfers has proved challenging. This is because federal Medicaid assistance (to states) is endogenous since funding levels are correlated with unobserved factors driving state economic activity. To address this concern, we construct an instrument based on a nonlinearity in the federal matching rate for state Medicaid spending. Using state-level panel data from 1990 to 2013, we find that federal Medicaid assistance does stimulate economic activity, but the implied cost per job created is quite high, and the multiplier is well below 1. Despite modest economic effects over the entire sample period, we find that federal Medicaid assistance provided powerful fiscal stimulus to states after the Great Recession when the implied multiplier exceeded 1.

Keywords Fiscal multiplier · Fiscal stimulus · Medicaid matching grants

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1 Introduction

Medicaid accounted for more than half of the nearly $800 billion the federal government sent to state and local governments as intergovernmental grants in 2019.[1] It is by far the largest and the fastest-growing means-tested transfer program in the U.S.A, constituting 9.4 percent of federal expenditures. The Congressional Budget Office (CBO) projects that federal Medicaid spending as a share of GDP will increase by 74 percent, from 1.9 percent in 2018 to 3.3 percent in 2047.[2] The program is funded jointly by the federal and state governments, with the federal government reimbursing 50–74 percent of states' Medicaid costs in the form of matching funds based on the Federal Medical Assistance Percentage (FMAP) formula. Furthermore, Congress often passes temporary increases to FMAP in order to send more money to states during periods of economic stress. This includes an across-the-board increase to FMAP of 6.2 percentage points in the Families First Coronavirus Response Act (FFCRA) to extend through the COVID-19 public health emergency.

Research and media reports point to positive spillovers from federal Medicaid assistance (henceforth FMA) on state employment and economic activity.[3] For example, in separate 2014 reports, the President's Council of Economic Advisors touted the effect of federal matching grants on state economies, arguing that the stimulative effects from temporary increases to federal Medicaid reimbursement rates in the 2009 economic stimulus package created or saved millions of jobs (Council of Economic Advisors, 2014a, 2014b).[4] Despite such triumphant pronouncements, there are surprisingly few estimates of the state-level multiplier from FMA, which has been in operation since 1965. Estimating its impact on local economic activity is a challenge because state Medicaid spending, by construction, is inherently endogenous and almost surely driven by local economic conditions.

Among the few papers to explicitly focus on FMA, Chodorow-Reich et al. (2012) and Chodorow-Reich (2019) estimated the local multiplier of a temporary increase in FMAP transfers under the American Recovery and Reinvestment Act (ARRA) during 2009–2010 and addressed the endogeneity by using state-level lagged (pre-recession) Medicaid spending as an instrument.[5] As noted in Chodorow-Reich et al. (2012), a potential concern with the lagged spending instrument is that it still

[1] See https://www.gao.gov/federal-grants-state-and-local-governments.

[2] The pace of growth accelerated further with the Medicaid expansion under the Affordable Care Act (ACA). The CBO estimates that as much as 21 percent of the overall Medicaid funding in 2019 will support adults made eligible because of the ACA Medicaid expansion.

[3] For example, see Kliff (2012) in *the Washington Post*.

[4] In the other case, they touted prospective gains to state economies from increased federal grants associated with expanding Medicaid. Here, they argued that federal Medicaid spending raises worker productivity partly by improving the health of recipients. This notwithstanding, empirical size of local multipliers is far from clear, as it depends on a number of factors, including how the spending is structured, how it is financed, on macroeconomic conditions, and on possible monetary policy responses.

[5] Among other papers, Carlino and Inman (2016) come closest to estimating the multiplier from intergovernmental federal grants, though not specifically from FMA. Estimating SVAR specifications using time series data from 1960 to 2010, they found large and significant multipliers, in excess of 2 at the peak, from targeted welfare aid combining both AFDC and Medicaid.

depends on state Medicaid spending rules which may be correlated with the severity of the downturn.

We propose a new instrument to overcome endogeneity concerns and, in so doing, make three contributions to this literature. First, our instrument is based on a long-standing nonlinearity in the slope of FMAP with respect to relative per capita income (RPCPI), where RPCPI is defined as a moving average of lagged state per capita personal income divided by the corresponding measure for the U.S.A as a whole. FMAP covers a declining share of state Medicaid spending as RPCPI increases before reaching a floor of 50 percent when RPCPI exceeds 1.054, which imparts the nonlinearity (see Fig. 1). We show that the nonlinearity in the FMAP schedule also induces a nonlinearity in per capita federal matching dollars states receive—something also found by Leung (2016) in addressing a different question. Second, rather than estimate the effect of bonus FMAP transfers to states often made during economic downturns, we are the first to estimate the local multiplier from traditional FMAP-based transfers. And third, our use of a long panel of state-level data from 1990 to 2013 allows us to estimate dynamic, time-varying, and long-term estimates of the multiplier, spanning the range of the business cycle.

To preview key findings, our preferred IV estimates indicate that FMA had a modest positive multiplier over the period 1990–2013. We find that an additional \$100,000 per year in FMA yields about 1.9 jobs over three years. This implies a statistically significant employment impact of 0.6 job years at a cost per job of about \$156,000 (in 2016 dollars).[6] However, these aggregate results mask substantial heterogeneity across subsets of years. Over the 1990s, we find that the FMA multiplier was modest with point estimate close to 0.6. We can rule out multipliers significantly exceeding 1. However, the estimated multiplier is substantially larger after 2000, exceeding 1.6 after the Great Recession, with an associated cost per job year of \$73,000. Thus, our multiplier estimates for 2008–2010 are somewhat smaller than those reported by Chodorow-Reich et al. (2012) for the same time period. They estimated a cost of \$26,000 (in bonus FMAP payments) in order to create one job, implying a multiplier of around 2.[7]

Our main identifying assumption is that the FMAP nonlinearity (with respect to RPCPI) is uncorrelated with state and local economic conditions. Thus, we assume that states do not pursue perverse economic policies in order to manipulate their Medicaid match rate, nor do they have enough influence to lead Congress to manipulate the formula. However, states may have a good idea as to what their match rate will be when making policy decisions regarding Medicaid. Thus, a state's location with respect to the FMAP threshold may influence its fiscal policy, including spending on Medicaid.

[6] Alternative specifications for the full time period sometimes yield a much larger, but still very small, jobs multiplier. For example, our full period estimate is a multiplier of 0.54 at a cost of over \$200,000 per job. It should also go without saying that intergovernmental transfers to states serve important purposes beyond creating jobs. And, policymakers will want to consider these other benefits, in addition to multiplier effects.

[7] In another paper, Chodorow-Reich (2019) also uses cross-sectional variation to identify stimulative effects of total ARRA spending. Here, he emphasizes a jobs multiplier of between 1.8 and 2.3 per \$100,000 in additional federal grant at a cost per job year of \$50,000 with an implied multiplier of 1.5.

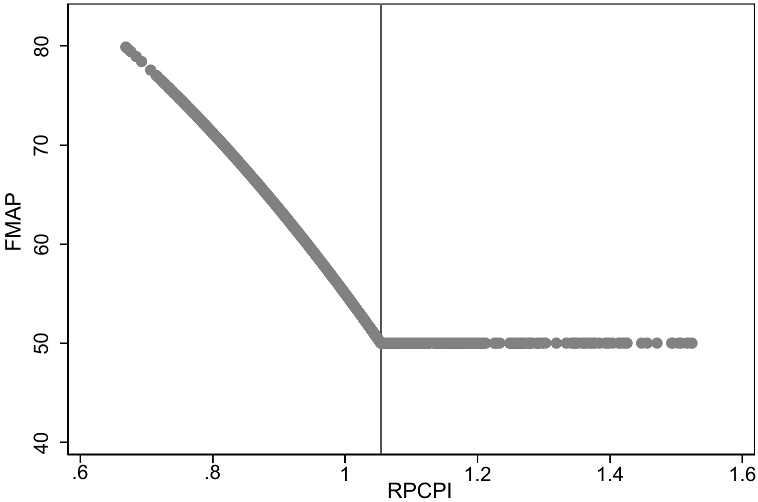

Fig. 1 FMAP formula based on state relative per capita personal income. The figure plots the exact formula-based relationship between Federal Medical Assistance Percentage (FMAP) and the running variable—state's per capita personal income relative to the nation (RPCPI). FMAP equals $1 - 0.45 * \text{RPCI}^2$ and is a declining function of RPCPI for values less than 1.054. FMAP reaches a floor of 50 percent when RPCPI exceeds 1.054, inducing a nonlinearity in FMAP-RPCPI relationship

The remainder of the paper is organized as follows. Section 2 connects our work with the literature on multipliers from intergovernmental grants; Sect. 3 describes the data and details the proposed FMAP instrument; Sect. 4 presents the econometric specification and discusses identification; Sect. 5 reports the results and Sect. 6 concludes.

2 Incentive effects from FMAP and literature on local multipliers

The federal government subsidizes state Medicaid spending via matching grants. The match rate varies across states and over time, but the rate is fixed for a state (in a given year).[8] In other words, the match rate is based on lagged economic variables and is independent of state Medicaid rules. We examine the effects of an increase in a state's Medicaid match rate, which increases the size of the matching grant sent to the state, on state economic activity. The incentive effects to states flow through two different channels. The first channel is an inframarginal effect, where the state receives more federal dollars (than it would have otherwise) for Medicaid spending than it would have absent an increase to its match rate. This inframarginal effect is akin to a lump sum or unrestricted grant to the state. The second channel is a marginal (or behavioral) effect, where the state has an incentive to increase Medicaid spending due to the increased match rate. Increased federal spending from this

[8] In some cases, states may have a different match rate for certain groups, such as those covered by the ACA Medicaid expansion.

second channel contributes directly to an expansion of Medicaid. In contrast to the inframarginal effect, to benefit from the marginal effect, the state must divert funds from other activities toward Medicaid in exchange for increased federal grants.[9] The effects of grants to states will depend partly on the relative importance of these two different channels and thus on the policy under evaluation.

2.1 The inframarginal channel

In examining the effects of the 2009 recovery act (i.e., ARRA), the inframarginal channel is of primary importance. This is because ARRA temporarily increased Medicaid match rates to states, with bigger increases for states that experienced greater increases in their unemployment rates. (Penalties prevented states from cutting Medicaid, while the FMAP bonus was in effect.) As a result, extra federal grants were almost entirely de facto lump sum grants to states. Chodorow-Reich (2019) reviews a number of papers examining the stimulative effects of the temporary FMAP increase, as well as producing new estimates.[10] Chodorow-Reich refers to these as "cross-sectional" multipliers because they measure the effects of targeted spending in states or localities and because cross-sectional variation in treatment is used to identify effects. Identification in these studies is generally based on instruments constructed from pre-recession variables.

Chodorow-Reich employs a cross-sectional approach where the dependent variable is average annual employment (or output) growth over the period of the act (normalized by the state's working-age population). This variable is regressed against a vector of state economic conditions and ARRA outlays, with these variables normalized (where relevant) in the same manner.

He then compares estimates using alternative instruments for ARRA outlays. With respect to job years per a $100,000 increase in spending, estimated multipliers from the four sets of instruments range from 1.8 to 2.2, with a mean of 2.1. Recalibrating these numbers, based on output per worker, yields a mean output multiplier of 1.9. This is in line with seven other papers that he considers, where the mean output multiplier is 2.1—and 1.8 when excluding two studies examining permanent, rather than transitory, spending increases.

2.2 The marginal channel

On the other hand, in analyzing the Medicaid expansion following the 2010 Affordable Care Act (ACA), the marginal channel would be the only relevant factor. For the most part, under the ACA Medicaid expansion, the federal government offered to cover new groups with, at least initially, a 100-percent match rate—and did not provide more funding for groups previously covered by Medicaid. Thus, added funds from ACA would represent federal support for new Medicaid spending and not an

[9] Note that, the first (inframarginal) channel could also result in some increased Medicaid spending.

[10] Also see Wilson (2012) and Conley and Dupor (2013) among papers estimating multipliers from ARRA.

unrestricted grant. While supporters of Medicaid expansion argue that it would be a boon to states, there is little hard evidence to support (or to counter) this contention.

2.3 Blended approach

As an alternative to these two examples, consider an increase (or decrease) to the match rate for a state, where the increase (or decrease) applies both to preexisting state Medicaid spending, as well as to increases in state Medicaid spending. This "blended" case is also policy relevant and is the focus of our analysis. For example, in its 2018 budget options volume, the Congressional Budget Office analyzed the federal revenue implications of three options that would modify Medicaid matching grants to states (Congressional Budget Office, 2018). One of these options involved removing the 50 percent floor on the federal match rate, which binds for some rich states, i.e., they examine the effects of removing the floor. Our analysis evaluates the broader policy implications of just such a policy change.

With respect to methodology, our paper has much in common with Lundqvist et al. (2014), who estimate the effect of intergovernmental grants on employment. Like us, their identification strategy centers on a nonlinearity in the generosity of intergovernmental grants. Unlike us, their focus is not on Medicaid match rates, but rather on a program in Sweden. Their main dependent variable is the per capita number of full-time equivalent employees (by municipality), and their key dependent variable is per capita intergovernmental grants received. As an instrument, they use a dummy variable that indicates whether the municipality is below or above the out-migration threshold that determines eligibility for supplemental internal grants. In contrast to many US-based ARRA papers, Lundqvist et al. report almost no effect of grants on employment.

3 Data and instruments

3.1 State-level panel, 1990–2013

Our analysis is based on panel data for U.S. states spanning the years 1990–2013. The primary dependent variable—per capita jobs—is based on nonfarm payroll employment data from the Current Establishment Statistics (CES) program of the Bureau of Labor Statistics (BLS). We calculate per capita jobs for each state by normalizing the annual average of monthly nonfarm payroll employment from the BLS by state population from the Bureau of Economic Analysis (BEA). Our focus on jobs is driven by the fact that they represents the most robust gauge of economic activity at the state level. Unlike more extensive measures like GDP, which suffer from significant measurement errors, as documented in prior studies on tax and spending multipliers, job figures are measured with greater precision.[11] In addition

[11] See for example, page 1461 in Zidar (2019) and page 19 in Chodorow-Reich (2019).

to the intricacies involved in calculating nominal GDP, there is also a paucity of reliable state-level price indices that can be used to calculate real GDP.

Data on state Medicaid expenditures are included in the Centers for Medicare & Medicaid Services (CMS) data files on "State Health Expenditures by State of Provider." For each state, we use FMAP to compute our key explanatory variable, federal assistance to states for Medicaid, also converted to per capita terms. FMAP data are from the U.S. Department of Health and Human Services (HHS) and data on state and U.S. per capita personal income are from the Bureau of Economic Analysis (BEA). Federal Medicaid dollars and other monetary variables, such as GDP, are expressed in 2016 prices. Data on demographic covariates included in various specifications come from the Current Population Survey (CPS) of the Census Bureau. Table 1 presents summary statistics for key variables.

3.2 Nonlinearity in the federal medicaid funding formula

Recall that our goal is to estimate the effect of an exogenous change to FMA on state employment. However, state employment and FMA are simultaneously determined since both are closely correlated with state PCPI. A floor in the FMAP funding formula implies no relationship between PCPI and FMA once the floor is reached. Here, we provide background on the determinants of federal funding for Medicaid and establish that the floor in the FMAP formula is mirrored by a floor in per capita FMA.

The FMAP formula governs the share of total Medicaid expenditures (appropriated by states) paid by the federal government. The formula is given by

$$\mathrm{FMAP}_{st} = \min\left(\max\left(0.5,\ 1 - 0.45 * \left(\frac{\overline{\mathrm{PCPI}}_{st}}{\overline{\mathrm{PCPI}}_{t}^{\mathrm{US}}}\right)^{2}\right),\ 0.83\right) \tag{1}$$

Central to FMAP is the relationship between state and U.S. per capita personal income (PCPI). In Eq. (1), $\overline{\mathrm{PCPI}}_{st}$ and $\overline{\mathrm{PCPI}}_{t}^{\mathrm{US}}$ represent a 3-year moving averages of PCPI in year t for state s and the U.S., respectively. For year t, PCPI is calculated based on income from years $t-3$, $t-4$, and $t-5$. The formula implies that FMAP is 55 percent if the lagged measure of state PCPI equals the U.S. average (US$\overline{\mathrm{PCPI}}$). Also note that, above the floor, FMAP is inversely related to the RPCPI, i.e., $\frac{\overline{\mathrm{PCPI}}_{st}}{\overline{\mathrm{PCPI}}_{t}^{\mathrm{US}}}$. The FMAP floor of 50 percent imparts a nonlinearity in the match rate when the RPCPI reaches 1.054. In other words, FMAP is greater than 0.5 if the RPCPI is below 1.054, and FMAP equals 0.5 if the ratio exceeds 1.054. In addition to a floor, FMAP also includes a ceiling of 83 percent. However, this ceiling almost never binds.

The nonlinearity in per capita FMA, with respect to state PCPI relative to the U.S., is used to construct our instrument. This is discussed in greater detail in the following section. Here, we present Fig. 2 in order to establish that the FMAP formula does, for a given level of state Medicaid spending, impart a nonlinearity in the level of per capita FMA. Figure 2 represents a stylized state with Medicaid expenditures equal to the 2018 national average of $1,811 per capita. The figure is constructed

Table 1 Summary statistics

	Mean	SD	Min	Max
Outcome var. (cumul per capita jobs)$^{\Psi}$	0.002	0.04	−0.173	0.133
Endogenous var. (cumul. per capita FMA)$^{\chi}$	0.023	0.01	0.007	0.081
IV (norm. RPCPI X Above)	0.029	0.072	0.000	0.394
Running var. (RPCPI—1.054)	−0.089	0.15	−0.385	0.394
Lagged union coverage	14.663	5.986	3.300	31.9
Lagged manuf'g share of GDP	14.869	6.679	0.299	31.467
Lagged pop (millions)	5.463	6.066	0.454	36.961
Lagged GDP per capita	44,884.69	16,902.712	23,904.338	176,245.2
Age 65+	15.835	2.171	6.000	21.666
Female	51.832	1.001	49.319	54.966
White	77.198	15.724	18.155	98.825
Black	10.078	11.087	0.114	67.581
Hispanic	6.698	8.337	0.091	42.834
High school	29.725	9.338	2.041	43.747
Any college	0.48	0.068	0.230	0.632
Jobs/pop	0.464	0.096	0.339	1.208
Medicaid spending per capita	928.071	408.212	223.286	2739.4
FMA/pop	554.927	248.816	111.643	1865.547
Non-Medicaid exp/pop ($100,000)	0.047	0.02	−0.007	0.192
TANF exp/pop ($100,000)	0.001	0.001	0.000	0.004
FMAP	60.087	8.472	50.000	80.18
RPCPI	0.965	0.15	0.669	1.448

Notes The table presents unweighted averages of variables across states. See Eq. (3) for definition of cumulative outcome and endogenous variables

$^{\Psi}$Outcome variable is cumulative 3-year future change in per capita jobs

$^{\chi}$Endogenous variable is cumulative 3-year future per capita FMA

such that, while per capita Medicaid expenditures are held constant, per capita FMA varies across a range of actual PCPI ratios observed for the 50 U.S. states.

Figure 2 shows that the nonlinearity in the FMAP formula does in fact mirror the nonlinearity in per capita FMA. For PCPI ratios exceeding 1.054, the grant hits a floor of $906 per capita. The difference between this floor and the near-linear relationship can be viewed as an exogenous bonus to states that hit the floor. This bonus exceeds $906 per capita for the very richest states since, absent the floor, the very richest states would have a negative FMAP. Note that FMA without a floor would be poorly suited for isolating the effects of federal grants to states. Without the floor, FMA is declining at a smooth rate with respect to state PCPI, which is closely correlated with both the need and the ability of states to finance Medicaid. Thus, the endogeneity cannot be disentangled. However, by comparing the relationship between state economic activity and deviations between actual FMA

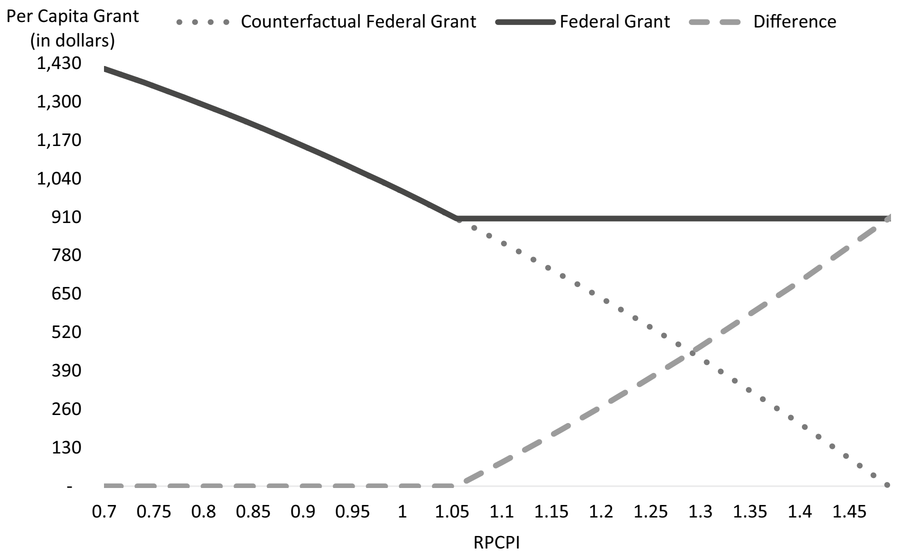

Fig. 2 Stylized per capita federal grant to states. Notes: Calculations are based on a stylized state with Medicaid expenditures equal to the national average of $1,811 (2018) per capita. The federal grant varies based on the actual range of the PCPI ratio for US states. For PCPI ratios exceeding 1.054, the grant hits a floor of $906 per capita. The difference between this floor and the near-linear relationship can be viewed as an exogenous bonus to states beyond the threshold

and counterfactual FMA (i.e., FMA in the absence of the floor), we can isolate the causal effects of the grants on state economic activity.

FMAP has remained largely unchanged since its inception and states have no control over the formula. While the FMAP formula is determined by (lagged) economic activity, the nonlinearity in the formula is exogenous with respect to policy or economic considerations. As detailed in Mitchell (2020), there have been some instances when FMAP has deviated from the formula. For example, FMAP for D.C. is set at 70 percent regardless of how its per capita income compares to that of the nation. Also, as part of the ACA, FMAP increased to 100 percent for newly eligible Medicaid enrollees in states that opted for Medicaid. FMAP was also increased in 2003–2004 to assist states during a slow economic recovery. During the Great Recession, ARRA permitted FMAP to deviate from the usual formula through 2010, with deviations tied to the state unemployment rates. There were also temporary adjustments for Alaska, Michigan, and Louisiana (due to Hurricane Katrina).

In addition to Medicaid funding, FMAP is also used for some much smaller programs: Guardianship Assistance, Child Care and Development Block Grant, Child Care mandatory and matching funds of the Child Care and Development Fund, Foster Care—Title IV-E, Adoption Assistance, and the phased down state contribution or the *clawback* for Medicare—Part D. Additionally, the Children's Health Insurance Program (CHIP) uses enhanced FMAP (E-FMAP) which equals FMAP $+ 0.3 \times (1 - \text{FMAP})$ with a cap of 85 percent. Thus E-FMAP also has a discontinuous slope with respect to *RPCPI* at the same place as FMAP.

4 Econometric framework and identification

4.1 Econometric framework

To estimate impulse responses with panel data, we follow the local projections (LP) approach with instrumental variables employed in influential work on government spending multipliers by Ramey and Zubairy (2018) and Chodorow-Reich (2019).[12] The LP approach is an alternative to vector autoregression (VAR), where a system of equations is estimated, and then impulse responses are produced by, for example, shocking the error term of one of the equations and then projecting forward. LPs are simpler in that they do not require multiple equations.[13] Jordà notes additional advantages of the LP approach, stating that LPs "are robust to misspecification of the data generating process; and they easily accommodate experimentation with highly nonlinear specifications that are often impractical or infeasible in a multivariate context." LPs are based on "projections local to each forecast horizon." By contrast, Jordà demonstrates that VAR-based impulse responses are more susceptible to biases as the forecast period increases—for example, resulting from the linear extrapolation of nonlinear phenomena.

Following Chodorow-Reich (2019), the h-period impulse response of FMA dollars on employment can be estimated using the following specification:

$$y_{s,t+h} - y_{s,t} = \beta_1^h \mathrm{FMA}_{st} + \lambda_1^h \widetilde{\mathrm{RPCPI}}_{st} + \gamma^h X_{st} + \mu_t^h + \alpha_s^h + u_{s,t+h}, \tag{2}$$

where $y_{s,t+h} - y_{s,t}$ is the h-period change in per capita jobs for state s in year $t+h$; FMA_{st} is the state's federal reimbursement for Medicaid spending in per capita terms and based on FMAP. It is worth noting that this multiplier specification, with h-period change in measures of economic activity on the left hand side, is almost identical to Eq. (1) of both Chodorow-Reich (2019) and Jordà and Taylor (2016). To exploit the nonlinearity in FMAP with respect to *RPCPI* as a source of identification, we augment the conventional specification with normalized *RPCPI* ($\widetilde{\mathrm{RPCPI}} = \mathrm{RPCPI} - 1.054$), i.e., normalized RPCPI that equals zero when $\mathrm{RPCPI} = 1.054$; μ_t^h are year effects; and X_{st} consists of other controls that may be correlated with both FMA_{st} and $y_{s,t+h} - y_{s,t}$; α_s^h is an unobserved state effect that may be correlated with other covariates. The LP methodology involves regressing $y_{s,t+h} - y_{s,t}$ on the right-hand side variables of (2) for a set of time horizons, h. In this LP framework, β^h captures the impulse response of one unit of initial shock to FMA_{st} on the outcome variable in period h.

Guided by econometric specifications in Chodorow-Reich et al. (2012) and Chodorow-Reich (2019), we include a set of variables (X_{st}) in our baseline specification to control for the economic and demographic composition of states. These variables include the share of state employment that is unionized, share of manufacturing in state GDP, state population to control for states' size, and per capita real GDP.

[12] LP approach was originally proposed in Jordà (2005).

[13] While a system of equations is not required, more than one equation may be required if an instrumental variables approach is used to address endogeneity issues.

These variables are lagged one year to avoid endogeneity. To account for trends in economic activity that may be correlated with FMA_{st}, we include year affects. We additionally control for state-level demographic covariates: the share of state population: over age 65; female; white non-Hispanic; black; Hispanic; with a high school diploma; and those with college education. We also show that the estimates from our baseline specification are robust to an expanded set of covariates that include Census division dummies and division-by-year effects. And finally, while we do not include lags of the dependent variable in our baseline specifications due to well-known problems with lagged dependent variables in unobserved effects panel data models, we show that the results are highly robust to the inclusion of such lags.

As discussed in Ramey and Zubairy (2018), in a dynamic environment, the multiplier has many definitions depending on the timing and scope of output response and/or the spending shock. We estimate the cumulative version of the multiplier—the response of H-year integral of change in per capita jobs to the H-period integral of per capita FMA_{st}, estimating the following specification:

$$\sum_{h=0}^{H} \left[y_{s,t+h} - y_{s,t}\right] = \beta_1^H \sum_{h=0}^{H} \mathrm{FMA}_{st+h} + \lambda_1^H \widetilde{\mathrm{RPCPI}}_{st} + \gamma^H X_{st} + \mu_t^H + \alpha_s^H + u_{s,t+H}, \tag{3}$$

where β_1^H denotes the H-year cumulative jobs impact. This cumulative multiplier specification is almost identical to Eq. (3) of Chodorow-Reich (2019), who used 24-month integral of h-period change in jobs (normalized by working age population) on the left hand side and the total of 24-month ARRA outlays from December 2008 to December 2010 (normalized by population) as the measure of spending on the right hand side. It is also worth noting that the left hand side of this specification is slightly different from Ramey and Zubairy (2018), who used the H-year integral of GDP level (relative to trend) rather than the integral of GDP changes.

We show that our estimate of the implied per-year jobs response to per-year FMA_{st} (i.e., β_1^H/H) is remarkably robust to the horizon H, so we set H to 3 in most specifications, unless indicated otherwise.[14] Noting that $\sum_{h=0}^{H} \mathrm{FMA}_{st+h}$ is in \$100,000 per capita, we calculate the implied cost per job year as $\$100,000/(\beta_1^H/H)$ and, following the suggestion in Chodorow-Reich (2019), also back out the implied multiplier by dividing an estimate of output per job with the implied cost per job. We estimate output per job as the average GDP across states divided by the average nonfarm payroll jobs over the estimation sample, which yields an output per job of \$108,700 in 2016 prices.

As previously noted, $\sum_{h=0}^{H} \mathrm{FMA}_{st+h}$ is likely correlated with α_s^h and $u_{s,t+h}$, making OLS estimates, with or without fixed effects, biased and inconsistent. In addition to being correlated with a variety of economic and demographic factors, FMA is also both a cause and an effect of state economic activity. For example, all else equal, lower-income states with slower income growth will tend to receive more FMA than more prosperous states. Furthermore, as noted in Chodorow-Reich et al. (2012), $\sum_{h=0}^{H} \mathrm{FMA}_{st+h}$ is almost surely correlated with the intricacies of state-specific

[14] Note that β_1^H in the numerator of β_1^H/H is β_1 with superscript H and should not be confused with power H.

Medicaid spending rules, which end up in $u_{s,t+H}$ because Medicaid spending tends to be countercyclical, and if such state-specific Medicaid rules are serially correlated, even the use of lagged Medicaid spending would not mitigate the endogeneity problem (Bellemare et al., 2017).

Our instrument is motivated by the approach in Lundqvist et al (2014) in the context local multipliers. The nonlinearity in $\sum_{h=0}^{H} \mathrm{FMA}_{st+h}$ with respect to RPCPI is driven by the well-known nonlinearity in the FMAP formula, which occurs when RPCPI equals 1.054: FMAP is greater than 0.5 if RPCPI is under 1.054 and FMAP equals 0.5 if RPCPI exceeds 1.054. Now let *Above* denote an indicator variable, $1(\widetilde{\mathrm{RPCPI}} > 0)$, for RPCPI being above the 1.054 threshold. Using the interaction term $\widetilde{\mathrm{RPCPI}} \times \mathrm{Above}$ as an instrument for $\sum_{h=0}^{H} \mathrm{FMA}_{st+h}$, the first-stage relationship can be written as[15]:

$$\sum_{h=0}^{H} \mathrm{FMA}_{st+h} = \gamma_1^H \widetilde{\mathrm{RPCPI}}_{st} + \eta_1^H \left[\widetilde{\mathrm{RPCPI}}_{st} \times \mathrm{Above} \right] + \delta^H X_{st} + \lambda_t^H + \gamma_s^H + v_{st+H} \tag{4}$$

The coefficient on the instrument, η_1, measures the nonlinearity in FMA_{st} with respect to RPCPI when the FMAP hits the floor ($\widetilde{\mathrm{RPCPI}} = 0$). If it is indeed a valid instrument, then the second stage becomes:

$$\sum_{h=0}^{H} \left[y_{s,t+h} - y_{s,t} \right] = \beta_1^H \widehat{\sum_{h=0}^{H} \mathrm{FMA}_{st+h}} + \beta_3^H \widetilde{\mathrm{RPCPI}}_{st} + \gamma^H X_{st} + \mu_t^H + \alpha_s^H + u_{s,t+H} \tag{5}$$

In both (4) and (5), we assume that conditional on a rich set of covariates, the unobserved state effects are uncorrelated with the instrument and $\sum_{h=0}^{H} \mathrm{FMA}_{st+h}$, respectively, and use pooled OLS.

As discussed at length later, this is because the instrument has little time variation within states. This is consistent with the recent literature on geographic cross-sectional multiplier, which is identified primarily from cross-sectional variation (Chodorow-Reich et al., 2012; Lundqvist et al., 2014; Serrato & Wingender, 2016; Chodorow-Reich, 2019).[16] We recognize that the presence of state-specific unobserved effect would induce serial correlation in the composite error term$\alpha_s^h + u_{s,t+h}$—a concern we address by clustering all standard errors at the state level. Additionally, we throughout estimate regressions unweighted by variables representing states' size as the objective is to get causal estimates of the jobs impact of FMA rather than nationally representative quantities.

4.2 Identification

The key identifying assumption is that the instrument, $\widetilde{\mathrm{RPCPI}} \times \mathrm{Above}$, is correlated with FMA_{st}, it has no direct correlation with $y_{s,t+h}$. In other words, the

[15] Note that using normalized RPCPI in this equation is simply for convenience, as the regression is numerically equivalent to one in which $\widetilde{\mathrm{RPCPI}}$ is replaced with RPCPI.

[16] See Nakamura and Steinsson (2014) for an alternative empirical approach using panel data.

location of the FMAP threshold is effectively exogenous, and states are unable to manipulate their location around the threshold. The ability to manipulate their location would imply that the nonlinearity itself is endogenous and, therefore, an invalid instrument. Informal tests indicate that RPCPI evolves continuously around the threshold in the FMAP formula (McCrary, 2008). This is hardly surprising, as RPCPI for state s in year t is calculated using personal income data from years $t-3, t-4$, and $t-5$, which is to say that they are several years old.

The primary identification condition that $\widetilde{\mathrm{RPCPI}} \times \mathrm{Above}$ is uncorrelated with the error term $u_{s,t+h}$, remains fundamentally untestable in the just identified case. However, we do conduct an informal test for the nonlinearity in other covariates potentially correlated with economic activity. Table 2 reports p values on the test of significance of $\widetilde{\mathrm{RPCPI}} \times \mathrm{Above}$ for several covariates, including two state-level spending measures—per capita TANF spending and per capita non-Medicaid spending. Notably, for the two non-Medicaid spending measures reported in the first two rows, the null hypothesis that no nonlinearity exists (at the location of the threshold in the FMAP formula) cannot be rejected. Overall, Table 2 shows that the presence of a significant nonlinearity among covariates is an exception rather than the norm. A few covariates out of multiple being tested would turn out to be significant simply by chance. When we adjust the p values for multiple testing, none of them remain significant. Even as Table 2 shows little evidence of nonlinearity across other covariates, Fig. 3 shows visual evidence of nonlinearity in the slopes with respect to RPCPI for both the endogenous variable (FMA) and the outcome variable (jobs).

Furthermore, in Appendix Fig. 6, we show that per-capita FMA does not predict lags of employment growth. We plot IV estimates from IV regressions of lags and leads of job growth on per-capita FMA and show that per-capita FMA predicts only current and future job growth, and IV estimates for lagged job growth are not statistically different from zero. In reduced form regressions, we also found that our instrument predicts only current and future job growth, not lagged job growth. Nonetheless, as an extra precaution and to guard against potential correlations with any such covariates, we control for a large set of covariates in our main results (presented in the next section).

5 Results

5.1 Full sample results

Table 3 includes regression results that examine the validity of our identifying assumptions. We control for year effects as well as state-level economic and demographic covariates (including $\widetilde{\mathrm{RPCPI}}$) for the full sample from 1990 to 2010.[17] While the instrument, $\widetilde{\mathrm{RPCPI}} \times \mathrm{Above}$, is uncorrelated with non-Medicaid spending in column (1), it is correlated with Medicaid spending in column

[17] Note that, while our data extend to 2013, the latest base year used in our analysis is 2010, since some variables include information from the three years following the base year.

Table 2 *P*-Values from bivariate regressions of other covariates of instrument

Non-medicaid spending/pop	0.7211
TANF spending/pop	0.4290
Lagged union coverage	0.1028
Lagged manuf'g share of GDP	0.9472
Lagged pop (millions)	0.0504
Lagged GDP per capita	0.3008
Age 65+	0.2272
Female	0.0030**
White	0.3733
Black	0.0810
Hispanic	0.6661
High school	0.5522
Any college	0.0948

Notes: *$p<0.10$, **$p<0.05$. The reports p-values on the test of significance of $\widetilde{\text{RPCPI}} \times$ Above from a simple regression of the indicated covariate as the dependent variable on the running variable $\widetilde{\text{RPCPI}}$ and the interaction term $\widetilde{\text{RPCPI}} \times \textit{Above}$

(2), with an estimated coefficient that is significant at a 10-percent level. Furthermore, the first-stage estimated coefficient of 0.052, reported in column (3), shows a strong statistically significant relationship with the endogenous variable—federal Medicaid dollars ($\left(\sum_{h=0}^{3} \text{FMA}_{st+h}\right)$. Column 4 of Table 3 presents the reduced form coefficient from regressing the key outcome variable, $\sum_{h=0}^{H} \left[y_{s,t+h} - y_{s,t}\right]$, on the instrument, along with other controls. The reduced-form coefficient of 0.078 is also precisely estimated. By definition, the IV estimate of 1.5 equals the ratio of the reduced form coefficient to the first-stage coefficient. That is, a $100,000 injection of FMA in states through the traditional FMAP formula is associated with 1.5 added jobs over 3 years, for a per-year job estimate of around 0.5, at a cost per job of nearly $200,000 over the entire sample period from 1990 to 2010.

Continuing with the full sample results, Table 4 compares IV estimates with simple OLS estimates and examines the sensitivity of the estimates to the inclusion of state fixed effects in our IV specifications. The OLS estimate in column (1) is consistent with a downward endogeneity bias. This estimate more than doubles in size when accounting for state fixed effects in column (2). This suggests substantial omitted variable bias with simple OLS due to unobserved state-level characteristics. IV estimates from column 3, without fixed effects, are similar to fixed effects OLS estimates, suggesting that the instruments help mitigate the bias from omitted state-specific factors. The first stage Kleibergen-Paap rk Wald F-statistic of 9.06, reported in column (3), shows that the instrument is sufficiently strong and the null of weak instruments is strongly rejected, with an associated p values of 0.004.

However, IV estimates are imprecise when state fixed effects are included in column (4) and are no longer reliable as the first stage F-statistic drops to a level

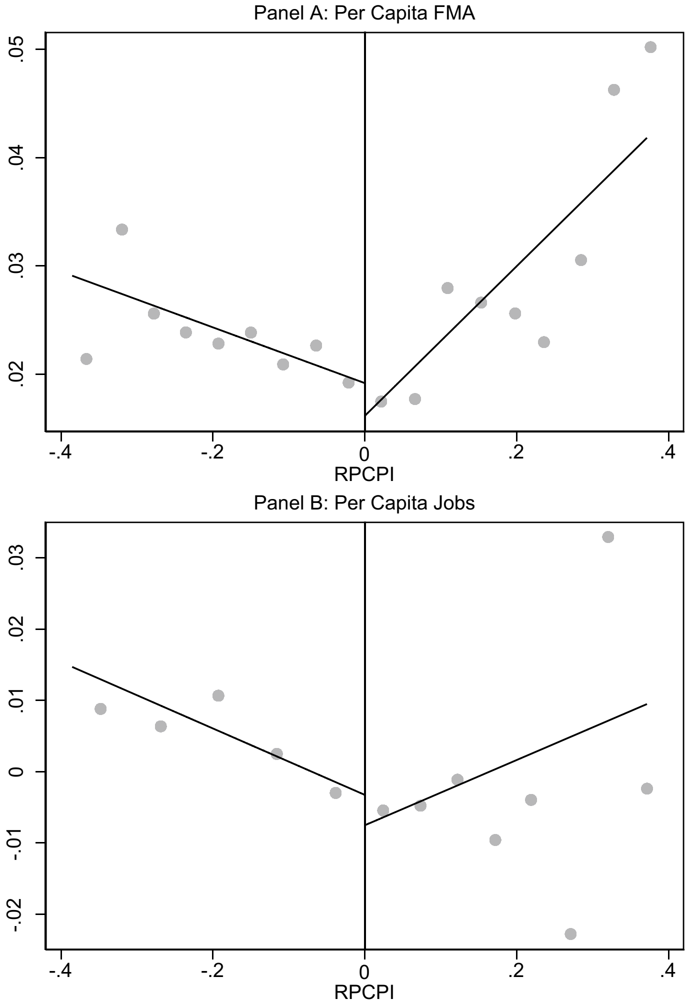

Fig. 3 Slope Nonlinearity. The figure plots binned sample means with the underlying linear fit on either side of RPCPI normalized to equal zero at FMAP threshold. The number of bins is selected using IMSE-optimal evenly-spaced method proposed in Calonico et al. (2014a, 2014b) for the full sample

that suggests (very) weak instruments. This is not entirely surprising because the instrument, RPCPI × Above, exhibits little time-series variation within states. This is because 80 percent of states have RPCPI that is always either above or below the FMAP threshold (at 1.054) for the entire sample period. Even RPCPI has limited within-state variation over time—the within standard deviation is just 18 percent of the overall standard deviation. Given the lack of within variation, the fixed effects IV models are not very informative. Therefore, like Chodorow-Reich et al. (2012), Chodorow-Reich (2019), and Lundqvist et al. (2014), identification in our case relies primarily on cross-sectional variation in the instrument,

Table 3 Basic full sample estimates

	(1) Non-medicaid spending	(2) Medicaid spending	(3) FMA	(4) Jobs
RPCPI × Above	0.038	0.012*	0.052**	0.078**
	(0.037)	(0.007)	(0.017)	(0.030)
Observations	1000	1068	1068	1068
R-Sq	0.501	0.692	0.694	0.714

Notes: *$p<0.10$, **$p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990–2010. Data sources include BLS, CPS, BEA, CMS, and HHS. All dependent variables are in per capita terms. Dependent variables for the first stage regression in column (3) and for the reduced form regression in column (4) are both calculated as 3-year cumulative. All regressions control for the running variable (RPCPI); year effects; first lag of share of union workers, share of manufacturing in state GDP, state population, and per capita real GDP; and demographic covariates–share of state population over age 65; female; white non-Hispanic; black; Hispanic; with a high school diploma; and those with any college education. See Eq. (2)

Table 4 Sensitivity of OLS and IV to fixed effects (Dependent Variable: Per Capita Jobs)

	(1) OLS	(2) OLS with fixed effects	(3) IV	(4) IV with fixed effects
Per Capita FMA	0.671**	1.676**	1.485**	12.715
	(0.265)	(0.794)	(0.571)	(12.028)
Observations	1068	1068	1068	1068
R-Sq	0.719	0.784	0.704	0.299
First stage *F*			9.058	0.629
Underid *P*-val			0.004	0.428

Notes: *$p<0.10$, **$p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990 to 2010. Data sources include BLS, CPS, BEA, CMS, and HHS. Dependent variable is cumulative 3-year future change in per capita jobs; the endogenous variable is cumulative 3-year future per capita FMA; for IV models, instrument is the product of RPCPI and a dummy variable for RPCPI exceeding the FMAP threshold of 1.054 (RPCPI X 1(RPCPI > 1.054)). All regressions control for running variable (normalized RPCPI); year effects; first lag of share of union workers, share of manufacturing in state GDP, state population, and per capita real GDP; and demographic covariates–share of state population over age 65; female; white non-Hispanic; black; Hispanic; with a high school diploma; and those with any college education. See Eq. (2)

relying on the assumption that conditional, on an extensive set of covariates, RPCPI × Above is uncorrelated with any remaining unobserved shocks to current or future economic conditions.

5.2 Robustness

Table 5 assesses the robustness of our estimates to the inclusion of covariates, providing insight into the degree of omitted variable bias in our IV estimates. Column

Table 5 IV Estimates of jobs impact of FMA: robustness across covariates (Dependent Variable: Per Capita Jobs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Per Capita FMA	0.873**	0.660**	0.620*	1.485**	1.916**	2.054**	1.861**
	(0.234)	(0.230)	(0.328)	(0.571)	(0.601)	(0.670)	(0.598)
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Economic	No	No	Yes	Yes	Yes	Yes	Yes
Demographic	No	No	No	Yes	Yes	Yes	Yes
Region FE	No	No	No	No	Yes	Yes	Yes
Region *X* year FE	No	No	No	No	No	Yes	Yes
Y Lags	No	No	No	No	No	No	Yes
Observations	1068	1068	1068	1068	1068	1068	1068
Cost/Job Year (000)	344	454	484	202	157	146	161
Implied multiplier	0.316	0.239	0.225	0.538	0.694	0.744	0.674
R-Sq	NA	0.676	0.686	0.704	0.715	0.793	0.810
First stage F	7.608	7.913	14.699	9.058	11.152	8.772	8.989
Underid P-val	0.030	0.025	0.001	0.004	0.007	0.011	0.012

Notes: $^{*}p<0.10$, $^{**}p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990–2010. Data sources include BLS, CPS, BEA, CMS, and HHS. The dependent variable is cumulative 3-year future change in per capita jobs; the endogenous variable is cumulative 3-year future per capita FMA; for IV models, instrument is the product of RPCPI and a dummy variable for RPCPI exceeding FMAP threshold of 1.054 (RPCPI X 1(RPCPI > 1.054)). All regressions control for running normalized RPCPI; economic covariates include first lag of share of union workers, share of manufacturing in state GDP, state population, and per capita real GDP; and demographic covariates–share of state population over age 65; female; white non-Hispanic; black; Hispanic; with a high school diploma; and those with any college education

(1) starts with the most parsimonious specification, with $\widetilde{\text{RPCPI}}$ as the only control in the IV regression. This barebones specification yields a precisely estimated coefficient of 0.87. We see a decline in the estimate with the inclusion of year effects in column (2), suggesting that other macroeconomic drivers of economic activity absent from column (1) were positively correlated with FMA. The addition of other state-level economic and demographic covariates in columns (3) and (4) does have an important effect on the size of the estimated coefficient, underscoring the need for their inclusion in the baseline specification. To account for any unobserved shocks by region or by region and time, column (5) includes Census division dummies, and the specification in column (6) contains interactions of these dummies with year fixed effects. Finally, column (7) shows that the estimates are robust to including three lags of job growth. From columns (5) to (7), coefficient estimates are largely stable and not statistically different from each other; therefore, the more parsimonious model in column (5), which relies on the full sample, is our preferred model and serves as the default specification unless otherwise indicated.

Stimulative effects of government spending are known to be larger when the economy is sluggish (or there is greater excess capacity). We include appendix tables documenting this analysis. In Appendix Table 8, we examine the heterogeneity in estimates with respect to state economic conditions using the state unemployment rate as a proxy. We augment the preferred speciation with the interaction between per capita FMA and an indicator for high unemployment (rate exceeding 9 percent). Consistent with previous research, we find that the estimated coefficient on the interaction term is positive, though imprecisely estimated and not statistically different from zero.

In Appendix Table 8, we also investigate the potential differences in the impact of federal Medicaid spending based on how states administer the program. Most states use managed care organizations, but some do not. To explore heterogeneity, we supplement the preferred specification with an interaction between per capita FMA and the share of states' Medicaid expenditure in managed care. Even though managed care programs may be economically beneficial, the results are not statistically significant, suggesting that managed care programs are not significantly more effective than other alternatives in stimulating the economy. The imprecision may be due to weak instruments, as the variation in FMA primarily represents differences in lump sum grants rather than variation in state Medicaid spending. In contrast, the instrument incorporates exogenous variation in aid to states for Medicaid support.

We examine the robustness with respect to alternative proxies for economic activity in Appendix Table 9. Results corroborate our earlier finding that the multiplier was modest over the entire sample period. Because the two variables are in dollars, the coefficients represent the implied multiplier. Similarly to the employment findings, the results indicate a small multiplier in the 1990s and a larger multiplier in the 2000s. We still prefer the multiplier estimates using state-level employment as the outcome variables because, as noted before, state-level GDP and income measures come with significant measurement errors, especially in real terms.

5.3 Sensitivity over time

The sensitivity of estimates over time is presented in Table 6, with column (1) reproducing the full sample results from the preferred specification. Column (2) restricts the sample to the 1990s, which reduces the coefficient substantially relative to the full sample estimates in column (1). The implied cost per job increases from $156,000 for the full sample to $234,000 for the 1990s, with the implied multiplier declining from 0.69 to 0.42. FMA appears to have done little to stimulate state economies during the 1990s expansion. However, the effect for the early 2000s was much bigger, with the coefficient jumping to 2.2 in column (3). Even this much higher estimate implies a still modest multiplier of 0.84. Column (4) suggests that the stimulus from FMA to states was strongest in the aftermath of the Great Recession from 2008 to 2010.

5.4 Discussion

Implied multipliers mirror estimated coefficients discussed earlier—they are small in the 1990s, larger in the 2000s, and quite substantial only during the three years after

Table 6 IV Estimates of jobs impact of FMA: by year (Dependent Variable: Per Capita Jobs)

	(1) 1990–2010	(2) 1990–1999	(3) 2000–2007	(4) 2008–2010
Per Capita FMA	1.916**	1.281*	2.214*	4.078
	(0.601)	(0.708)	(1.158)	(3.185)
Observations	1068	510	408	150
Cost/Job year (000)	156	234	135	73
Implied multiplier	0.694	0.424	0.844	1.652
R-Sq	0.715	0.545	0.678	0.750
First stage *F*	11.152	16.535	5.723	2.374
Underid *P*-val	0.007	0.001	0.047	0.162

Notes: $*p<0.10$, $**p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990 to 2010. Data sources include BLS, CPS, BEA, CMS, and HHS. The dependent variable is cumulative 3-year future change in per capita jobs; the endogenous variable is cumulative 3-year future per capita FMA; for IV models, the instrument is the product of RPCPI and a dummy variable for RPCPI exceeding FMAP threshold of 1.054 (RPCPI X 1(RPCPI > 1.054)). Estimates are based on the specification in column (5) of Table 5. See notes to Table 5 for other covariates included

Table 7 Implied multiplier and cost pr job estimates

	(1) 1990–2010	(2) 1990–1999	(3) 2000–2007	(4) 2008–2010
Panel A: implied multiplier of FMA				
Implied multiplier	0.694**	0.634**	0.844*	1.652
	[0.267, 1.121]	[0.244, 1.023]	[− 0.021, 1.709]	[− 0.877, 4.181]
Panel B: implied cost per Job				
Cost/Job year ($1000)	156**	234*	135*	73
	[60,252]	[− 19,487]	[− 3, 274]	[− 39,186]
Observations	1068	510	408	150

Notes: $*p<0.10$, $**p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990–2010. Data sources include BLS, CPS, BEA, CMS, and HHS. Implied multiplier is estimated as $\$108,700/(\$100,000/(\beta_1/3))$ using estimates of β_1 from estimating Eq. (5). 95 percent Confidence intervals are estimated using delta method. Estimates are based on the specification in column (5) of Table 5. See notes to Table 5 for other covariates included

the Great Recession. Given the uncertainty around point estimates, the multipliers have fairly wide 95 percent confidence intervals, which we present in Table 7 and Fig. 4. Focusing on the 1990–2010 period, the upper bound of the multiplier in column (1) is close to 1, so we can rule out large multipliers from FMA—a dollar of spending led to about a dollar of output at most. We know that estimates using data from 1990 to 2010 mask substantial heterogeneity across years. As noted earlier, the multipliers were more modest during the 90 s expansion, and so is their upper bound

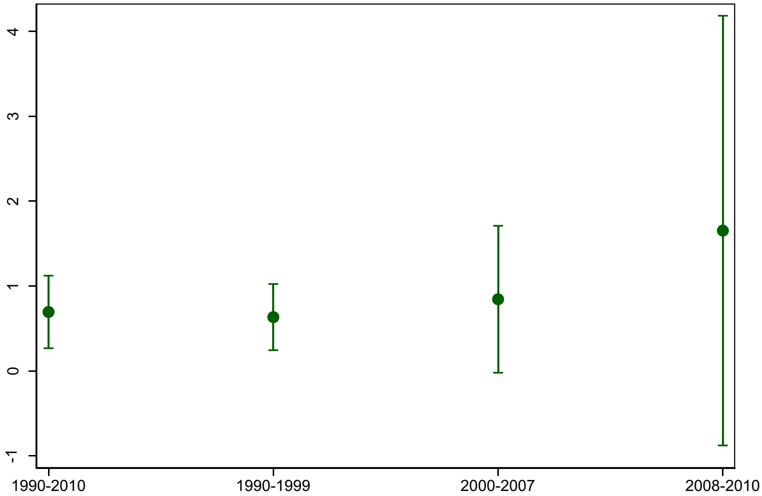

Fig. 4 Implied multipliers. The figure plots implied multiplier and 95% confidence intervals reported in Panel A of Table 7. The implied multiplier is estimated as $\$108,700/(\$100,000/(\beta_1/3))$ using estimates of β_1 from estimating Eq. (5). Confidence intervals are estimated using delta method

presented in panel A of Table 7. Between 2000 and the Great Recession, the multiplier had an upper bound of around 1.7. Implied multiplier estimates are the largest following the Great Recession, where we cannot rule out very large multipliers as high as 4.2.

Cost per job estimates reported in panel B of Table 7 reveal a pattern analogous to that for the multiplier estimates. Using data for all years, the estimated cost per job is quite high—nearly \$156,000 for the full sample, with an upper bound of \$252,000.[18] Cost per job estimates declined in the 2000s, reaching as low as \$73,000 after the Great Recession, though confidence intervals are wide and suggest that the cost could have been as high as \$186,000/job. These cost per job estimates are larger than Chodorow-Reich's et al. (2012) estimates for the same period.

While we have focused on the cumulative 3-year jobs impact, Fig. 5 shows that the cumulative effects at other horizons yield similar estimates (in per year terms). The figure plots estimated coefficients for the post-2000 period and shows that cumulative estimates are quite persistent over the first 5 years. Estimates for longer horizons are not feasible with our data as the LP framework requires more data for longer horizons.

[18] Note that confidence intervals for cost per job may not exactly align with the bounds for the coefficients as they are nonlinear functions and their intervals have been estimated using delta method.

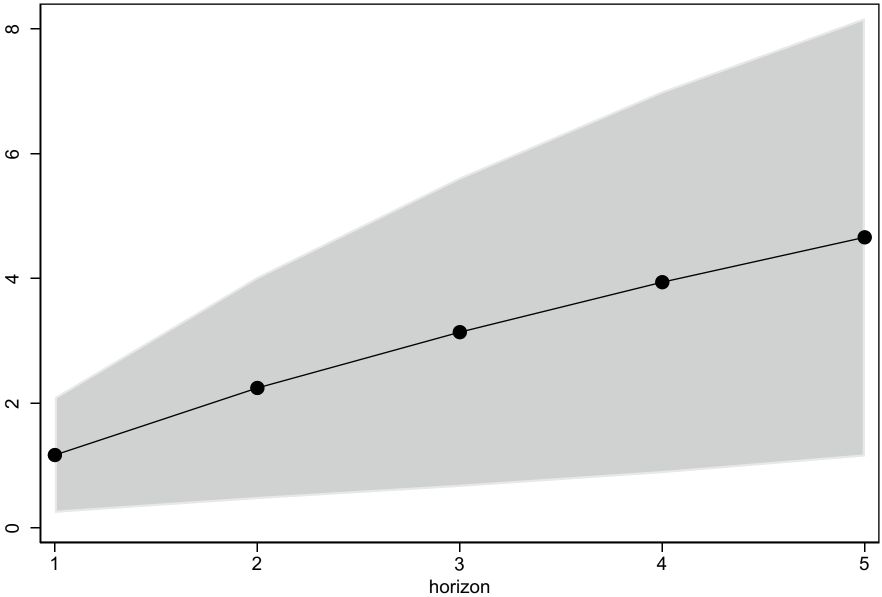

Fig. 5 Cumulative jobs impact of FMA: 2000–2010. Notes: The figure plots the coefficients on cumulative FMA (β_1^H) at horizons (H) ranging from year 1 through year 5 (see Eq. (5)). cumulative H-year future change in per capita jobs all regressions control for running variable (normalized RPCPI); year effects; first lag of share of union workers, share of manufacturing in state GDP, state population, and per capita real GDP; and demographic covariates–share of state population over age 65; female; white non-Hispanic; black; Hispanic; with a high school diploma; and those with any college education

6 Conclusion

Using state-level data from 1990 to 2013, we propose a new instrument to estimate the federal Medicaid assistance multiplier and present dynamic and long-term estimates of the multiplier. We find that the long-standing nonlinearity in the slope of FMAP with respect to state RPCPI also induces a corresponding nonlinearity in states' federal Medicaid assistance. We posit that this nonlinearity is otherwise uncorrelated with local economic conditions and show that non-Medicaid spending and most other covariates do not exhibit a similar nonlinearity.

Our preferred IV estimates suggest that the multiplier from federal Medicaid dollars was modest in the 1990's expansion but rose to exceed 1.6 during the Great Recession. On average, from 1990 to 2010, federal Medicaid assistance through FMAP transfers had a modest positive multiplier—an additional \$100,000 in federal Medicaid assistance created about 1.9 jobs over three years, yielding a statistically significant employment impact of 0.6 job years at a cost per job of nearly \$156,000, with an implied multiplier of 0.69.

This relatively modest macroeconomic stimulus from FMAP transfers should not be conflated with the overall welfare effects of the Medicaid program, which extends well beyond its multiplier effects through its impact on health, productivity, and well-being of the target population. Furthermore, in line with this literature, we ignore contractionary effects (experienced by other states) from financing federal FMA.

While we introduce a new instrument to estimate the outside-financed geographic cross-section fiscal multiplier, our estimates come with the caveat that it should be viewed as a local average treatment effect (LATE) specific to states that would alter their Medicaid spending due to the FMAP threshold—and hence would get more or fewer federal Medicaid dollars than would be the case otherwise.

Despite the caveat, we anticipate our estimates to be valuable in assessing the stimulative effect of federal Medicaid assistance through FMAP, which amounts to nearly $400 billion annually and comprises more than half of all federal grants to states. Our results suggest that the regular FMAP transfers during normal times are not very stimulative. However, our estimates imply that the $50 billion provided to states as fiscal relief through a 6.2-percentage point increase in FMAP rate during the COVID-19 downturn was likely effective in spurring economic activity.

Appendix

See Fig. 6 and Tables 8, 9.

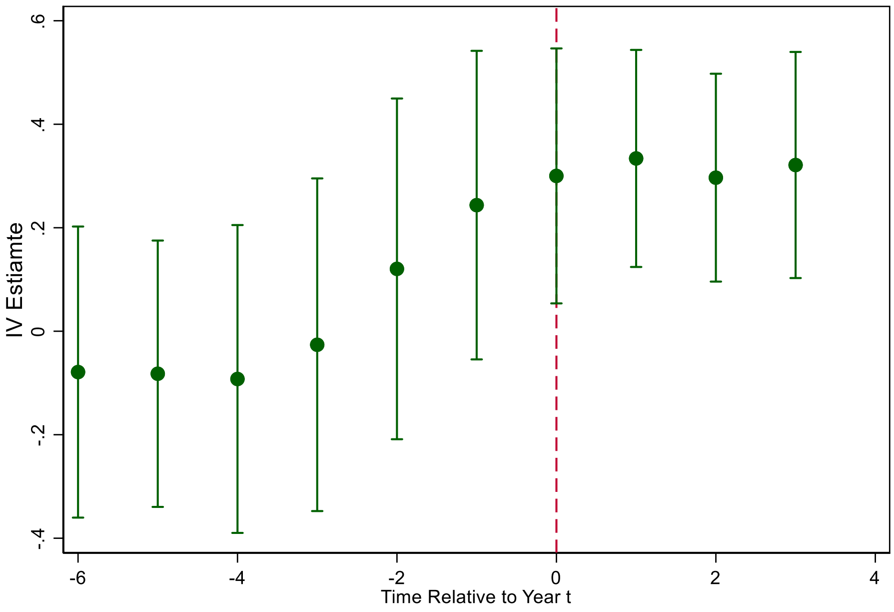

Fig. 6 Relationship of FMA with lags and leads of job growth. Notes: The figure plots IV estimates with their 95 percent confidence intervals from specifications with lags and leads of job growth as dependent variables. Estimates are based on the specification in column (5) of Table 5. See notes to Table 5 for other covariates included

Table 8 Heterogeneity in IV estimates of jobs impact of FMA by managed care and economic conditions

	(1) Full sample	(2) By High UR	(3) By managed care
Per Capita FMA	1.916**	1.849**	2.848
	(0.601)	(0.649)	(3.916)
Per Capita FMA × High UR		0.369	
		(5.196)	
Per Capita FMA × Share Managed			−0.007
			(0.176)
Observations	1068	1068	607
R-Sq	0.715	0.717	0.683
First Stage *F*	11.152	0.025	0.306
Underid *P*-val	0.007	0.804	0.407

Notes: $^{*}p<0.10$, $^{**}\ p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990–2010. Data sources include BLS, CPS, BEA, CMS, and HHS. The dependent variable is cumulative 3-year future change in per capita jobs. The table presents IV coefficient on the endogenous variable–3-year cumulative per capita FMA and its interaction term with high UR in column 2 and with share managed care in column 3–from IV regression using RPCPI X 1(RPCPI > 1.054) and its interaction as instruments. The coefficient should be interpreted as the number of jobs created from $100,000 per capita of FMA in 3 years. In column (2), high unemployment states are those with an unemployment rate 9 percent or higher. In column (3), "share managed" is the share of managed care in states' total Medicaid expenditure. Other covariates included are the same as the specification in column (5) of Table 5. See notes to Table 5 for other covariates included

Table 9 IV estimates of FMA impact on alternative measures of economic activity

	(1) 1990–2010	(2) 1990–1999	(3) 2000–2007	(4) 2008–2010
Panel A: real personal income				
Per Capita FMA	1.197	−0.126	4.312*	4.651
	(0.927)	(0.908)	(2.452)	(4.338)
R-Sq	0.475	0.554	0.331	0.455
Panel B: real GDP				
Per Capita FMA	1.028	−0.708	4.489*	5.269
	(1.611)	(1.828)	(2.636)	(6.581)
R-Sq	1068	510	408	150
Observations	0.425	0.378	0.500	0.402
First stage *F*	11.152	16.535	5.723	2.374
Underid *P*-val	0.007	0.001	0.047	0.162

Notes: $^{*}p<0.10$, $^{**}p<0.05$. Standard errors clustered by state in parentheses. Estimates based on annual state-level panel data from 1990–2010. Data sources include BLS, CPS, BEA, CMS, and HHS. The dependent variable is cumulative 3-year future change in real personal income (Panel A) and real GDP (Panel B). The table presents IV coefficient on the endogenous variable–3-year cumulative per capita FMA–from IV regression using RPCPI X 1(RPCPI > 1.054) as instrument. The coefficient should be interpreted as 3-year cumulative multiplier from FMA. Estimates are based on the specification in column (5) of Table 5. See notes to Table 5 for other covariates included

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