How large are fiscal multipliers in Africa? Evidence from the 2013 Kenyan Fiscal Decentralization

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This paper estimates the subnational fiscal multiplier in Kenya using a natural experiment stemming from the 2013 fiscal decentralization. The reform, mandated by the 2010 Constitution, reallocated 15% of national revenues—equivalent to 4% of GDP—to 47 newly formed county governments. Because the allocation was determined ex-ante by a fixed formula, the variation in expenditure is plausibly exogenous to contemporaneous county-level economic shocks. Despite an increase in spending in some of the poorest regions of 15-35% of county GDP, it is difficult to detect any effects of the devolved expenditure on country level economic growth. Our preferred panel IV estimate is a precisely estimated regional multiplier close to zero.

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1. Introduction

The size of the fiscal multiplier determines the benefits of fiscal stimulus in recessions and the costs of fiscal consolidations. It is also broader summary statistic for how responsive an economy is to aggregate demand shocks, and hence captures the unintended side-benefits of a wide range of spending with other primary goals (such as cash transfers or infrastructure projects). The fiscal multiplier, which we denote $\mathcal{M}$, is defined as the $\$\mathcal{M}$ increase in GDP in response to a $1 increase in spending. Despite a voluminous literature on the size of the fiscal multiplier in advanced economies after the 2008 financial crisis, there is much less evidence on the size of the fiscal multipliers in developing countries, and only a handful of studies on Africa. This paper fills this gap by estimating the subnational fiscal multiplier in Kenya, leveraging the country's 2013 fiscal decentralization as a natural experiment.

It is important to estimate the fiscal multiplier in developing countries because there are strong reasons to believe that the fiscal multiplier may differ substantially across developed and developing countries — though the direction and size of the difference are subject to debate. One recent strand of papers, mostly focused on the macroeconomic effects of cash transfers, points to *larger* multipliers in developing countries (see Egger et al 2022, 2025, Mendes et al 2024, Braco et al 2022). This literature makes two theoretical arguments, often backed up by empirical evidence. First, households in developing countries are financially constrained may which amplify the demand-side effects of fiscal policy (and especially transfers). Second, is that small firms in developing countries may have higher "slack", which allows them easily to expand capacity in response to an increase in demand. An opposing strand of literature, mostly empirical and focused on government consumption and investment, finds *small* multipliers in developing countries such as Kraay (2012, 2014), Ilzetzki et al (2013), Sheremirov and Spirovska (2022) and Fuiratti et al (2025). Our evidence helps to bridge these two literatures, as we study the effect of government consumption, but in regional Kenya where Egger et al (2022) recently found large multipliers for cash transfers.

Estimating the size of the fiscal multiplier in developing countries is especially challenging because fiscal policy is highly endogenous, and growth in developing countries is volatile. In our context of developing countries, fiscal policy is usually procyclical (Frankel et al 2013), in part because governments face tighter borrowing constraints in recessions. Procyclical fiscal policy tends to bias naïve estimates of the fiscal multiplier upwards. Fiscal policy can also be endogenous (especially at a subnational level) if it seeks to respond to increases in poverty, or natural disasters, which will usually bias the multiplier down. Moreover, even if exogenous variation in spending can be identified in developing countries, it is often too small to precisely estimate the fiscal multiplier. Growth rates in developing countries are more than twice as volatile in developed countries, meaning that much larger exogenous spending shocks are needed to

generate standard errors of a constant size.[2] Through this lens, 2013 Kenyan fiscal decentralization is close to an ideal setting to estimate fiscal multipliers, because the reallocation of spending was plausibly exogenous based on a pre-existing revenue formula, and the increase in spending was very large, making impacts easier to detect.

Institutional context and Identification Strategy. In 2010 Kenya approved a new Constitution via referendum with the objective of devolving political power and producing a more equitable regional distribution of spending. The new constitution established 47 new county governments, and in starting 2013, earmarked at least 15% of national tax revenue to be transferred to them. Before the new constitution, spending by subnational government was small, comprising <5% total public revenues (Boex and Smoke 2020, p299). As such the devolution represented a huge reallocation of spending, totaling around 4% of national GDP in 2013. Moreover, the expenditure was focused in more remote and poorer counties, which amplified its size as a share of the county economy. We calculate that for some of the poorest counties, the increase in spending was worth 15-35% of county GDP. To put this in context, at the upper end, it is similar in magnitude to the increase in US government spending during World War II.

Perhaps more importantly for identification, the allocation to each county was determined by a fixed formula recommended by the Commission on Revenue Allocation (CRA) in 2012, prior to the reform's rollout. The CRA formula was based on structural indicators like population, poverty, and land area --- as well as a basic equal share for every county to cover fixed costs. The country weights were fixed ex-ante for 5 years and written into law, and hence were not subject to manipulation by county officials or national politicians in response to shocks. The indicators underlying the calculation were also measured infrequently. As such, the fixed formula removed any link possible between country-level economic shocks the amount of revenues allocated to the county, allowing a clean identification of the causal impact of government spending on local economic output.[3]

To estimate the fiscal multiplier, we run a standard panel-IV regression of the two-year change in county-level output growth on the two-year change in county-level government spending (as a share of GCP).[4] Our preferred specification instruments actual spending with equitable share revenues during devolution, determined by CRA's predetermined formula as discussed above. The first stage specification is extremely strong, with a first-stage F-statistic above 100, a first stage

[2] Government spending is a smaller share of GDP in developing that developed countries, often resulting in smaller fiscal shocks as a share of GDP.

[3] Counties also had limited capacity to offset or respond to the inflows: under the 2010 Constitution they cannot freely borrow, and their own-source revenues (from property taxes, fees, etc.) are small.

[4] We allow for up to a six-month lag between spending and its economic impact (also because fiscal and calendar years are offset by six months), and study non-overlapping two-year periods because implementation of devolution spending was incomplete at the end of the first fiscal year (FY2013-14).

coefficient on equitable share revenues close to one. We also include time and county fixed effects to absorb aggregate shocks or county-level growth trends.

Results summary. Despite a large spending shock in some countries of up to 15-35% of county GDP, we are unable to detect any effect of spending on contemporaneous county-level economic growth. More specifically, our preferred estimate of the subnational fiscal multiplier in Keyna is precisely estimated at 0.1 (se=0.12) and insignificant, which we summarize as a "precise zero".[5] Our baseline specification is able to reject any multiplier above about 1/3, which means we can also rule out a large multiplier ($\mathcal{M} > 1$) as has discussed in the literature. Our findings are robust to a battery of robustness tests, including dropping counties or years one-by-one, winsorizing volatile data, adding controls, using alternative specifications of the instrument, and reconstructing prerevolution spending.

While it is difficult to know why the multiplier is so small, one candidate explanation is low productive capacity in the remote and poor counties with the largest increases in spending. These countries have economies focused on agriculture, and so almost anything that the government buys has to be produced elsewhere, limiting the boost to the local economy. Moreover, there are likely only a small number of qualified administrators, and so an expansion of government employment is likely to crowd out employment in the private sector. Uncovering the mechanism driving the small multiplier is an interesting area for future research.

Contribution. This paper contributes to 4 literatures. First, this paper contributes the literature subnational fiscal multipliers, which estimate local effects of government spending in a panel or cross-section when aggregate variables like monetary policy are controlled for by time fixed effects. As discussed in Nakamura and Steinsson (2014), the subnational multiplier is different from aggregate multiplier --- the latter is sensitive to monetary policy and other aggregate variables. While mapping the subnational multiplier into the aggregate multiplier is not straightforward, Chodorow-Reich (2019) argues that local multipliers can provide a lower bound on aggregate effects when official interest rates don't respond to the fiscal shock. To our knowledge, this paper provides the first estimates of the subnational government purchase multiplier in an African country, and one of very few in a developing country.

Second, we contribute to a broader literature on the size of fiscal multipliers (national and subnational) in developing vs developed countries. Our small multipliers on government purchases in a developing country context are consistent with others such as Kraay (2012, 2014), Sheremirov and Spirovska (2022) and Fiuratti et al (2025), and the effects are much lower than are often estimated in developed countries. However, they contrast with some of the larger estimates

[5] That is, the multiplier insignificantly different from zero at the 5% level, because the point estimate is small, not because the confidence intervals are wide.

of subnational purchase multipliers (Corbi et al 2018), and transfer multipliers (Egger et al 2022, Mendes et al 2023). One explanation is Kenya is very different from Brazil studied in Corbi et al (2018) and Mendes et al (2023), though is the exact same country studied by Egger et al (2022), though for a different instrument (cash transfers). Our results suggest that large multipliers in some developing countries may not be generalizable outside the context in which they are estimated.

Third, our study speaks to the public finance and political economy literature on fiscal decentralization (e.g. Faguet 2004). This literature mostly focuses on the efficiency, equity or politics of the spending changes, which we complement with an estimation of its growth impacts (or lack thereof). As Kenya's devolution reform is notable in its scale and rules-based implementation, we hope that our data set will encourage further studies of this shock.

Finally, our paper contributes to a classic debate on the role of a lack of demand as cause of underdevelopment, and "big push" development strategies to overcome it (Murphy, Shleifer, and Vishny 1989, Goldberg and Reed 2023). Our natural experiment creates a huge boost to demand in the poorest counties in Kenya – of 15-35% of GDP – but produces no short-term growth boost. This may temper expectations of large near-term development impacts stemming from large increases in spending in the poor regions.

The rest of this paper is organized as follows: Section 2 describes the institutional context and process of devolution (which informs our specification). Section 3 presents our empirical approach, including our identification strategy, empirical specification and the data used. Section 4 presents our main results, which we show are robust to a battery of tests in Section 5. Section 6 concludes.

2. Institutional Context and Stylized Facts on Devolution in Kenya

Kenya's 2013 fiscal decentralization marked a major structural shift in intergovernmental relations, grounded in the 2010 Constitution. The reform established 47 county governments and mandated the transfer of a constitutionally guaranteed equitable share of at least 15% of national revenues to these subnational entities. The resulting intergovernmental fiscal system placed counties at the center of service delivery and public investment, with important implications for macroeconomic management and local development.

The sweeping reforms of the 2010 constitution attempted to rectify some of the deep grievances with the Kenyan state that lead to the post-election violence in 2007-08. Its design responded to three linked problems: persistent regional inequalities in public investment and service access; weak citizen participation and limited downward accountability; and a highly centralized fiscal system that politicized resource allocation (Kanyinga 2016; Akech 2010; Lind 2018). The 2007–08

post-election crisis underscored these risks and informed reformers' emphasis on diluting patronage and localizing accountability (Mueller 2011; D'Arcy & Cornell 2016). Article 174 codifies these aims—democratic and accountable exercise of state power, public participation, equitable sharing of resources, protection of marginalized communities, and bringing services closer to citizens—providing the normative basis for shifting functions and revenues to counties (Republic of Kenya, Constitution of Kenya 2010; see also Gathii & Otieno 2018).

2.1 The Revenue Allocation Formula

The equitable share was distributed to counties using a pre-specified formula designed by the Commission on Revenue Allocation (CRA) and adopted in 2012—prior to any county-level economic data that could be affected by devolution. The formula combined long-run structural indicators such as population, poverty rates, land area, and a fixed equal share (Table 0). As discussed in Kimenyi (2013), this rule-based approach sought to reduce political discretion and ensure fairness and predictability.

The formula remained largely stable throughout the study period. While the CRA is required to revise the allocation basis every five years, the second formula (2018–2023) featured weights nearly identical to the first (2013–2018). Thus, variation in transfers across counties was effectively fixed during the main rollout period and plausibly exogenous to contemporaneous county-level shocks.

Table 0: Revenue allocation Formula

Parameter	First Basis Weight (%)
Population	45%
Poverty Index	20%
Land Area	8%
Basic Equal Share	25%
Fiscal responsibility	2%

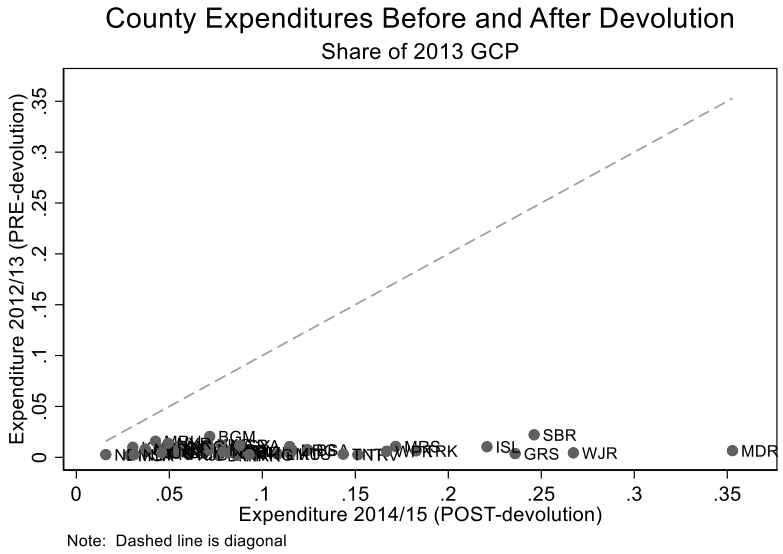
Source: Kimenyi (2013)

2.2 Pre- and Post-Devolution County Expenditure Levels

Prior to devolution, county-level expenditure (constructed from 44 former municipalities) was very low in most counties, typically under 2% of Gross County Product (GCP). In contrast, post-devolution expenditures increased sharply, especially in the poorest and most remote counties. Figure 1 highlights the magnitude of the fiscal shock, with counties like Isiolo (ISL), Samburu (SBR), Garissa (GRS) and, Wajir (WJR) experiencing increases to 25% of GCP, and Mandera (MDR) 35% of GDP. Note that the robustness section investigates an alternative calculation of expenditures in 2012/13

One striking example of the scale of the fiscal shock introduced by devolution is Mandera County (MDR), located in arid northeastern Kenya near the Somali and Ethiopian borders. Despite accounting for only around 0.2% of Kenya's national population—roughly 100,000 people out of a total 50 million—Mandera received a disproportionately large share of the equitable allocation transfers. This was due to the structure of the revenue allocation formula, which gave 25% weight to a basic equal share distributed equally across counties, regardless of population size. As a result, even very small counties received a meaningful baseline allocation. In addition, Mandera's high poverty levels further increased its formula-driven share of national transfers. Because both the numerator (equitable transfers) was large --- and the denominator (local GDP) was small --- in this case, Mandera experienced one of the largest observed increases in public spending relative to local output—reaching approximately 35% of GCP within two years of devolution. This underscores how the allocation formula, while rule-based and exogenous, generated very large fiscal shocks in some of the poorest counties relative to the size of their economies.

Figure 1. Pre- vs Post-Devolution Expenditure (% of GCP)



2.3 The Rollout of Devolution: Year-by-Year Evidence

Devolution was supposed to happen in one year (2013-14), but implementation was uneven in the early years.[6] In the first year of devolution (2013–14) (Figure 2A), counties received their first equitable share allocations. However, they managed to spend only around 60% of the total allocated funds overall, likely reflecting significant implementation bottlenecks in newly formed county governments. These challenges included weak administrative capacity, unfamiliarity with procurement and budget procedures, and delays in disbursement or execution.

[6] Note that the first county governments were elected in March 2013, and the Transition Authority began formal function transfers via legal notices in 2013. However, the first fully devolved budget year is 2013/14, which is the relevant date for our study.

Figure 2A. Change in Expenditure vs Change in Equitable Share (2013–14)

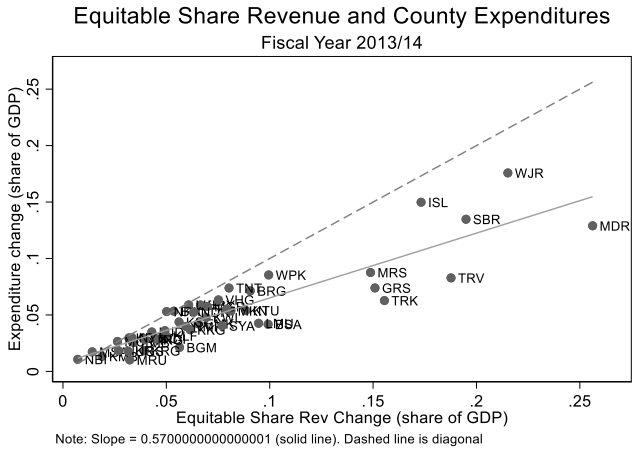


Figure 2B. Change in Expenditure vs Change in Equitable Share (2014–15)

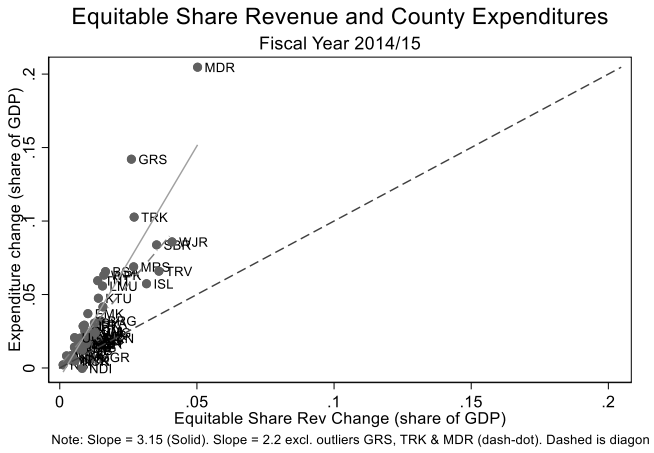
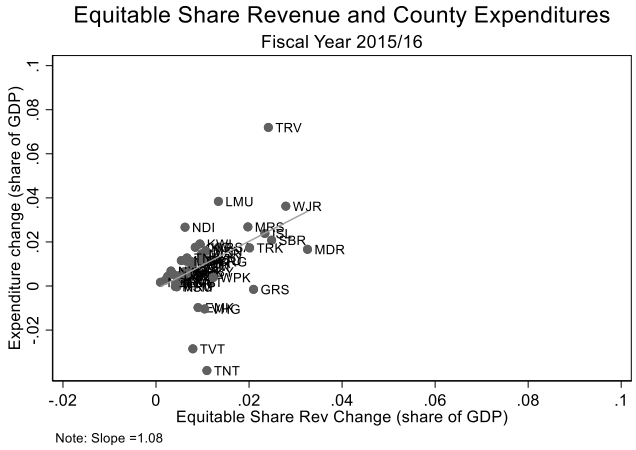


Figure 2C. Change in Expenditure vs Change in Equitable Share (2015–16)

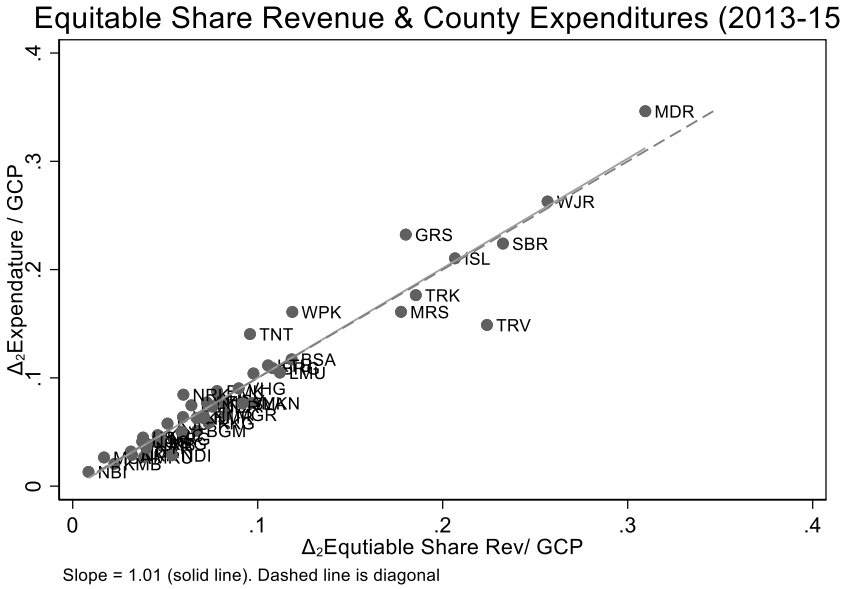


Source: Author calculations based on Controller of Budget and KNBS

In the second year (2014–15 Figure 2B), equitable share allocations increased modestly, but actual county expenditures increased substantially. Specifically, equitable share revenues rose by less than 5% of county GDP, but spending increased by up to 20% of GCP. It seemed that this point, most county governments had adapted to new fiscal responsibilities, and the absorptive capacity of local public financial management systems improved visibly.

By the third year (2015–16), the fiscal adjustment process had largely stabilized. Increments in both equitable share revenues and expenditure were much smaller, and the year-to-year variation in spending diminished. The more compressed relationship reflects stabilization in both transfers and expenditures, supporting the view that the devolution rollout was complete by this point.

Figure 3. Two-Year Change in Expenditure vs Two-Year Change in Equitable Share



This time-path of fiscal adjustment—rapid but uneven in year one, continuing with catchup in year two, and stabilizing by year three—motivates our empirical focus on a two-year change specification (Δ_2). Figure 3 shows a tight Shilling-for-Shilling relationship between two-year revenue changes and two-year expenditure responses, supporting the identification assumption that changes in spending are driven by exogenous variation in equitable share revenues. This relationship will serve as the first stage in our 2SLS estimation.

3. Identification Strategy, Empirical Specification, and Data

Estimating the causal effect of government spending on output is complicated by a number of endogeneity concerns. In a developing country context, the most serious challenge is reverse causality: economic growth may itself affect fiscal outcomes. Fiscal policy in developing countries is often procyclical (Frankel et al 2013): negative growth shocks reduce revenue

collection and thus constrain spending, while positive shocks can expand revenues and hence spending. In such cases, simple OLS estimates of the multiplier would be biased, as higher spending could be a consequence (not just a cause) of higher GDP. Note that in developed countries, fiscal policy is usually counter-cyclical, resulting in OLS multiplier estimates that are biased downwards.

A second concern is omitted variable bias (OVB), where unobserved shocks could simultaneously affect both economic performance and government expenditure. For instance, a major flood could depress output while triggering reconstruction-related increases in spending, biasing the estimated fiscal multiplier downward. Political changes can also lead to omitted variable bias: a new leader may introduce new policies that affect growth, while also changing spending.

We partially OVB by including a rich set of fixed effects and allowing for additional controls. County fixed effects absorb all time-invariant differences between counties (such as geography or long-run growth), and year fixed effects control for common national shocks in each year (such as country-wide business cycle movements, inflation, or national policy shifts). This means that the only remaining potential confounders would be those that vary at the county-year level. In Section 5, we also perform robustness checks adding specific time-varying controls such as other types of revenues

3.1 Identification Strategy

To address both reverse causality and OVB challenges, we exploit the natural experiment created by Kenya's 2013 fiscal decentralization, which introduced large, formula-based equitable share transfers to counties. Because the equitable share allocation was determined using a fixed formula agreed upon in 2012—prior to the first year of county-level GCP data—it generates plausibly exogenous variation in county-level public spending. Moreover, the size of the aggregate change revenues allocated to equitable share expenditure was largely predetermined in the 2010 constitution (which set a minimum of 15% of national tax revenues to be devolved). Since counties were unable to borrow and had only minor own-source revenues, changes in expenditure are largely driven by this central transfer

More specifically, we implement our identification strategy using 2SLS specification, where the two-year change in equitable share revenues (as a share of GCP) during devolution act as an external instrument for the two year change actual county spending (share of GCP we show robustness to variants of this identification strategy, such as using actual devolution expenditure as the instrument, or all equitable share revenue (including away from devolution).

3.2 Empirical Specification

The second-stage specification is shown in Equation 1. The main outcome variable Gross County Product (GCP) growth, and the main explanatory variable is county-level change in spending as a share of GCP. Given our data, and implementation issues during devolution, our preferred specification examines two-year changes in expenditure and two-year output growth. To reduce serial correlation, we use non-overlapping two-year periods, so t∈{2013, 2015, 2017, 2019, 2021}.

$$(1) \qquad \frac{y_{c,t+2}-y_{c,t}}{y_{c,t}} = \beta\left(\frac{\Delta_2 Exp_{c,t+2-1/2}}{y_{c,t}}\right) + \theta.X_{c,t} + \gamma_t + \mu_c + \varepsilon_{c,t}$$

Where $\frac{y_{c,t+2}-y_{c,t}}{y_{c,t}}$ is the growth rate of GCP in county c, from year t to $t+2$, $\frac{\Delta_2 Exp_{c,t+2-1/2}}{y_{c,t}}$ is the instrumented two-year change in spending as a share of GCP (lagged by half a year to align with the fiscal year), γ_t are county fixed effects, μ_c are year fixed effects and $\varepsilon_{c,t}$ is the error term.

The coefficient of interest is β --- the relative fiscal multiplier. Its interpretation is as follows, if one county received an extra shilling of expenditure relative to other counties, output in that county would be β higher (relative to other counties). As discussed in the introduction, this is related but distinct from the aggregate fiscal multiplier.

The first-stage equation is:

$$(2) \qquad \left(\frac{\Delta_2 Exp_{c,t+2-\frac{1}{2}}}{y_{c,t}}\right) = \phi\left(\frac{\Delta_2 Z_{c,t+2-\frac{1}{2}}}{y_{c,t}}\right) + \theta.X_{c,t} + \gamma_t + \mu_c + \epsilon_{c,t}$$

Where $\frac{\Delta_2 Z_{c,t+2-\frac{1}{2}}}{y_{c,t}}$ is our main instrument is the two-year change in equitable share revenue (as % of GCP) during devolution (zero otherwise). We also report related instruments, where $\Delta_2 Z_{c,t+2-\frac{1}{2}}$ is the change in equitable share revenue in all years, or actual expenditure during the first two years of devolution. These generate very similar results. The first stage turns out to be very strong, with $\hat{\phi} = 1$ (as in Figure 3).

Our identification assumption is that changes in Equitable Share Revenues around devolution $\frac{\Delta_2 Z_{c,t+2-\frac{1}{2}}}{y_{c,t}}$ is uncorrelated with shocks of GCP growth in the second stage $\varepsilon_{c,t}$

3.3 Data

The empirical analysis utilizes a newly constructed annual panel dataset covering Kenya's 47 counties over the post-devolution period from 2014 to 2023. In principle, this yields up to 470 county-year observations, though as we utilize non-overlapping two-year changes (as in Nakamura and Steinsson 2014), this is reduced to 235. This dataset includes each county's growth rate of Gross County Product (GCP), the change in public expenditure which is measured as share of GCP to ensure a multiplier interpretation. All variables are deflated by "real", in that that they

are deflated by the national GDP deflator. Ideally, one would deflate by county-level prices but these are unavailable. Unfortunately, there is no county GDP data exist prior to 2013 (before devolution), which prevents the inclusion of pre-treatment periods.

The primary outcome variable is the annual growth rate of Gross County Product (GCP), as published by the Kenya National Bureau of Statistics (KNBS) in Statistical Abstracts and GCP reports posted on their website. GCP data are available on a calendar year basis from 2013 through 2023.[7] GCP estimates are calculated on a "top down" basis, by splitting national GVA of each industry across the 47 counties. The share of each county in each industry's total GVA is known as its distribution key, which are updated using county level surveys which depend on the surveys.[8]

The main explanatory variables are derived from county-level fiscal data. These include total expenditure and the equitable share revenue allocated to each county by the national government. Fiscal data are reported for financial years that begin on July 1st and end on June 30th. To better align fiscal changes with economic outcomes, we assume a six-month lag between expenditure and output response, such that fiscal year data are matched to the subsequent calendar year.

The equitable share data come from the Office of the Controller of Budget's County Budget Implementation Review Reports, which document budgeted and actual transfers to each county. Total expenditure is also sourced from these reports and includes both recurrent and development spending. Pre-devolution expenditure levels are not directly available at the county level. To construct these for 2012-13 --- which serve as a baseline for pre-devolution regional spending --- we use administrative expenditure data from 44 pre-2013 municipalities (details in the appendix).[9] Our key explanatory variable is the annual county government spending, which we construct from official budget execution reports. [10] These reports provide actual spending by county, and the different financial sources such as the formula-based unconditional "equitable share transfers from national government, conditional grants, and counties' own-source revenue.

[7] By construction, the sum of GCP across all counties equals the national GDP, aside from minor discrepancies (for example, taxes on products are not fully allocated to counties due to data limitations).
[8] The exact methodology of updating the distribution keys varies across industries, counties and over time. We were concerned that GCP data might be interpolated just by splitting national industry output across sectors using constant distribution keys. But we verified that for available data (2017 onwards), the distribution keys are never constant.
[9] In counties with a one-to-one mapping to a municipality, we assign expenditure directly. For counties composed of multiple municipalities, we aggregate the relevant components. In cases where no direct mapping exists, we impute county expenditure based on nearby proxy municipalities with comparable per capita characteristics
[10] We use data from the Office of the Controller of Budget (OCOB), specifically the County Budget Implementation Review Reports

Unfortunately, all expenditure data follows the Kenyan fiscal year, which runs from July 1 to June 30; to align fiscal data with calendar-year GDP data, we assume that expenditure may affect GDP with a six-month lag. This timing issue becomes less important as we study two-year windows, which allow for an 18 month overlapping period. For example, two-year GCP growth compares GCP in calendar 2015 to that in calendar 2013, which is regressed on the change in expenditure in fiscal year 2014/15 relative to fiscal 2012-13 (as a share of GCP).

In the first full year of devolution (FY2013/14), the 47 counties collectively had an approved budget equivalent to about 5.4% of national GDP, of which roughly 4.3% of GDP (almost 80% of county budgets) was financed by national equitable share transfers. Actual spending in that initial year was lower – about 3.4% of GDP – due to capacity constraints and underspending of development budgets. In the following, county expenditures grew as absorption improved. Our dataset captured actual expenditure data reported by OCOB, not planned expenditure, and the two-year windows helps us look through any teething problems during the roll out of devolution.

4. Results

This section presents the main empirical findings on the impact of increased subnational government spending on county-level output. The analysis focuses on the two-year period following the onset of fiscal decentralization (2013–2015), during which the majority of the fiscal shock was absorbed by county governments. As discussed in previous sections, this Δ2yr specification is designed to capture the full initial effect of the reform, while avoiding year-to-year volatility and implementation lags.

4.1 Graphical Evidence: The Subnational Fiscal Multiplier

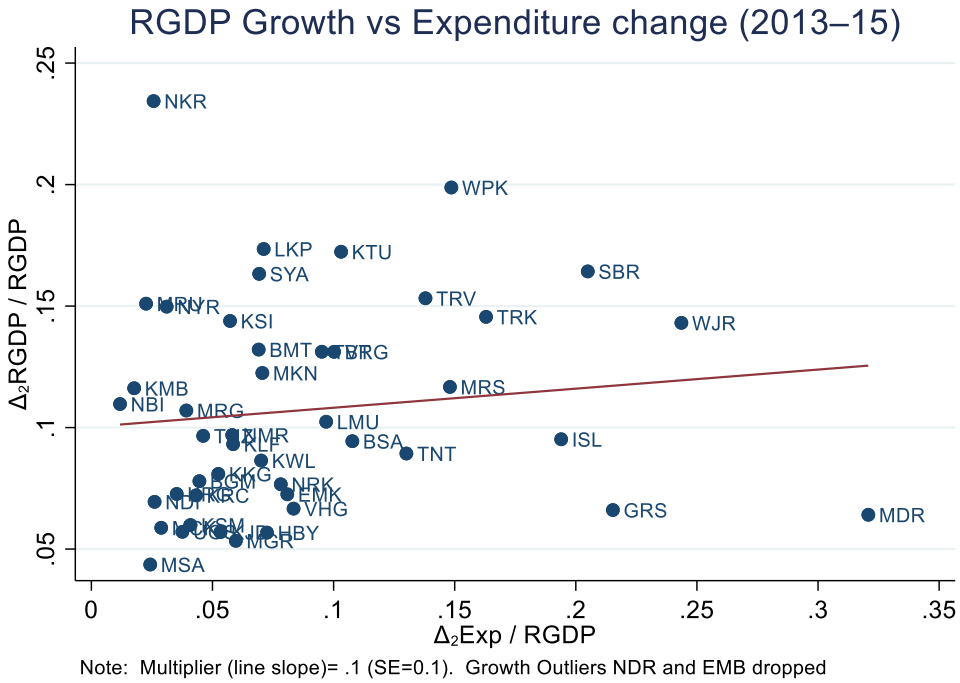
Figure 4 plots the core relationship estimated in our paper: the two-year growth rate of Gross County Product (GCP) on the vertical axis, and the two-year change in government expenditure (as a share of GCP) on the horizontal axis, for the period 2013 to 2015. Each point represents a county. The fitted line shows the slope of the best linear approximation to this relationship, which corresponds to the fiscal multiplier. The slope of this line is 0.1, with a standard error of 0.1—implying that the point estimate is statistically indistinguishable from zero.[11] These results are similar to the panel estimates.

This figure provides intuitive and visual confirmation of our central finding: there is no systematic relationship between the size of the fiscal shock and local output growth across counties. Some counties that received large increases in expenditure—such as Mandera (MDR) and Garissa (GRS)—recorded low GCP growth, while others with smaller expenditure shocks saw higher

[11] Specifically, the coefficient in Figure 4 is 0.087 (se=0.125) with N=45. With the two outliers (NDR and EMB) added back in, the coefficient changes slightly to 0.088 (se=0.0135) with N=47. Both coefficients are insignificant at the 5% level, but significantly less than 1 (or 0.5).

growth. While there is considerable variation in both variables, the near-zero slope of the regression line suggests that, on average, additional public spending did not translate into measurable short-run economic gains.

Figure 4. Two-Year Growth of RGDP vs. Change in Government Expenditure (2013–2015)



Note: Multiplier (line slope)= .1 (SE=0.1). Growth Outliers NDR and EMB dropped

This flat reduced-form relationship is the graphical counterpart to our econometric analysis and highlights the surprising null result. In a context where many counties experienced spending increases in excess of 10% of their local GDP, one might have expected at least moderate multiplier effects. The fact that the estimated slope is so close to zero suggests either a highly inefficient translation of spending into economic activity, or significant measurement issues—both of which are explored in later sections.

4.2 Regression Results

The full regressions estimates, presented in Table 1, confirm the findings from the scatter plot. In column (1) we first report a simple OLS panel regression with both county and year fixed effects, and standard errors clustered at the county level. The regression estimate of 0.1 (SE=0.12) is nearly identical to the slope of the line in the scatter plot. This is unsurprising, as the observations with the greatest leverage are those from the 2013-15 devolution rollout period in the scatter plot.

Column (2) (in bold) reports our 2SLS baseline specification, instrumenting the two-year change in county expenditure with the formula-based change in equitable share revenues, exploiting the exogenous variation created by the 2013 devolution reform. As before, the coefficient on

government spending is very close to zero ($\hat{\beta}$ =0.1), and statistically insignificant from zero at the 5% level. This implies that a large, exogenous increase in subnational public spending had no detectable short-run impact on county GDP growth. Moreover, because the estimates are precisely estimated (se=0.12), we are able to reject that the multiplier is above about 1/3 at the 5% level. Note that the first stage F-statistics is 118 indicating the instruments are highly relevant. The first-stage coefficient ϕ is close to 1, suggesting that a 1 shilling increase in equitable share revenues translates into a 1 shilling increase in expenditure (and explaining the similarity to the OLS results).

Table 1. Estimated Subnational Fiscal Multipliers Using Two-Year Changes

Dep. variable: Real GCP Growth	**(1) OLS**	**(2) 2SLS**	**(3) 2SLS**	**(4) 2SLS**	**(5) 2SLS**	
Multiplier	0.11	**0.11**	0.049	0.116	0.11	
	(0.12)	**(0.12)**	(0.107)	(0.132)	(0.125)	
County FE & Year FE	YES	**YES**	YES	YES	YES	
Obs	235	**235**	235	235	235	
Instrument: (or notes)		**Δ Equitable share Revenues (%GCP) (FY13, FY14)**	Δ Actual Devolution Expend. (%GCP) (FY13, FY14)	Δ Equitable share Revenues (%GCP) (All years)	Δ Equitable share Revenues (%GCP) (FY13, FY14)	Δ Equitable share Revenues (%GCP) (Not FY13/FY14)
First-stage F-Stat.		118	1657	157.5	116	
First-stage coefficient		0.99***	0.997***	0.972***	0.99***	0.59***
		(0.091)	(.0244)	(0.077)	(0.085)	(0.198)

Notes: County-clustered robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Notes: Dependent variable: two-year real growth in Gross County Product (RGCP); standard errors clustered at county level. Columns (4)–(6) report 2SLS estimates using variation in equitable share revenue as an instrument for spending

The remaining three columns present results with different variations of the devolution instrument, to show robustness of the baseline multiplier estimates. Column (3) replaces the change in equitable share revenues during devolution with actual expenditure during devolution. This is justified by the argument at the allocation of expenditure during that period was effectively orthogonal to any contemporaneous growth shocks. The multiplier is similar to the baseline, but with a marginally small coefficient (and an even stronger first-stage F-statistic).

Column (4) uses the change in equitable share revenues (share GCP) over the whole sample as the instrument. The results are again similar to the baseline, mostly because the devolution period has the highest leverage.

Finally, Column (5) splits the equitable share revenue instrument in column (3) into two parts: the change in equitable share revenue during devolution (as in the baseline in column (2)), and the change in equitable share revenue during the post-devolution period (a Bartik-style component mainly driven by national-revenue movements). The multiplier estimate is almost identical to the baseline ($\hat{\beta}$ =0.1), as is the first stage coefficient on equitable share revenues during devolution.

Away from devolution, the first stage coefficient on equitable share revenues is smaller (≈0.6), but still highly significant, indicating there are other determinants of expenditure during more normal times.

5. Robustness

This section shows that our results are robust to concerns about the measurement of pre-devolution expenditures, outliers/influential observations and controls for other revenue types. All robustness tests start with the baseline specification (Table 2, Column (2)).

The first concern is about under-measuring pre-devolution expenditure at the county level in 2012/13.[12] This is a serious threat to our main results, as it could lead to a smaller increase in county spending over 2012/13-2014/15, and hence a larger multiplier. Our default measure, we has included pre-devolution expenditure at the municipality level in 2012/13 (recall municipalities are a pre-devolution regional government). However, it didn't include federal expenditure that was implemented at the regional level before devolution, for example a health clinic operating Isiolo in 2012-13, but funded directly by the federal government. Care is needed because we need to distinguish between federal expenditure in 2012/13 in a county that "devolved" in 2013/14, and regional federal expenditures that were unaffected by devolution. For example, county health services - dispensaries, health centers and county referral hospital (Levels 2-5) where devolved, whereas national referral hospitals (Level 6) continued to be operated by the National government throughout devolution.[13] As such our measure of 2012-13 expenditure in Isiolo should include health expenditure on devolved county health services , but should exclude national referral hospitals and vertical national programs that did not devolve. Unsurprisingly there is no publicly available data with this level of detail (to our knowledge).

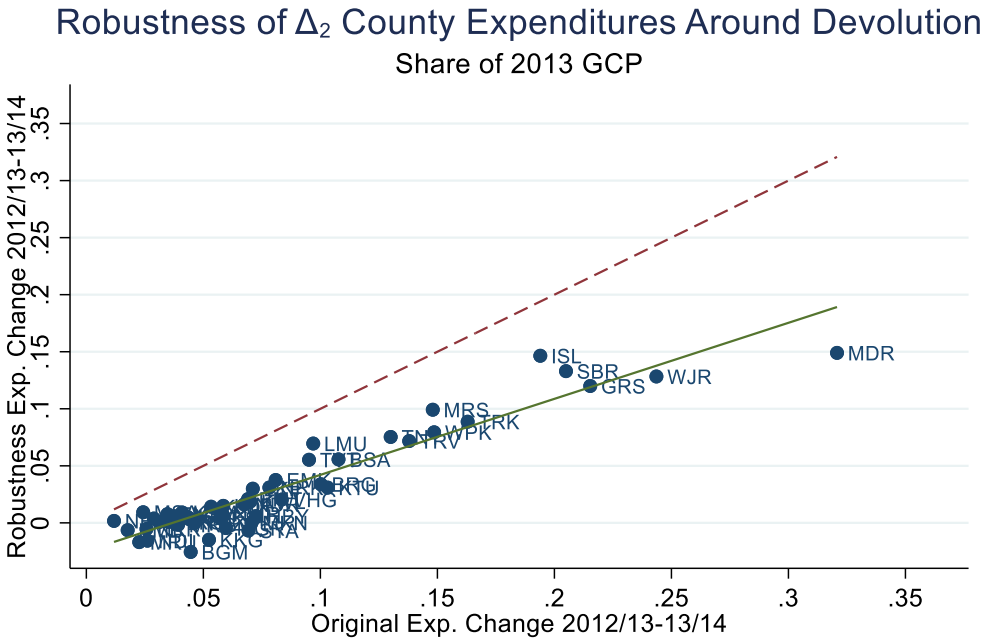
Instead, we construct a proxy for 2012-13 regional devolved expenditure, based on official documents from the lead-up to devolution, and a conservative assumptions about the cross-country allocation. In 2012, based a line-by-line budgetary analysis with ministries to the district

[12] We thank Aart Kraay for raising this concern.

[13] In practice, the national government also financed conditional allocations for Level-5 hospitals to compensate counties that host facilities serving patients from other counties (Consitution of Kenya 2010, Fourth Schedule; Health Act 2017). The full list of function assigned to counties are "a. Agriculture; b. County health services (including county health facilities, as well as functions such as cemeteries and solid waste management); c. Control of air pollution, noise pollution, other public nuisances and outdoor advertising; d. Cultural activities, public entertainment and public amenities; e. County transport, including county roads; f. Animal control and welfare; g. Trade development and regulation; h. County planning and development; i. Pre-primary education, village polytechnics, homecraft centres and childcare facilities; j. Implementation of specific national government policies on natural resources and environmental conservation; k. County public works and services (including water and sanitation services); l. Fire-fighting services and disaster management; m. Control of drugs and pornography; n. Ensuring and coordinating the participation of communities in governance at the local level." (Boex and Smoke p302)

level Ministry of Finance (2012) concluded that "The total cost of funding the devolved functions based on the existing level of services…was estimated at Ksh. 154 billion for 2012/13." (p29)[14] We take the Ksh 154 billion aggregate, and allocate it across counties in 2012/13 assuming equal per capita expenditure. This is a conservative assumption because pre-devolution expenditure was well known to be focused on urban areas (which was one of the motivations of the unrest that lead to the new constitution in the first place).[15] We add this to our existing estimate of country-level expenditure in 2012.

Figure 5: Robustness to alternative expenditure measure in 2012-13



Note: Dashed line is diagonal. Slope of solid line is 0.66 (se=0.035). Robustness measure assumes that the 2012/13 cost of devolved functions is equally shared across counties on a per capita basis.

The resulting two-year change in expenditure (share of GCP) is shown on the Y axis is Figure 5 (the X axis is out default measure), labelled "robustness". The resulting increase in expenditure is substantially smaller than our default measure, on average about 2/3 as large. For example, the increase expenditure in Madera (MDR) is revised from from 33% of GCP to 15% of GCP, in Wajir (WGR) it falls from 25% of GCP to 12% of GCP, with smaller revisions for less-poor countries. As mentioned above this is probably an under-estimate of the increase in spending in these counties. Despite substantial change in estimated expenditure over 2013-15, multiplier estimates continue to be close to zero. The first column of Table 2 re-estimates our baseline specification using the alternative estimates of expenditure in 2012-13. As expected, the multiplier increases, but only marginally from 0.11 to 0.15 (se=0.18) and remains insignificant at the 5% level. The reason for

[14] Note that the Kenya Commission on Revenue Allocation (2012) estimates the required budget to be at about Ksh. 200 billion, but this includes administrative expenses for 47 new county governments, as well as a 10% buffer.
[15] In other words, 2012/13 expenditure in rural counties was likely lower than we assume in the robustness exercise, and so the increase in expenditure was larger than in the robustness test.

the small multiplier is because of the absence of an economic boom in the poorest counties over 2013-15 – even with the alternative expenditure estimates, a multiplier of 1 would require growth rates 25ppts higher in the poorest countries than in Nairobi, which we simply don't see in the data. For the rest of the paper, we return to using the default estimates of spending in 2012-13.

Table 2. Robustness Estimated Subnational Fiscal Multipliers Using Two-Year Changes

Dep. variable: Real GCP Growth	**(1) Alternative 2012-13 Expenditure (per capita)**	**(2) Windorized**	**(3) Revenue Controls**	**(4) Revenue Controls**
Multiplier	0.15	0.101	0.108	0.120
	(0.18)	(0.11)	(0.118)	(0.114)
Controls for other revenues				
Δ2(Own Source Rev./GCP)			1.152	
			(1.528)	
Δ2(Conditional Grants./GCP)				0.920
				(0.930)
5% Winsorized Growth		YES		
County FE & Year FE	YES	YES	YES	YES
Obs	235	235	235	235
Instrument:	Δ Equitable share Revenues (%GCP) (FY13, FY14) - All specifications			
First-stage F-Stat.	136	118	115.7	118.4
First-stage coefficient	0.65***	0.99***	0.99***	0.99***
	(0.055)	(0.09)	(0.09)	(0.09)

Notes: County-clustered robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1.

The second group of concerns stems from the high volatility of county-level GCP growth rates (a standard deviation of 7%). As such our results might just be picking up some very low growth rates in some poorest countries in 2013-15 (or other high expenditure years) and are not representative of a general relationship between government spending and economic growth. To assuage the concern, Table 2 Column 2 presents estimates where the GCP growth variable has been winsorized at the 5% level, which results in almost identical multiplier estimates.[16] Moreover Figure 6 presents re-estimates baseline multipliers, dropping one county at a time (Panel A) or one year at a time (Panel B). Multiplier estimates are fairly robust. Although they are marginally higher when Madera (MDR) is dropped, they are still close to zero and are statistically insignificant. There is some more variability in multipliers when different non-devolution years are dropped, with the multiplier falling slightly below when growth from 2015-17 is excluded, and increasing to 0.3 when growth over 2017-19 is excluded (which becomes marginally significant).[17] But even a multiplier estimate of 0.3 is considered small.

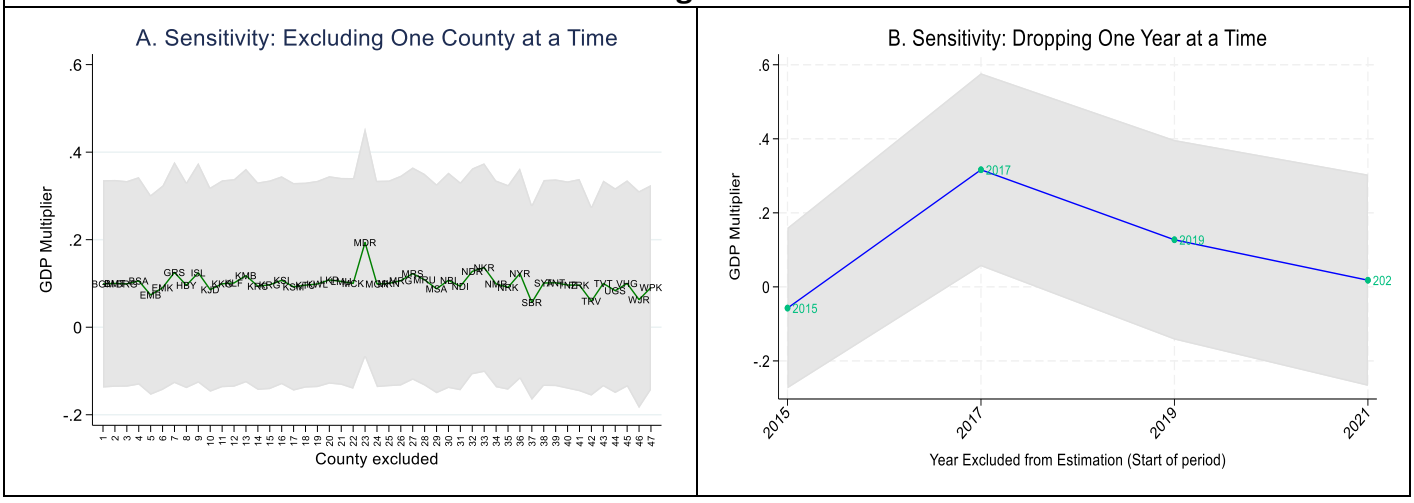
[16] That is growth above the 95th percentile are reduced to the 95th percentile, and growth below the 5th percentile are increased to the 5th percentile. This reduces the influence of extreme observations, without dropping those observations.

[17] Our baseline instrument is not defined when growth over 2013-15 is excluded which is why we don't drop those years.

Our final set of concerns is that possibly endogenous changes in other types of revenues might be affecting expenditures. Although equitable share revenues form the vast majority of county revenues , there are two other sources of revenues: own-source revenues and conditional grants. Own-source revenues are the endogenous as partially depend on the state of the economy.[18] Hence in Column 3 of Table 2 we add a control for the contemporaneous 2 year change in real own source revenues as a share of GCP. The multiplier on our main instrumented spending variable barely changes in size or significance. The coefficient on own-source revenues is large, but very imprecisely estimated, and so also insignificant.

Column 4 instead adds controls for conditional grants (2 year real change as a share of GCP). Conditional grants allocations from the national to county government for specific of projects, and so are also potentially endogenous if those projects are correlated with growth shocks at the county level.[19] Nonetheless, Column 5 shows that the main results are robust to controls for conditional grants, with the coefficient on conditional grants being insignificant themselves.[20]

Figure 5



Our results also seem to be reasonably homogenous across counties, which assuage concerns about correctly estimating heterogeneous treatment. Specifically, we split the sample in half at the median national poverty rate of around 34%, which we label poor counties for those above the median and rich counties for those below. We then allow multipliers to vary across the poor/rich

[18] Counties are responsible for taxes on property, entertainment and some other areas. However own-source revenues are typically small, only 8% of total county revenues (Boex and Smoke 2013, p304

[19] For example, a natural disaster might destroy a road (and reduce growth), and so the government might implement a conditional grant for reconstruction . For instance, the Road Maintenance Fuel Levy Fund operated as a conditional allocation to counties for repair and maintenance of county roads. [Division of Revenue Bill 2018/2019, Division of Revenue Bill 2024 historical note, Budget Policy Statement 2023 4.6.1 and Circular No. 25 (2020)].

[20] Adding both controls together leads to similar to results to adding them one-by-one, specifically

groups. Estimated multipliers are both around 0.1 for each group, and we fail to reject the restriction that multipliers are equal with a p-value of 0.97 (not reported)

6. Conclusions

This paper provides new evidence on the size of subnational fiscal multipliers in a low-income country context, using Kenya's 2013 fiscal decentralization as a natural experiment. The reform, mandated by the 2010 Constitution, constituted a large and plausibly exogenous fiscal shock, redistributing approximately 15% of national revenues—equivalent to 4% of GDP—across 47 newly created county governments via a formula-based allocation rule. In some of the poorest counties, this translated into increases in government spending of 15-35% of local GDP over just two years.

Despite the scale of the intervention, our main finding is that the subnational fiscal multiplier is close to zero and precisely estimated. Counties that experienced larger increases in spending did not, on average, record faster output growth. This result holds across a range of specifications; it remains robust to excluding influential observations, adding controls, a more conservative interpolation of pre-devolution county-level spending, and is supported both by graphical evidence and instrumental variable regressions.

The estimated multipliers are smaller than we expected. In a stylized benchmark where county governments hire new workers who were previously underemployed, and those workers spend their salaries on goods produced elsewhere, one might expect a fiscal multiplier near one. Given that public salaries are typically included in GCP, even modest local multiplier effects should be detectable.

Several potential explanations merit further investigation. As mentioned above, a near zero multiplier can be rationalized if (i) spending was either on goods/services produced elsewhere or government employees who exit private-sector jobs (crowding out) (ii) any second-round spending that leaks to goods/services produced elsewhere. This is possible given the counties with the largest expenditure increases are predominantly remote and agricultural, and likely only a small number of potential qualified administrators.

Another potential explanation is that the scale of the shock may have overwhelmed local administrative capacity, especially in historically underserved counties. Evidence of implementation lags in the first year of devolution supports this view. Because we use executed rather than budgeted spending, which should reduce this concern. However, there is some evidence of unusual types of spending in some of the most-affected counties. For example, in 2013-14 around 45% of Wajir county spending was on "Buildings and Structures" (2015 Kenya statistical abstract Table 179). If these were purchases of existing "Buildings and Structures",

rather than new construction, the growth impact could be small. Going forward, we hope to obtain more disaggregated information of spending types, to test this hypothesis.

Finally, measurement issues may be non-trivial. If GCP is poorly measured or partially interpolated the poorest counties local changes in activity may not be well captured in the data. This is also another fruitful area for future research.

Overall, this paper provides some of the first rigorous evidence on the short-run macroeconomic effects of subnational fiscal expansions in sub-Saharan Africa. The null result challenges optimistic assumptions about the effect of local fiscal stimulus of demand boosts on development in low-income regions.

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