

# Fiscal decentralisation and redistributive politics

## Evidence from Kenya

**Michael Mbate**

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Public finance and service delivery

### Abstract

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Fiscal decentralisation theories posit that ‘yardstick competition’ – comparison between peers – creates political incentives to enhance public service delivery. However, little is known about how party politics shape decision-makers’ incentives to compare each other’s performance across neighbouring localities. This working paper examines how the spatial distribution of support for political parties affects public spending across local counties in Kenya by examining the spillover effects of a partisan reform that increased spending in 10 of Kenya’s 47 counties. Using a spatial difference in difference design, this paper shows that policymakers in the counties that bordered the 10 reform counties engaged in ‘free riding’ – reducing their own public goods and services spending (relying on citizens accessing services in neighbouring counties). They re-allocated these resources to targeted goods, an effect that was stronger in the run-up to elections and for incumbents with a political stronghold. Further evidence from household surveys suggests that free riding reduced household’s access to public goods and services. The findings suggest that party politics may render the disciplinary effects of yardstick competition ineffective by generating incentives to benchmark performance vis-à-vis neighbours sharing the same party, rather than among neighbours under different parties.

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## **About the author**

Michael Mbate is a Senior Research Officer in ODI’s Development and Public Finance programme.

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# Acronyms

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<b>DD</b>	difference in differences
<b>GDP</b>	gross domestic product
<b>KSh</b>	Kenyan shilling
<b>CEE</b>	County Executive Committee
<b>MCA</b>	Member of the County Assembly
<b>MKAEB</b>	Mount Kenya and Aberdares Economic Bloc

# Executive summary

Most of the literature on fiscal decentralisation emphasises the importance of ‘yardstick competition’ in generating incentives to improve political performance. However, limited attention has been devoted to understanding the mechanisms that underlie this proposition – or the potential impact on households. Moreover, a contrasting theory suggests that if one jurisdiction increases its spending on public services, neighbouring jurisdictions may ‘free ride’ – reducing their own spending on the basis that residents can make use of improved services elsewhere.

This paper uses a newly compiled and digitised dataset on county-level spending in Kenya to illustrate how the spatial distribution of support for political parties can reduce the effectiveness of yardstick competition as a tool for political accountability. The analysis looks at spending in counties bordering those involved in the 2015 Mount Kenya and Aberdares Economic Bloc (MKAEB) and compares this to spending in counties located further away.

The reform, which saw 10 neighbouring counties<sup>1</sup> jointly agree to harmonise and improve spending on county-level infrastructure to raise local living standards,<sup>2</sup> provides a unique setting in which to investigate how border counties reacted to neighbouring spending increases as all of the reform counties were governed by the same political alliance.

This paper also examines the effect of accessibility costs on the incentives to free ride, the political motivations that may drive this free riding and what effect it has on proxies of household welfare.

## Key findings

All else being equal, **the reform led to a substantial decline (37%) in total spending on public services in border counties** relative to observationally similar counties located further away. This suggests that the spatial distribution of support for political parties can *reduce* the effectiveness of yardstick competition and *generate* incentives for bordering counties to free ride on the spending and service improvements of their neighbours.

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1 Embu, Kiambu, Kirinyaga, Laikipia, Meru, Murang’a, Nakuru, Nyandarua, Nyeri and Tharaka Nithi.

2 In theory, when local governments are granted fiscal autonomy and given a mandate to provide local public goods, it is often a common practice to coordinate spending policies to capitalise on economies of scale and to address common development needs (Dollery and Johnson, 2005; Neumark and Simpson, 2015; Alder et al., 2016).

**This difference is not only statistically significant but also economically substantial;**

given that each border county spends approximately 1.7 billion Kenyan shilling (KSh) per year (\$16 million) on public goods provision, the baseline estimates suggest that free riding reduced spending by 651 million KSh (\$5.9 million) to 704 million KSh (\$6.4 million) per year between 2016 and 2017.

The reduction in local government spending on public goods and services was **greater in counties where residents faced shorter distances and travelling time to reach services in the reform counties**. This suggests that accessibility costs are not only an important determinant of spending, but they also exert an effect that is independent of other socioeconomic and political factors that have received greater attention in the literature (Trounstone, 2015; Tajima et al., 2018).

**Border counties changed the composition of their expenditure**, decreasing spending on broad public goods and increasing their spending on categories of expenditure that are likely to be associated with patronage. High frequency data shows that these increases were greater before elections – suggesting a strategic motive to influence electoral outcomes – and for incumbents who had been in power for a long time and therefore may have already established clientelistic networks. A further examination of independent county audits shows that these spending shifts were associated with significant irregularities and thus largely inconsistent with the objective of improving the quality of governance.

Finally, the analysis finds a **substantial decline the quality of and household access to important public goods and services in border counties** after they reduced their spend on these goods and services following the neighbouring 2015 reform. This reduction occurred only for locally, not centrally, provided public goods, ruling out any unobserved county characteristics that determine both spending and accessibility as driving the results and suggesting that free riding was detrimental to the welfare of residents.

## Reflections

This paper makes three key contributions. First, it sheds light on the conditions under which fiscal decentralisation can improve public service delivery outcomes. The empirical results suggest that party politics can play an important role in shaping spending on public goods and services. Second, the analysis provides evidence on the role of electoral incentives in budgetary decision-making processes. Third, the results suggest that there is a strong link between spending policies and proxies of household welfare.

## Recommendation 1

Local governments should promote *budget credibility* by anchoring their spending policies on development objectives, plans and priorities. This might have the potential to reduce discretionary spending that is associated with targeted goods or the budget cycle.



## Recommendation 2

Independent entities, such as supreme audit institutions, should promote *budget accountability* by preventing the misappropriation of public funds through comprehensive audits that are disseminated to key stakeholders.

## Recommendation 3

Local governments as well as supreme audit institutions should promote *budget transparency* by improving access to information on spending policies. This may in turn enable citizens engage with policy processes and monitor local government actions, an aspect that may lead to better development outcomes.

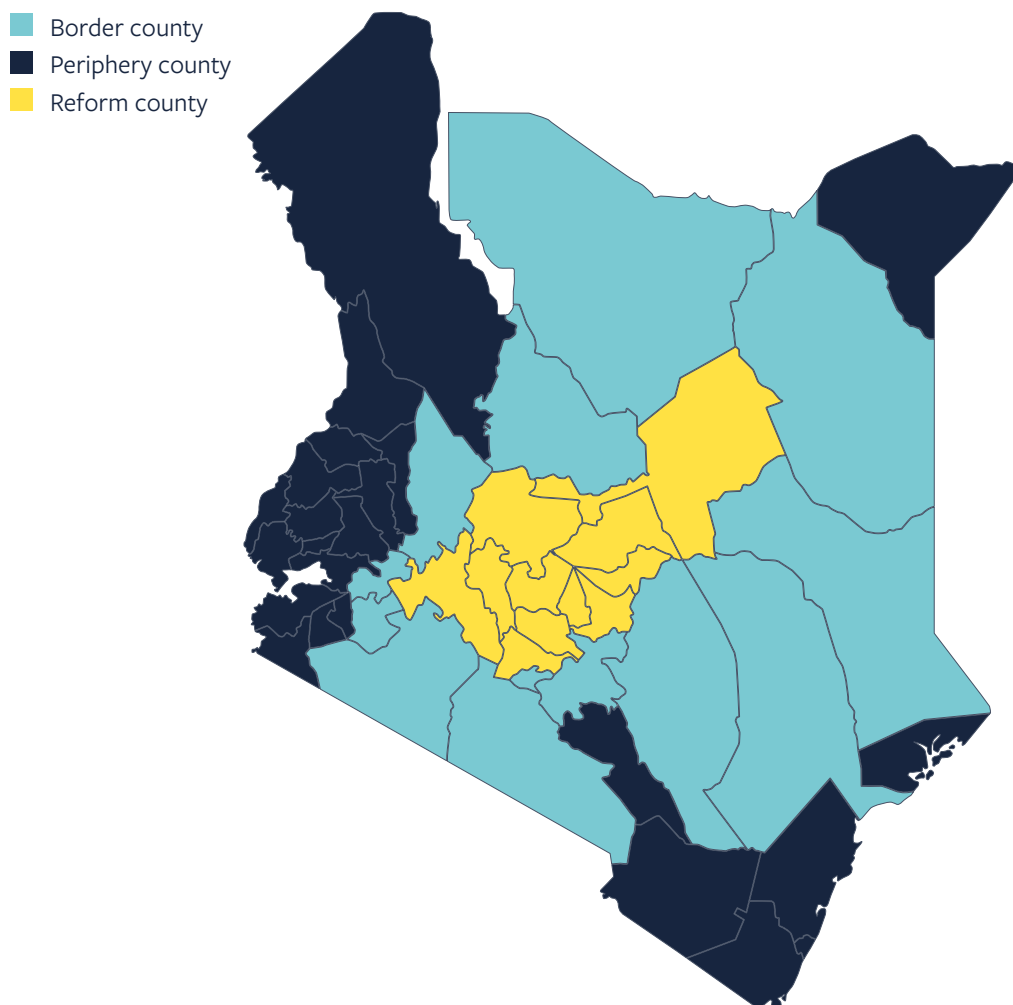
# 1 Introduction

The extent to which political factors influence the distribution of public resources is an important issue in the literature on fiscal decentralisation. There are two competing theories as to how spending decisions in neighbouring local governments might affect each other. The first theory is based on the notion of ‘yardstick competition’, whereby to evaluate the performance of their own locally elected politicians, citizens compare policies across local governments, and therefore spending in one jurisdiction should lead to improvements in another (Capuno et al., 2015). The second argues conversely that if one jurisdiction increases its spending on public services, neighbouring jurisdictions may ‘free ride’ – reducing their own spending on the basis that residents can make use of improved services elsewhere.

A growing body of literature has empirically examined yardstick competition hypothesis (Oates, 1972; 1999; Shah, 1994; Besley and Case, 1995; Grazzini and Petretto, 2017). However, the traditional approach does not incorporate the incentives and constraints that are generated by the local electoral system. Disentangling this relationship is challenging; local political factors, including internal party politics, are likely to shape the nature of yardstick competition across neighbouring localities. For example, voters might not compare the performance of local areas run by different parties – and even if they do, it may not influence their voting behaviour (Geys and Vermeir, 2008).

In this paper, I examine how counties in Kenya react to an increase in spending on public goods and services by their neighbours, and how this affects service delivery outcomes. There is some evidence in the literature of political considerations in redistributive decisions (Jablonski, 2014; Ejdemyr et al., 2018; Harris and Posner, 2019). However, these studies analyse spending patterns within a particular jurisdiction and do not consider their effects on neighbouring areas. Nor does such analysis explore the potential benefits or costs of redistributive policies on household welfare. I address this gap by testing for expenditure spillovers across local counties in Kenya using highly disaggregated budgetary and geo-referenced household-level surveys.

The analysis in this paper focuses on the Mount Kenya and Aberdares Economic Bloc (MKAEB) reform that followed Kenya’s transition to a new system of devolved governance in 2013 (Figure 1).

**Figure 1** Classification of counties in Kenya

Note: This figure shows the classification of counties based on their exposure to the reform.

Source: Author's own elaboration

The reform saw 10 neighbouring counties<sup>3</sup> jointly agree in 2015 to harmonise and improve spending on county-level infrastructure, health, roads, agriculture, public amenities, water and sanitation to raise local living standards. Importantly, this particular reform was based on partisanship: participating counties were governed by the same political alliance, unlike their neighbouring counties. This provides a unique setting in which to investigate whether neighbouring counties ('border counties') reacted to the increase in spending, as we can compare any changes in their spending with changes in counties located further away ('periphery counties'). On one hand, such reforms can generate positive spillovers and incentivise border counties to increase their own spending (Caldeira et al., 2015; Que et al., 2018). On the other hand, they can generate negative spillovers due to challenges associated with collective action (Akai and Suhara, 2013; Yang and Lee, 2018).

3 Embu, Kiambu, Kirinyaga, Laikipia, Meru, Murang'a, Nakuru, Nyandarua, Nyeri and Tharaka Nithi.

In theory, given that public goods are non-excludable (that is, individuals cannot be prevented from accessing or using them), and individuals are mobile across localities, then border counties that are close to reform counties are expected to internalise their benefits at lower costs. I therefore also examine the effect of accessibility costs on the incentives to free ride using the spatial layout of Kenya's road connectivity network and exploiting the variation in commuting time and distance between the border and the reform counties.

I also investigate the potential motivations behind the free riding behaviour. One possibility is that reducing spending on public services frees up resources that can be channelled to targeted goods, especially in highly clientelistic settings (Wantchekon, 2003; Green, 2011).

Finally, beyond budgetary indicators, I estimate the effect of free riding on the welfare outcomes of residents in border counties. Empirically, there are at least two main inferential challenges. To overcome the first one on measurement, I use detailed nationally representative surveys that contain information on indicators of access and quality of local public goods. I extract information at the community rather than the household level to mitigate potential biases arising from households' self-selection into the usage of specific public goods. To address the second concern that many other factors besides the reform might simultaneously influence spending and welfare, I compare differences in these indicators in border and periphery counties, prior to and after the reform.

## 2 Related literature and institutional context

### 2.1 Yardstick competition, party politics and public spending

Although many factors can shape the nature of yardstick competition across neighbouring localities, I focus on the notion that the spatial distribution of support for political parties could play a crucial role (Revelli, 2005; Geys and Vermeir, 2008). In traditional models of fiscal decentralisation, voters lacking information about the quality or performance of their own politicians can overcome this information asymmetry by observing policies in neighbouring localities (Tiebout, 1956; Besley and Case, 1995). Several studies provide empirical evidence in support of this proposition (Caldeira et al., 2015).

However, the extent to which neighbouring localities can act as yardsticks may be contingent on the spatial distribution of support for political parties. Strong political parties at the local level can dampen interparty competition. If parties without a stronghold in a particular jurisdiction are unable to effectively contest or win there, each cluster of localities under a political party acts as a separate political market. This in turn implies that politicians face significant competition from *within* their own political blocs and reduces incentives to benchmark policies against neighbouring localities under different political blocs. As such, increased spending on public goods in particular localities might not necessarily lead to positive spillovers to neighbouring localities. In fact, given that public goods are non-excludable, and residents are mobile, neighbouring localities could leverage increases in neighbouring spending to reduce their own and ‘free ride’. This may be especially true for counties in closer geographic proximity, as citizens face lower accessibility costs in terms of commuting time and distance.

Any corresponding reduction in public service spending can free up resources and induce substantial changes in spending in the border counties more generally. Existing literature suggests that local governments have at least two options. The first is to provide complementary public goods to those being provided by their neighbours. The second is to seek to provide targeted goods in exchange for votes – known as clientelism. The literature on electoral politics has long documented clientelism as crucial for political survival in developing countries (Diaz-Cayeros, 2008; Golden and Min, 2013), and Kenya in particular (Kiai, 2008; Githinji and Holmquist, 2012).

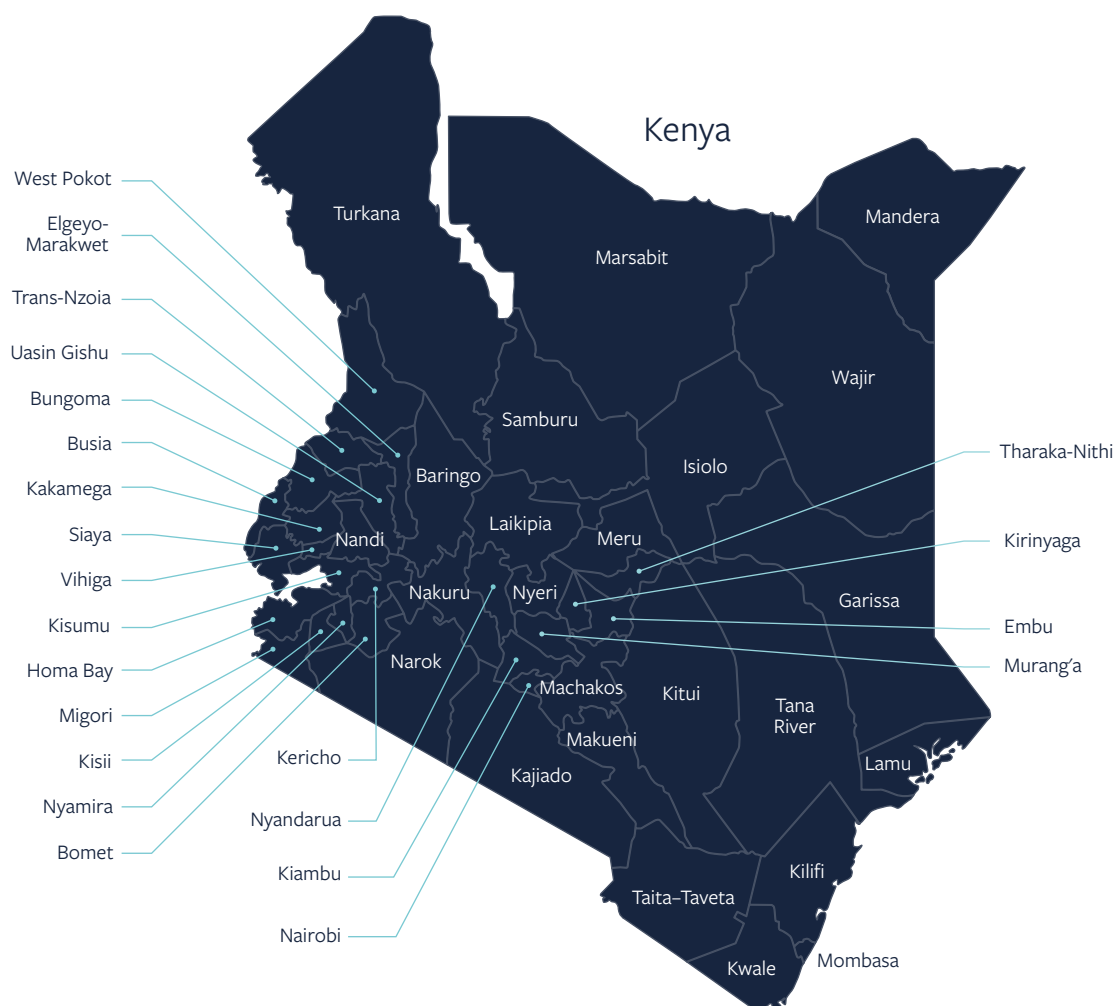
To analyse the factors that might enhance such clientelistic exchanges, I focus on two key variables. First, the political horizon of incumbent politicians matters for clientelistic spending (Green, 2011). Politicians who have been in power for a long time have had repeated interactions with voters, which means it may be clearer who to target and the types of benefits required (Dube et al., 2013). Over time, this enhances trust, which is crucial for maintaining clientelistic contracts and consolidating the payoffs from targeted spending (Keefer and Khemani, 2005).

Clientelistic spending is therefore likely to be higher for incumbent politicians who have been in power for a long period. Second, and as theories of opportunistic budget cycles suggest, targeted spending is likely to be driven by election timing, as it signals an incumbent's commitment to rewarding political supporters if re-elected (Rogoff, 1990; Capuno et al., 2015). As such, the shift to clientelistic spending will be higher during pre-election periods.

## 2.2 Fiscal transfers in Kenya

Intergovernmental transfers provide significant resources for public service delivery across counties in Kenya. The country's devolution system, introduced in 2013, led to the creation of 47 counties (Figure 2), with at least 15% of national revenues being allocated to counties in each year allocated using a pre-determined formula. Approximately 90% of a county's annual revenue consists of central government transfers. These resources are not only substantial but have increased over time, implying that spending policies can significantly affect both public service delivery and welfare outcomes (See Figure A1 in the Appendix).

**Figure 2** Administrative map of Kenyan counties



Source: Kenya Open Data Project

Kenya's constitution (2010) grants counties significant autonomy in terms of budgetary decisions and delineates powers and functions between the central government and local government. According to the constitution's Fourth Schedule, counties are mainly responsible for providing agricultural, health, cultural, urban planning, transport and infrastructure, and trade development services. To encourage economies of scale in public service provision, Article 189(2) allows counties to:

co-operate in the performance of functions and exercise of powers, as well as set up joint committees and joint authorities to coordinate policies and pool resources together for common investments. (GoK, 2010: 115)

This clause forms the basis for the MKAEB reform's creation.

Politically, a locally elected governor heads each county and is the most influential person in terms of budgetary decision-making. Governors are elected for a maximum period of two 5-year terms and thus are incentivised to maximise public resources use for political gain.<sup>4</sup> Governors run for public office under a specific political party. Figure 3 depicts the spatial distribution of the political alliances following the 2013 local elections. The figure shows spatial clustering, with political alliances having strong bases in different regions of the country. It shows a significant difference in the number of votes for incumbent governors in the MKAEB counties, compared to the second contender affiliated to a different political party. A novelty of the devolution reform in Kenya is that the geographic boundary of each county perfectly coincides with its bureaucratic and political jurisdiction, permitting the assessment of spending behaviour without aggregation outcomes across different administrative units.<sup>5</sup>

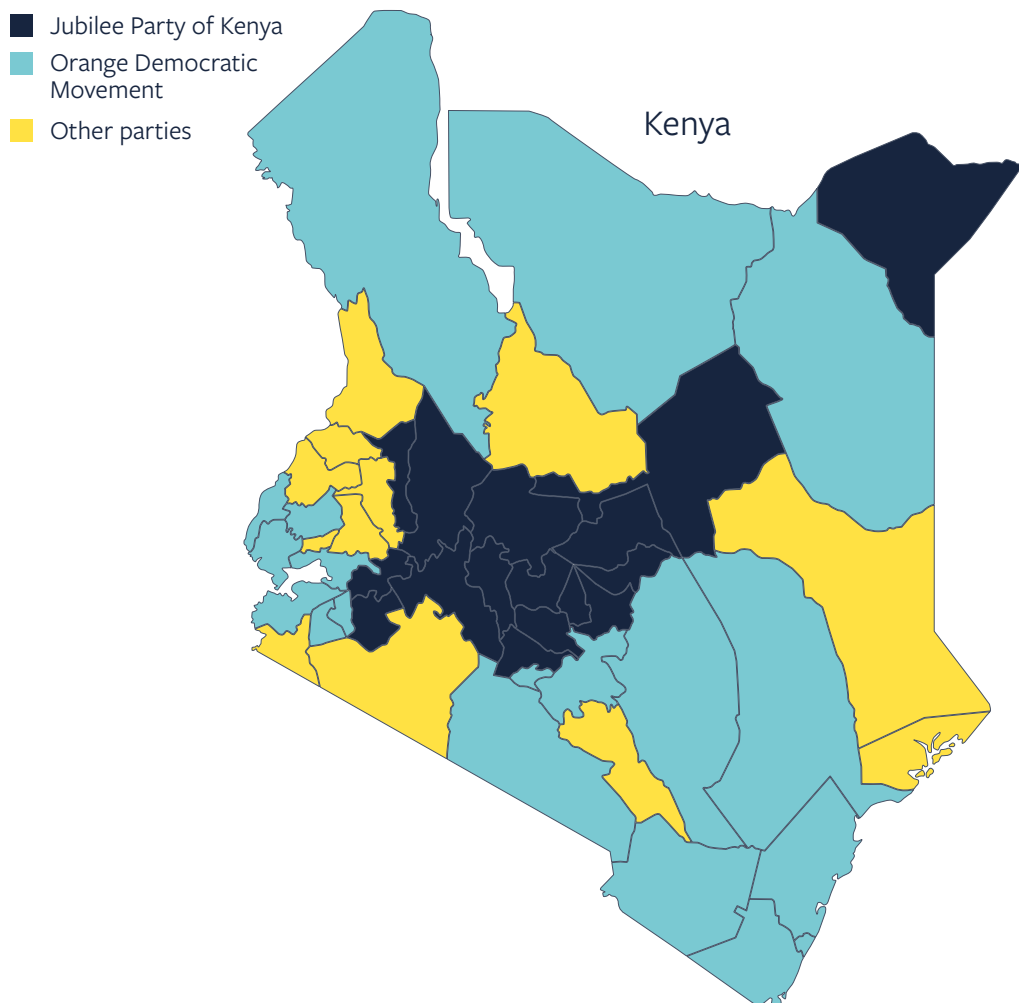
### 2.3 Mount Kenya and Aberdares Economic Bloc (MKAEB) reform

In 2015, 10 counties in Kenya<sup>6</sup> embarked on the Mount Kenya and Aberdares Economic Bloc (MKAEB) reform to promote economic and social investment by harmonising public spending policies (GoK, n.d). Formalised through a memorandum of understanding in February 2016, the reform aligned laws and regulations in order to facilitate trade and investments, leverage competitive and comparative advantages, and exploit economies of scale. It aimed ultimately to create employment and tap into technological development to expand economic frontiers. The bloc's formation was intended to spur economic growth through 'policy harmonization and resource mobilization' (GoK, 2019), aiming to mobilise 100 billion KSh (\$1 billion) (Ndung'u, 2018).

4 Each county also has a County Executive Committee (CEE) and Member of the County Assembly (MCA) who are also involved in endorsing annual county budgets. However, CEEs are nominated by governors, while most MCAs are party affiliated to the governor, making the governor influential in budgetary decisions.

5 The use of the pre-existing boundaries helps address concerns about boundaries correlating with county-level characteristics that might affect public spending.

6 Embu, Kiambu, Kirinyaga, Laikipia, Meru, Murang'a, Nakuru, Nyandarua, Nyeri and Tharaka Nithi.

**Figure 3** Spatial distribution of political alliances in Kenya

Source: Author's own elaboration

They focused on increasing spending in seven clusters: (1) productive sectors such as agriculture, agribusiness, tourism and industrialisation; (2) social sectors such as healthcare and education; and (3) enablers such as infrastructure, water and resource management, financial services and information and communication technology. In each financial year, governors are required to harmonise and increase spending in these categories, although there are no explicit targets (Council of Governors, 2016). County governors provide oversight and political guidance while the Economic Development Council, created as part of the bloc's governance structure, oversees the development agenda. Other entities include a Secretariat drawn from county members that oversees preparing sector strategies and policies (Council of Governors, 2020).

Unlike most reforms targeted at underperforming regions by the central government, the MKAEB was established by governors largely based on party alliance – the Jubilee Party of Kenya. Entrenched in its party manifesto and central to its 2013 election campaign, the party had pledged to pool resources across counties to enhance the large-scale provision of public goods and services. Indeed, between 2013 and 2017, there was a significant spike in spending



across the 10 reform counties. For example, expenditure on public goods and services increased steadily increased from 6.9 billion KSh in 2014 (\$69 million) to 18.2 billion KSh (\$182 million) in 2016 while total expenditure increased from 38.8 billion KSh (\$388 million) in 2013 to 67.3 billion KSh (\$678 million) in 2017. Given this unprecedented increase in spending, I assess how the neighbouring counties reacted by adjusting their spending patterns.

## 3 Methodology

### 3.1 Empirical challenges

There are several methodological challenges to testing for spillover effects. The first relates to defining what constitutes a neighbour (Gibbons and Overman, 2010). Different studies conceptualise ‘neighbourhood’ along various social economic and political dimensions, often with mixed or inconclusive results (ibid.; Yu et al., 2016). The second challenge is the lack of a well-defined area of spending impact, which in turn makes it difficult to identify an appropriate unit of analysis. The third challenge is simultaneity bias: unobserved determinants of spending decisions might be correlated across localities. For instance, if a county reacts to its neighbours’ spending decisions, then spending decisions are jointly determined in equilibrium (Atella et al., 2014). The fourth challenge relates to causal identification: many factors could drive differentials in spending across counties.

This analysis employs an empirical strategy to overcome these identification challenges. First, I define ‘neighbours’ by means of a spatial weight matrix that attaches higher weights to counties that are geographically close to the MKAEB counties. Second, I leverage the fact that the reform was deliberately targeted at well-defined counties, which means this paper can provide a clear delineation of designated and non-designated localities to establish a clear unit of analysis. Third, I address simultaneity bias by taking advantage of the fact that border and periphery counties are in close geographic proximity and thus likely to share unobserved traits – such as preferences for particular types of public goods and services – that might correlate with spending behaviour. Additionally, the spatial distribution of the counties mitigates any potential location-specific confounders, such as weather shocks, which could explain cross-county differences in spending. To elicit causal inference, I use a spatial difference in difference design. Identification comes from comparing changes in public expenditure in border counties to periphery counties before and after the reform in 2015.

### 3.2 Data

The empirical analysis set out in this paper uses data from a variety of sources. First, budgetary data on county-level revenue and spending from annual county government budget implementation review reports published by the Office of the Controller of Budget in Kenya. These are the most comprehensive and official sources of data harmonised across all counties. The census data that provides information on county-level socioeconomic and demographic indicators came from several editions of the country-wide census and economic surveys published by the Kenya National Bureau of Statistics. County-level electoral outcomes were obtained from reports published by Kenya’s Independent Electoral and Boundary Commission. The entire dataset is a balanced panel of 47 counties for the period between FY13/14 and FY17/18, looking at all counties both before and after the 2015 reform.

### 3.3 Defining the counties to be studied

#### 3.3.1 Treatment group (border counties)

Given that the focus of this analysis is on assessing spillover effects, rather than the direct effect of the reform, the treated group is defined as the 14 counties that share a geographical boundary with those implementing the 2015 MKAEB reform.<sup>7</sup> This raises two methodological issues. First, the interpretation of the results is highly dependent on the assumed spillover structure (Baskaran, 2014). One potential concern is that some counties that are geographically close but not contiguous to the reform counties could have been directly affected by the reform. Excluding these counties from the treatment group could lead to an underestimate of the reform's true effect. This concern is mitigated by the extensive body of empirical studies that show that spillovers are strong across neighbouring geographical units and that they tend to decay with (increasing) distance (Lychagin et al., 2016). Moreover, changes in spending on public goods and services in the reform counties are highly uncorrelated with those of counties that are geographically proximate but do not share a boundary (the correlation coefficient estimate is 0.024).

#### 3.3.2 Control group (periphery counties)

The second potential concern is that the estimated coefficients are local average treatment effects based on local boundaries. This implies that the internal validity of the results partly depends on whether the border counties are comparable to the periphery counties. To generate a control group, I used two complementary approaches: (1) selecting all remaining 23 periphery counties as possible controls;<sup>8</sup> and (2) using propensity score matching (PSM) technique to match border counties with periphery counties based on the similarity in their propensity score using several indicators such as poverty (Gini coefficient), a multidimensional index of poverty, population density (all measured at baseline using the 2009 census) and time-varying indicators such as log population density and per capita gross domestic product (GDP) for 2013 to 2017. Although this slightly reduces the sample size,<sup>9</sup> it produces a set of control counties that have similar attributes to the border counties.

The validity of the PSM depends on the assumption that there are no unobserved differences between the treated and control counties that are correlated with potential outcomes (Rosenbaum, 2007; Gertler et al., 2011). I validated this assumption using several tests. First, I examined whether the counties within the control group generated by the PSM have similar

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7 The border counties are Marsabit, Wajir, Samburu, Baringo, Garissa, Kericho, Tana River, Bomet, Kitui, Narok, Nairobi, Machakos, Isiolo and Kajiado.

8 There are 47 counties in the sample. The 10 MKAEB counties are dropped from the analysis. This leaves a total of 37 counties (14 border counties and 23 periphery counties).

9 This reduces the sample by 15%.

characteristics to the treated counties; a covariate balance test yields statistically insignificant differences across several economic demographic and political variables that have theoretically been found to affect spending decisions (see Table A1 in the Appendix). Second, I examined the distribution of the estimated propensity score, finding strong evidence of common support, as indicated by the significant overlap of the scores in the treated and control counties (see Figure A2 in the Appendix).

To compare the two sets of control counties (the 23 periphery counties and the 19 PSM counties), I conducted a bias reduction test. The results show the distribution of bias between the treated and control groups before and after matching (see Table A2 in the Appendix). For each of the covariates, the bias obtained from the periphery counties was higher than the control group generated using the propensity score. After matching, the matched sample had a mean bias of 12.4, which was significantly lower than that of the unmatched sample (45.8), suggesting that the counties obtained by the PSM technique serve as a better control group.

### 3.4 Empirical specification

In this section, I test the hypothesis that border counties free ride on neighbouring reform counties' increased expenditure by examining whether there was a differential effect in spending on public goods and services in the border counties compared to the control group. The identification strategy relies on a difference in differences (DD) approach.

The specification takes the form:

$$y_{i,t} = \alpha_i + \alpha_t + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 (Post_t * Treat_i) + \beta_4 X_{i,t} + \varepsilon_{i,t} \quad (1)$$

where  $y_{i,t}$  is log expenditure on public goods and services as a share of the total spending for county  $i$  in year  $t$ ,  $Post_t$  is equal to 1 if year  $\geq 2015$  and 0 otherwise.  $Treat_i$  is an dummy variable that takes the value of one if county  $i$  was treated (defined as sharing a boundary with at least one of the reform counties),  $Post_t * Treat_i$  captures the interaction effect of being treated after the reform,  $X_{i,t}$  is a vector of covariates and  $\varepsilon_{i,t}$  is the error term. County effects  $\alpha_i$  are included to capture differences across counties – historical or geographical – that are constant over time, while year effects  $\alpha_t$  absorb differences over time that are common to all counties. By including fixed effects, I control for the average differences across counties in any observable or unobservable predictors, such as differences in geographic, history or culture. The variation comes from within-county changes by comparing spending patterns in border and periphery counties before and after the reform. The DD parameter of interest is  $\beta_3$  and it compares the changes in public expenditure before and after 2015 in the border counties to expenditure changes in the control group.

$X_{i,t}$  is a vector of time varying county-level social economic indicators. These include log per capita GDP, as a proxy of local economic conditions, log population density as a measure of demand for

public goods, revenue per capita as a proxy of a county's capacity to mobilise resources locally.<sup>10</sup> Overall, these variables aim to account for the possibility that border and periphery counties are systematically different in time-varying factors that might affect spending patterns. Finally, standard errors are clustered at the county level to account for the within-county correlation (Duflo et al., 2004).<sup>11</sup>

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10 Governor-level controls were not included due to lack of variation: all incumbent governors were male with similar levels of education and age.

11 Due to the potential small sample, the main results are also re-estimated using bootstrap standard errors of 500 replications.

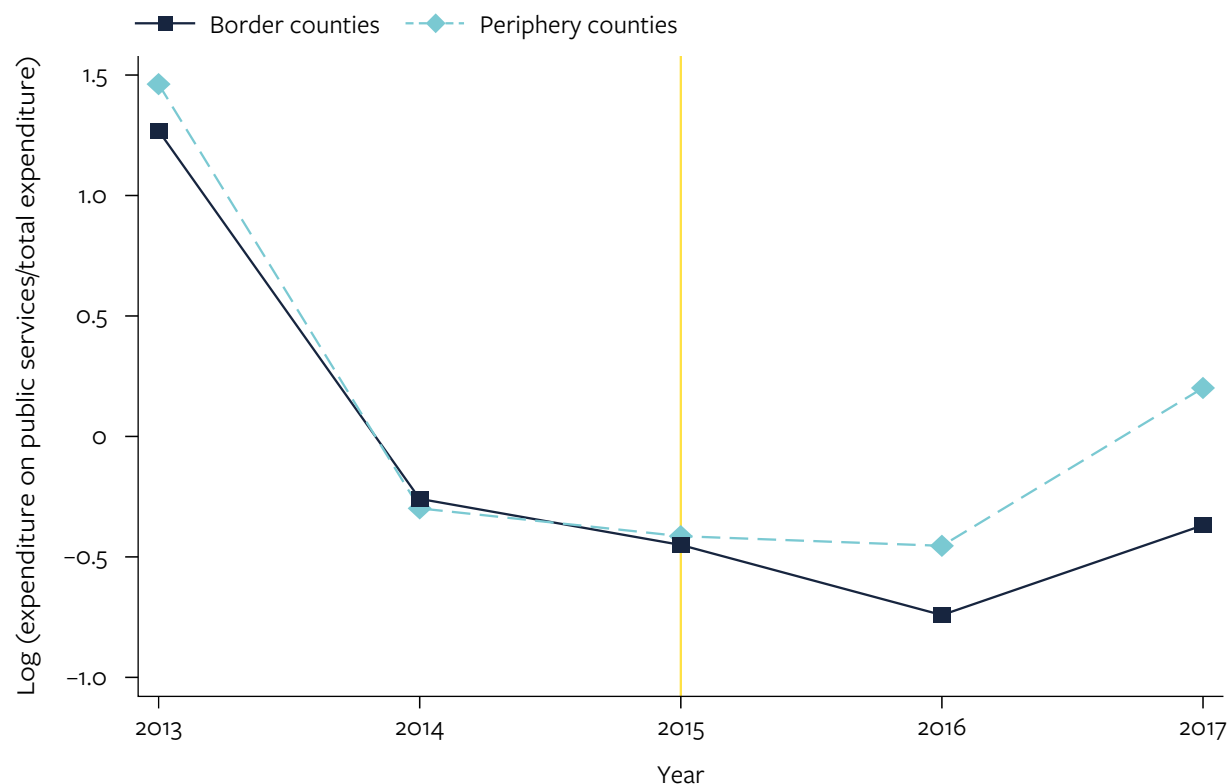
## 4 Findings

### 4.1 Evidence of free riding by counties bordering those that were part of the 2015 MKAEB reform

Combining the DD technique with PSM, this analysis finds that border counties disproportionately reduced their expenditure relative to those located further away. Supporting this interpretation is the fact that this effect was higher in border counties where residents faced lower accessibility costs – e.g. to reach or access services in neighbouring counties. These results may be considered robust according to a comparison of expenditure trends before the MKAEB reform (which shows no difference between border and periphery counties), which confirms that spending reductions in border counties were not caused by lower tax revenues due to economic activity shifting to the MKAEB reform counties, and the fact that results are not driven by urban counties or by anticipatory effects.

As Figure 4 illustrates, there is a clear reduction in spending in border counties relative to periphery counties that coincides with the 2015 reform and this reduction is persistent over time; while it slightly improved in 2017, it was still below its pre-reform level in 2014.

**Figure 4** Comparing border county and periphery county spending changes, 2013–2017



Note: This figure plots spending on public goods and services in the border (treated) and periphery (control) counties, as a share of total expenditure, on a logarithmic scale. The vertical line represents the year the MKAEB policy was initiated.

The regression results in Table 1 are consistent with these insights and show the main DD estimates obtained from equation 1. The results in columns (1) to (3) are derived from using the periphery counties as the control group, while those in columns (4) to (6) are estimated using control counties obtained through the matching technique. Both approaches, however, yield results that are similar in magnitude. Columns (1) and (4) provide simple differences estimated without the inclusion of either year or county fixed effects or any controls. In columns (2) and (5), the specification only includes year and county fixed effects, while those in columns (3) and (6) include both fixed effects as well as the whole set of control variables.

**Table 1** Evidence of spending reduction in border counties

Dependent variable: Log(public expenditure/total spending)						
	Periphery counties			Matched counties		
	(1)	(2)	(3)	(4)	(5)	(6)
Post X treat	-0.973** (0.365)	-0.973** (0.367)	-0.942** (0.376)	-0.944** (0.473)	-0.939** (0.477)	-0.895** (0.446)
Observations	180	180	180	153	153	153
Number of counties	36	36	36	33	33	33
R-squared	0.065	0.100	0.065	0.110	0.16	0.220
Year fixed effects	No	Yes	Yes	No	Yes	Yes
County fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Note: This table shows the effect of the MKAEB reform on spending on public goods and services in border and periphery counties. The dependent variable is the log of public expenditure as a share of total spending. Control variables include log per capita GDP, log population density and revenue per capita. Standard errors are clustered at the county-level and reported in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

The results from the baseline specification provide strong evidence of free riding in spending on public goods due to the MKAEB reform. Across the different specifications, the DD estimate is negative and statistically significant at the 5% level. This suggests that, relative to control counties, border counties reduced their share of spending on public goods and services by approximately 37–40% following the implementation of the MKAEB reform. The effect is not only statistically significant, but also economically substantial; given that the average border county allocated 1.7 billion KSh per year (\$16 million) to public goods provision, the DD estimate suggests that, everything else being equal, free riding led to a reduction of approximately 651–704 million KSh (\$5.9–6.4 million).

## 4.2 The effect of distance and travel time between counties

According to theories of free riding, if individuals can travel across counties to access public goods, then shorter distances may reflect lower transportation costs and time (Saraiva and Costa, 2012; Akai and Suhara, 2013). Assuming that accessibility costs increase in line with distance, differences in commuting distance and time should affect the degree to which counties free ride on the spending of their neighbours. To test this, I use two sources of spatial variation to assess how differences in accessibility between the border and reform counties affected the reduction in spending on public goods and services.

The first source of variation is derived from differences in the length of a county's boundary. Using ArcGIS, I construct a variable that captures the length of the shared boundary (in kilometres) between each border county and its contiguous county in the reform and then normalise this measure using the total length of the county's boundary. While simple in nature, the assumption is that border counties that share a longer boundary with the reform counties should have a higher degree of accessibility given the porous nature of local boundaries.

The second source of variation relies on the transport connectivity between counties. In Kenya, roads are the dominant mode of transport. I overlay the road network (see Figure A3 in the Appendix) and county administrative border shape files and construct two variables. The first captures the distance (in kilometres) between the most densely populated areas in the border counties and the nearest main town in the reform counties. The second variable captures the time (in minutes) that it takes category A, B and C vehicles (mainly public buses, vans and private cars) to travel from the most densely populated area in the border counties to the nearest main town in the reform counties.<sup>12</sup>

I then estimate the following specification:

$$y_{i,t} = \beta_1 Post_t + \beta_2 Distance_{ij} + \beta_3 (Post_t * Distance_{ij}) + \varepsilon_{i,t} \quad (2)$$

where  $y_{i,t}$  denotes public expenditure on public goods and services as a share of the total spending for each border county  $i$  in year  $t$  and  $Post_t$  is an indicator variable equal to 1 if the year  $> 2015$ . The variable  $Distance$  is a measure of spatial variation and is proxied by three different indicators. The first is a binary variable that equals 1 if county's  $i$  shared boundary with the contiguous reform county  $j$  is lower than the median distance, and 0 otherwise. The second is a continuous measure of the road distance in kilometres between the main town<sup>13</sup> in each border county with the closest

12 I construct this by combining geo-referenced road network data that contains detailed information on both the physical and visual conditions of the road (such as road class, width, number of lanes, surface type and condition) with data on official speed limits from the Kenya Road Board.

13 I assume that the public goods and services provided by the reform are geo-located in the main town in each of the reform counties (Banerjee et al., 2007).



town in the nearest reform county, and the third is a time indicator variable that captures the shortest travelling time by public transport between each border county and the nearest reform county. The parameter of interest is denoted by  $\beta_3$  and captures the effect of spatial proximity on spending patterns in the border counties. Due to the small sample size, standard errors are calculated using a bootstrap procedure with 1,000 replications.

Table 2 reports the main estimates obtained from estimating equation 2. The interaction term compares changes in the share of expenditure on public goods and services between border counties that have higher spatial proximity to the reform counties to those with lower proximity. Across the different specifications, this coefficient is negative and statistically significant, revealing the important role of spatial characteristics in explaining the variation in allocative decisions of local politicians. In column (1), each additional kilometre of distance reduces spending by 0.22 percentage points, while the result in column (2) shows a higher reduction for counties with a shorter road distance to the reform counties (20 percentage points). In column (3), the reduction in spending seems to be higher in border counties with a lower travelling time. Overall, these findings suggest that spatial proximity shapes allocative decisions through accessibility costs.

**Table 2** Heterogeneity in free riding

Dependent variable: Expenditure on public goods and services/total expenditure			
	(1)	(2)	(3)
Post X distance	-0.215**	-0.200**	-0.212**
	(0.101)	(0.29)	(0.106)
Controls	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	65	65	65
Mean of dependent variable	0.523	0.554	0.765

Note: Robust standard errors in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

This table reports the effects of spatial proximity on public spending in border counties. Column (1) presents the results where the variable Distance is a binary variable equal to 1 if the shared length of the boundary between border and MKAEB counties is lower than the median distance (150 kilometres). In column (2), the variable Distance is a continuous measure of the road distance between the main town in each border county and the nearest main town in the MKAEB county. In column (3), the variable Distance is a continuous measure of the average time (in minutes) between the main town in each border county and the nearest MKAEB county.

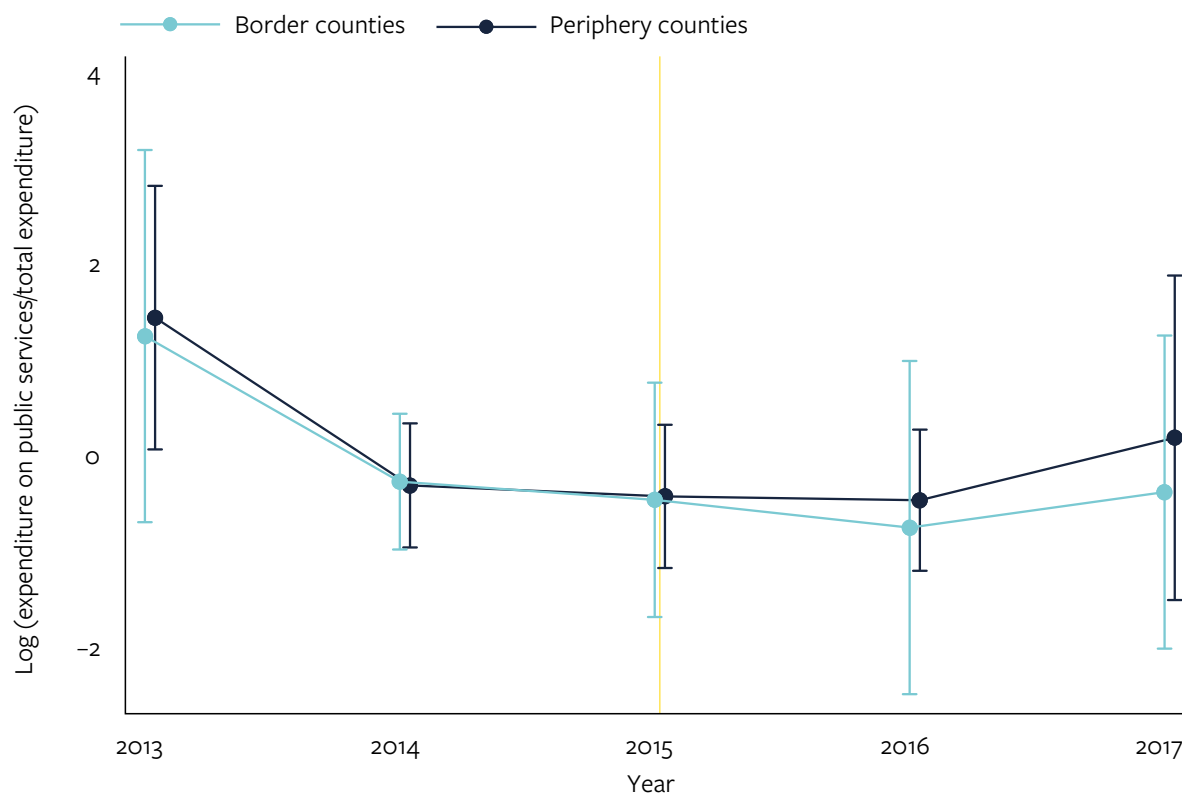
### 4.3 Testing the robustness of the findings

This section provides evidence to support the attribution of this free riding on the part of border counties to the reform. It does so by ruling out: (1) systematic differences in spending patterns between border and control counties before 2015; (2) the use of alternative measures of expenditure as the main dependent variable; (3) reduction in local revenue due to displacement effects; (4) sub-sample analysis; (5) anticipatory effects of the reform; and (5) capacity constraints that might have led to reduced spending in border counties.

#### 4.3.1 Parallel trend assumption

The primary assumption underlying the baseline DD estimates is that spending would have exhibited similar trends in both border and periphery counties in the absence of the MKAEB reform. I test for this by comparing spending for the two sets of counties in 2013 and 2014. If the main results were being influenced by pre-existing differentials in spending, the trends could be expected to follow a different trajectory. However, public expenditure as a proportion of total expenditure was broadly the same in both border and periphery counties and the reduction in border county spending seems to arise only when border counties were exposed to the 2015 reform (Figure 5).

**Figure 5** Testing the parallel trend assumption, 2013–2017



Note: This figure depicts the parallel trend assumption. The y-axis measures the log of expenditure on public services as a share of total expenditure. The vertical line represents the period when the MKAEB reform was initiated.

### 4.3.2 Alternative measures of transfers

A second potential concern is that the baseline results are sensitive to the measurement of the dependent variable. To address this, I re-estimate equation 1 using alternative measures of spending, as shown in Table 3. Column (1) uses development expenditure (KSh, millions) while the results in column (2) scale the total amount of development expenditure with a county's total population. The results remain robust to these changes. In columns (3) and (4), I disaggregate county revenues into conditional transfers that are earmarked for specific purposes and equitable share where governors have full discretion in allocative decisions. The results show that the reform induced a reduction in spending only in the equitable share component but did not have a substantial effect on earmarked (conditional) transfers.

**Table 3** Alternative measures of county spending

Dependent variable	Development expenditure (KSh, millions)	Development expenditure per capita	Conditional transfers	Equitable share
	(1)	(2)	(3)	(4)
Post X treat	-39.832** (17.940)	-1.674** (0.320)	-0.387 (0.494)	-0.506** (0.260)
Observations	153	153	153	153
Number of counties	33	33	33	33
R-squared	0.13	0.65	0.18	0.19
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: This table presents the effect of the MKAEB reform on alternative measures of spending. Control counties as defined using the propensity score matching technique. All the specifications control for log per capita GDP, log population density and revenue per capita.

Standard errors are clustered at the county-level and reported in parentheses.

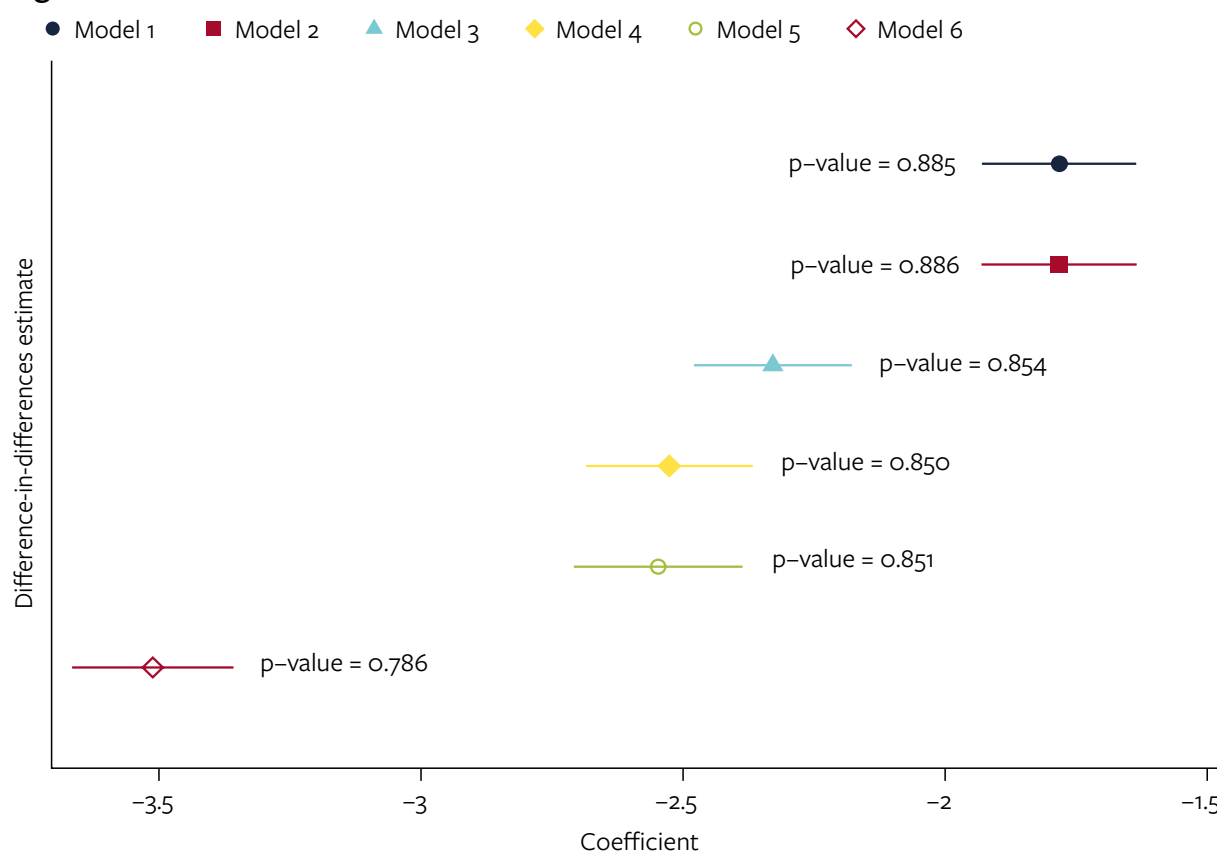
\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

### 4.3.3 Displacement effects

One important concern is that the reform might have led to a reduction in spending in border counties by encouraging households or businesses to relocate to the MKAEB counties. If this were to happen, the reduction in border county spending might be mechanically explained by a reduction in tax revenue. However, there are three reasons why this interpretation is less plausible. First, I assess the short-term effect of the reform, which makes it unlikely that residents permanently relocated within two years. Second, several studies on internal migration in Kenya show that residential mobility is limited due to the nature of the land markets, significant

relocation costs and ethnic tensions (Platteau, 2000). Third, I conduct a formal test to examine the effect of the reform on local revenues by re-estimating equation 1 and replacing the dependent variable with local revenues. As illustrated in Figure 6, the corresponding estimates are small and statistically insignificant.

**Figure 6** Effect of the reform on local revenues

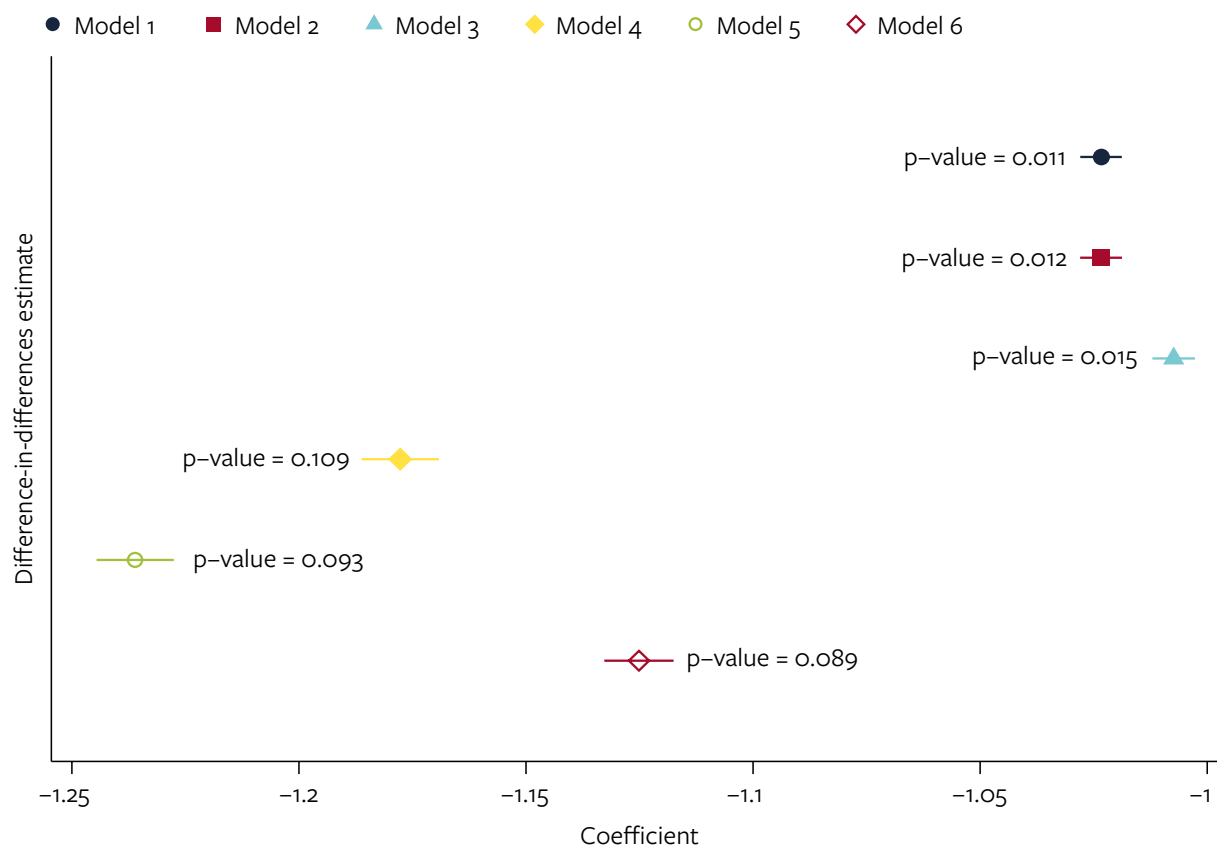


Note: Figure 6 shows the estimates of the reform on local taxes. The estimates in models 1 to 3 use periphery counties as controls while models 4 to 6 use control counties obtained through matching. Models 1 and 3 are the basic DD with no controls or year/county fixed effects. Models 2 and 4 further include year and county fixed effects. Models 3 and 6 further include controls log per capita GDP, log population density and revenue per capita.

#### 4.3.4 Urban counties

To further ensure that the results could not be driven by a small set of counties that have different budgetary structures or spending priorities, I re-estimated spending on public goods and services excluding all urban counties from the sample.<sup>14</sup> This modification does not alter the main finding.

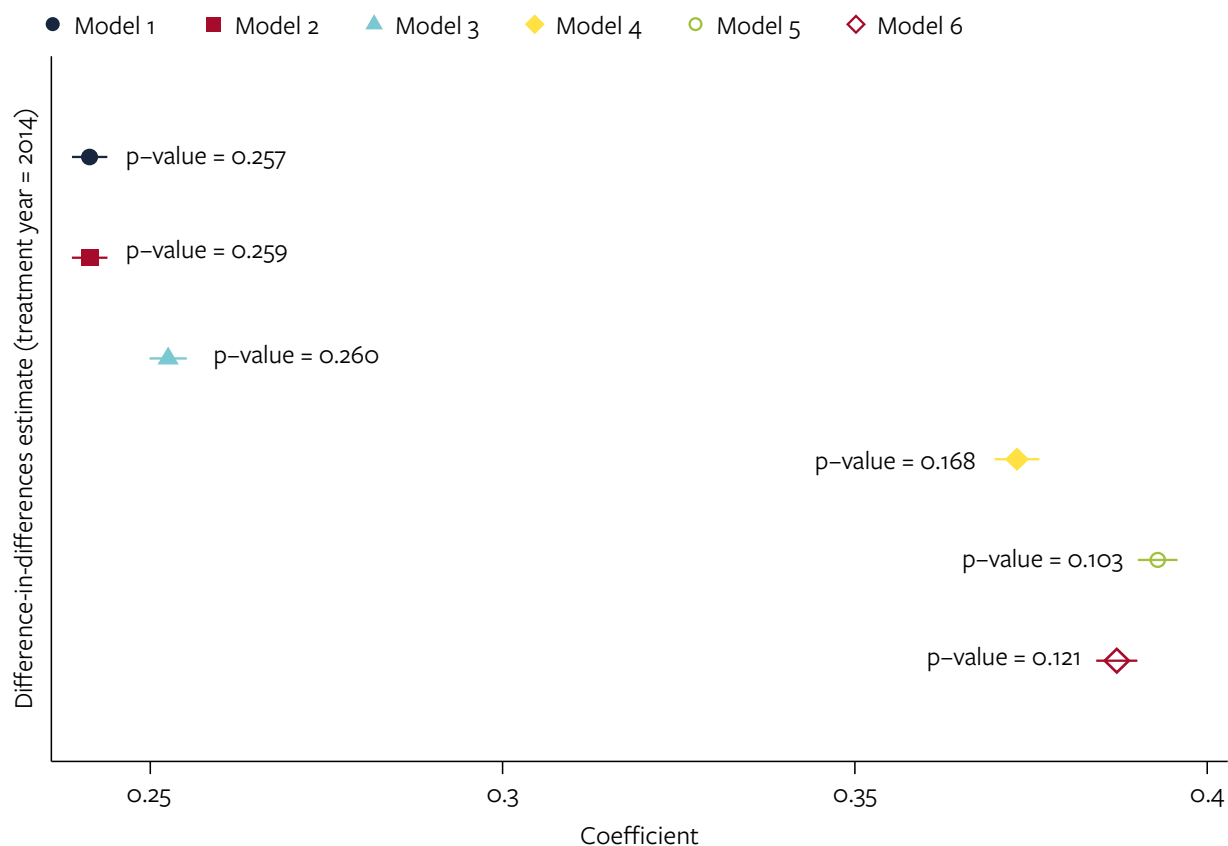
<sup>14</sup> These counties are Kisumu, Mombasa, Nairobi and Nakuru.

**Figure 7** Spending on public goods and services excluding urban counties

Note: This figure shows the estimates of the reform on spending on public goods and services. The estimates in models 1 to 3 use periphery counties as controls while models 4 to 6 use control counties obtained through matching. Models 1 and 3 are the basic DD with no controls or year/county fixed effects. Models 2 and 4 further include year and county fixed effects. Models 3 and 6 further includes controls log per capita GDP, log population density and revenue per capita.

#### 4.3.5 Anticipatory effects

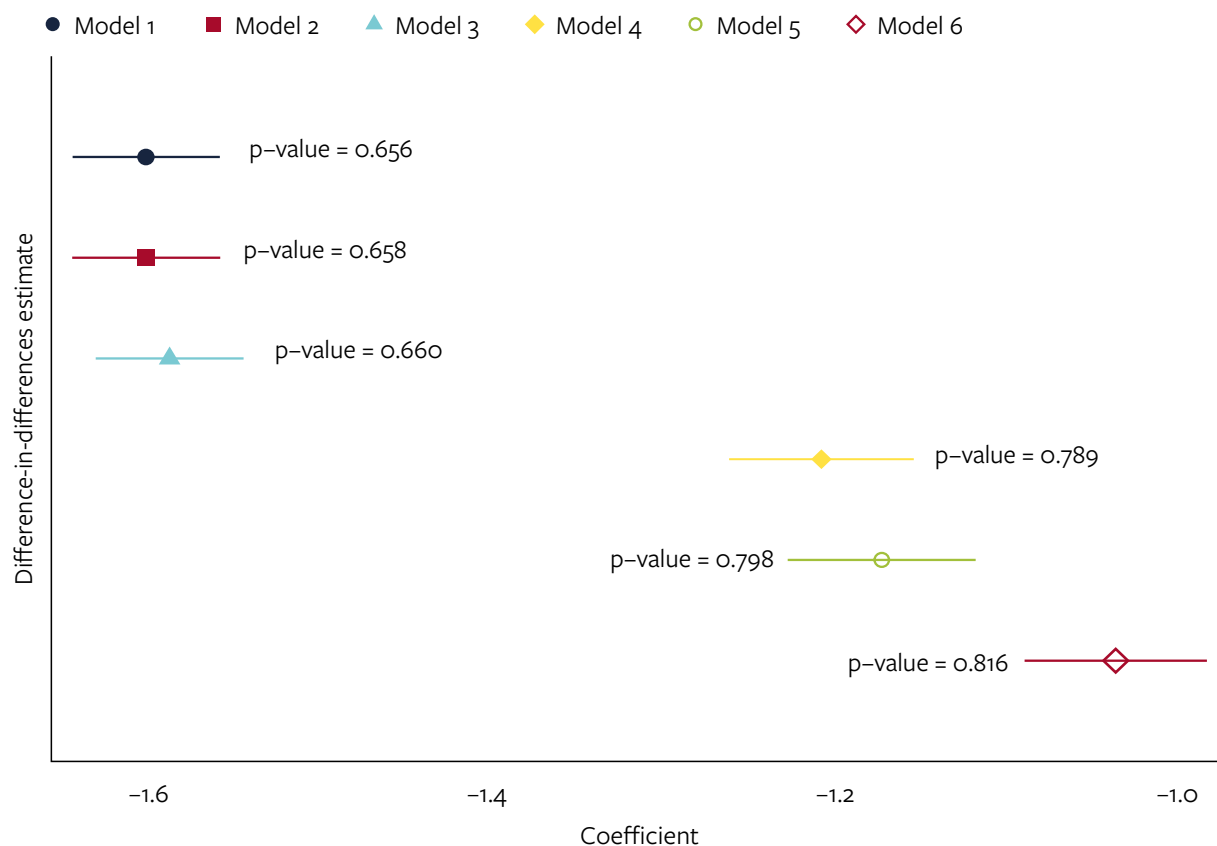
In theory, information about the reform, rather than the reform itself, could have incentivised border counties to strategically reduce their spending. To directly test this possibility, I re-estimated equation 1 and redefined the treatment year – that is, the year of the reform – as 2014, one year before the actual implementation of the MKAEB reform. As Figure 8 shows, this is unlikely to explain the spending reduction in border counties.

**Figure 8** Testing for anticipatory effects

Note: This figure shows the estimates of the reform on spending on public goods and services. The treatment year is a placebo, 2014, rather than the actual one 2015. The estimates in models 1 to 3 use periphery counties as controls while models 4 to 6 use control counties obtained through matching. Models 1 and 3 are the basic DD with no controls or year/county fixed effects. Models 2 and 4 further include year and county fixed effects. Models 3 and 6 further include controls log per capita GDP, log population density and revenue per capita.

#### 4.3.6 Capacity constraints

Finally, to test whether the reduced spending could be attributed to a lack of in border counties' bureaucratic capacity to spend, I re-estimate equation 1 replacing the dependent variable with the budgetary absorption rate. This is calculated as the share of resource spend relative to the budget. The results show that there is no significant difference between border counties and periphery counties in terms of their capacity to spend resources (Figure 9).

**Figure 9** Testing for capacity constraints

Note: This figure shows the estimates of the reform on budgetary absorption rates. The estimates in models 1 to 3 use periphery counties as controls while models 4 to 6 use control counties obtained through matching. Models 1 and 3 are the basic DD with no controls or year/county fixed effects. Models 2 and 4 further include year and county fixed effects. Models 3 and 6 further include controls log per capita GDP, log population density and revenue per capita.

## 5 Analysis of changes in the composition of budgets

This chapter provides suggestive evidence that border counties seemed to shift their spending from public to private goods. This is reinforced by two findings: that such spending is higher for governors who have previously held a local political position (and thus are likely to have more established patronage networks); and the timing of these shifts in relation to local elections.

To explore how spillovers affected the composition of public spending among border counties, I disaggregated public expenditure into categories that reflect targeted spending such as travel and meeting allowances, salaries and wages, the purchase of office equipment, and the acquisition of assets. I then re-estimated equation 1, where the dependent variable is expenditure on targeted goods, expressed as a share of total expenditure. The results reveal that the reform had a significant effect on the composition of expenditure in border counties as compared to the composition of spending in counties located further away (Table 4). The point estimates in columns (1) to (4) are positive and statistically significant at conventional levels, suggesting a shift in spending patterns towards targeted goods.

**Table 4** Difference-in-differences estimates of the effect of the reform on spending composition in border counties

	Periphery counties		Matched counties	
	(1)	(2)	(3)	(4)
Post X treat	0.200** (0.08)	0.181** (0.09)	0.232** (0.16)	0.221** (0.07)
Controls	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Observations	150	150	180	180

Note: This table reports the effects of the reform on the composition of public spending in border counties. The interaction term (post x treat) is the standard difference in difference estimate. Post is a dummy indicator for the years 2015 to 2017 while treat is a dummy variable that is equal to 1 if a county was treated (that is, if it were to share a boundary with any of the MKAEB counties). Robust standard errors in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1



## 5.1 Clientelistic spending

To provide further evidence in support of clientelistic spending, I examine the effect of a politician's duration in power, consistent with the notion that incumbent governors who had held local power prior to 2013 as Members of Parliament were more likely to have established clientelistic and patronage networks. I estimate a model whereby I regress the share of targeted spending as a share of the total expenditure on a dummy variable that takes the value of 1 if the incumbent governor held a local political office prior to 2013. As shown in column (1) of Table 5, a long duration in office positively correlates with higher spending on targeted goods. Second, I examine the temporal dimension of clientelistic spending with respect to the timing of the elections. Using quarterly data, I test for opportunistic cycles in spending while treating elections as having external cause or origin. I estimate a model whereby I regress targeted spending as a share of total budget on an indicator that takes the value of 1 if the budget cycle is  $t$  quarters away from the 2017 elections, including quarterly fixed effects to account for seasonality trends.

**Table 5** Clientelistic spending and incumbent duration in political office

Dependent variable: As a share of total expenditure				
	Clientelistic spending	Acquisition of equipment	Salaries and wages	Travel and meeting allowances
	(1)	(2)	(3)	(4)
Incumbent in power before 2013	0.107** (0.005)	0.179*** (0.066)	0.009* (0.005)	0.021** (0.009)
Controls	Yes	Yes	Yes	Yes
Observations	65	65	65	65

Note: This table reports the effect political duration on targeted spending by comparing whether incumbents in power prior to 2013 have differential levels of targeted spending compared to new political entrants. The estimation is restricted to border counties only. Robust standard errors in parentheses

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

As shown in column 1 of Table 6, expenditure on targeted goods experienced the first significant spike (of around 11.8%) three quarters before the elections. The rest of the columns examine the effect of the elections on different components of the budget. The results in column 2 show that one quarter before the elections, the share of spending on the acquisition of equipment had a spike of 23% compared to the previous quarter, while the share of travel and meeting allowances in total expenditure rose by around 9% compared to its level in the previous quarter. Taken together, these results suggest that the timing of this spending was significantly influenced by electoral incentives.

**Table 6** Electoral cycles and changes in clientelistic spending

	Clientelistic spending	Acquisition of equipment	Salaries and wages	Travel and meeting allowance
	(1)	(2)	(3)	(4)
Quarter -3	0.118**	0.036	0.011	0.007
	(0.004)	(0.025)	(0.031)	(0.004)
Quarter -2	0.001	0.004	0.001	0.000
	(0.000)	(0.005)	(0.000)	(0.000)
Quarter -1	0.004	0.232***	0.000	0.091**
	(0.003)	(0.106)	(0.005)	(0.039)
Election month	0.008	0.000	0.001	0.004
	(0.006)	(0.106)	(0.000)	(0.002)
Quarter +1	0.001	0.000	0.001	0.004
	(0.000)	(0.000)	(0.001)	(0.000)
Quarter +2	0.003**	0.017	0.000	0.002
	(0.004)	(0.002)	(0.005)	(0.000)
Quarter +3	0.120	0.001	-0.007	0.000
	(0.000)	(0.001)	(0.000)	(0.000)
Controls	Yes	Yes	Yes	Yes
Observations	65	65	65	65

Note: This table reports the effect of electoral cycle on targeted spending. Minus = before the elections; plus = after the elections. Robust standard errors in parentheses \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

## 5.2 Alternative hypothesis

Using data on targeted spending as a proxy for clientelism presents some inferential challenges. A potential concern is that border counties could have had a systematically lower quality of bureaucracy, and thus an increase in wages, salaries or allowances could reflect an investment in improving the bureaucratic capacity.

However, I argue that most of this spending – especially in Kenya – is more than likely spending on public goods and services to reflect clientelistic spending. First, the annual reports from the Office of the Auditor General document significant evidence of irregularities in spending among border counties after 2015. For instance, the audit reports for the financial year 2015 for Kericho County (a border county) show illegal payments for the acquisition of county assets amounting to 1.6 billion KSh (\$16 million). In another county – Taita Taveta – the audit reveals an unaccounted

75 million KSh (\$750,000) related to scholarship awards, 3.4 million KSh (\$33,000) of illegal payments to road contractors, 17.7 million KSh (\$177,000) for water projects and 163.0 million KSh (\$1.6 million) for county-level projects. D’Arcy and Cornell provide evidence showing that governors in Kenya divert public funds for political gain, and they argue that these spending ‘benefits primarily go to [political] individuals, they are unearned, and disproportionately high and above legitimate levels’ (2016: 264).

## 6 What free riding means for household welfare

In this final chapter, I provide evidence that suggests free riding on neighbouring county expenditure on public goods and services has a detrimental effect on households' welfare. Using experienced-based surveys containing information on households' access to public services, I test whether the reduction in spending on public goods and services led to a decline in living standards for residents in border counties relative to those in the periphery. This poses three key methodological challenges. First, it is difficult to credibly quantify households' living standards. Second, households can themselves choose to use different public goods. Third, many other factors – unrelated to spending – could affect both the provision and accessibility of public services.

To address the first challenge, I construct indicators that quantify the quality and accessibility of locally provided public services using two nationwide censuses. One a potential problem is that the baseline survey was conducted in 2006 while the end-line survey was conducted in 2015, and the long time lapse between might bias the results. The key assumption here is that any factors that might affect access to or the quality of public goods and services affected all counties equally before the 2015 MKAEB reform. As such, I interpret these results as suggestive. To address the second concern – arising from household decisions to use specific public services – I rely on questions regarding access to local public goods at the community rather than the household level. To address the third challenge on causal identification, I estimate a DD model that compares changes in these welfare indicators for the residents of the border counties relative to those in the periphery before and after 2015.

The econometric specification takes the form:

$$y_{j,t} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_j + \beta_3 (Post_t * Treat_j) + \beta_4 X_{j,t} + \varepsilon_{j,t} \quad (3)$$

where  $j$  indexes counties and  $t$  indexes years,  $y_{j,t}$  is a binary indicator that captures households' access to public goods and services (such as community infrastructure, water, agricultural services, paved roads, health),  $Post_t$  is equal to 1 if year  $\geq 2015$  and 0 otherwise and  $Treat_j$  is a dummy variable equal to 1 for the border counties. The variable  $X_{j,t}$  includes population density and a poverty index. As before, the standard errors are clustered at the county level. The coefficient of interest is denoted by  $\beta_3$ . A negative sign suggests that a reduction in spending is associated with worse outcomes.

The outcome variables are restricted to public goods and services provided by the county governments. As a specification check, I also use two placebo outcome indicators – electricity and security, which are provided by central government and should therefore be unaffected by the county-level reform.

As shown in Table 7, two key findings emerge from these estimations. The first is that free riding did reduce welfare; across all county-provided services (columns 1 to 3), border county residents reported lower access to public goods in these counties following the spending reduction (shown by the coefficients of interest, which are negative and statistically significant at standard levels). Second, the results shed light on the distributional effects of free riding and highlight how the magnitude differs across the different public goods and services.

With reference to the placebo tests, the estimated coefficients in columns (5) and (6) are not only small in magnitude, but also statistically insignificant. This provides reassuring evidence that unobserved county characteristics that determine the association between spending and accessibility are not confounding the results.

**Table 7** The effect of the reform on household welfare in border counties compared to periphery counties

Dependent variable: Log (public expenditure/total spending)						
	Community infrastructure	Piped water	Public sanitation	Agricultural extension	Security	Electricity
	(1)	(2)	(3)	(4)	(5)	(6)
Post X treat	-0.432**	-0.117**	-0.234***	-0.208**	0.102	0.075
	(0.199)	(0.059)	(0.058)	(0.097)	(0.121)	(0.043)
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors in parentheses.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

## 7 Conclusion

The findings set out in this paper suggest that counties strategically benchmark their spending patterns against neighbouring counties governed by the same political party (and not their neighbours ruled by different parties). This appears to be consistent with the idea that the spatial distribution of parties creates multiple clusters of political markets, which in turn generates collective action problems – including some counties free riding on the efforts (and resources) of others. Further analysis reveals evidence of resource misallocation, as counties that engage in free riding shift their spending from public to targeted goods. The cost of such redistributive pattern is a reduction in welfare, demonstrated by a decline in both the quality of and access to public goods encountered by residents in these counties.

This paper makes several contributions. The debate about the conditions under which local governance can improve public service delivery is long-standing. I provide a new dimension to this literature by exploring the interaction between fiscal decentralisation and party systems at the subnational level (Ponce-Rodríguez et al., 2018; Hankla et al., 2019). The results suggest that if political parties are unable to contest and win outside their stronghold region, then each cluster of counties under a single political party acts as a separate political market. As such, politicians are incentivised to benchmark their performance against only neighbours that share the same party affiliation, rather than neighbours under different parties. This result sheds light on how elected local governments behave in the presence of a locally dominant party and a polarised electorate.

I extend this line of work in two ways. First, by focusing on electoral incentives, I offer a different insight into why spending might not map into local needs and preferences as theory would suggest. The results suggest that in clientelistic settings, free riding provides a channel through which politicians focused on career goals can provide a minimum level of public services while also engaging in targeted spending. Second, by assessing spillovers, rather than direct allocations, I provide a more general and dynamic framework wherein politicians do not simply act as unitary decision-makers but behave strategically along partisan lines. This finding suggests that there are significant redistributive and political consequences of expenditure spillovers, in sharp contrast with prominent theoretical work that assume the non-existence of spillovers (Tiebout, 1956).

The second contribution is to quantify the welfare implications of resource misallocation. Empirically, disentangling how a reduction in spending affects welfare is challenging because it requires establishing a benchmark that captures the optimal level of welfare (Kyle et al., 2017). To overcome this, I combine household surveys conducted before and after 2015 – the year of the reform – with the reform's phase-in to estimate a DD model that tests for differentials in access to public goods in border counties relative to periphery counties. By doing so, I improve upon the conventional approach of estimating social welfare functions, the results of which are often sensitive to the underlying theoretical assumptions (Ozdemir et al., 2016).

Third, the empirical analysis provides a methodological contribution to measuring spillovers. Contrary to studies that assess spillovers across national boundaries (Devereux et al., 2008; Cassette et al., 2012; 2013), I conduct a subnational analysis to mitigate potential biases arising from unobserved factors such as cross-country differences in institutional frameworks. In addition, combining budgetary and spatial data provides a new approach to examining the determinants of public goods provision at lower levels of governance.

## **Policy implications**

Some key policy implications emerge from these findings. First, in contexts where electoral motives shape spending patterns, it is important that local governments, including politicians and bureaucrats, ensure that spending policies are aligned with the objective of providing public goods and services. This could be through sound public management systems, especially well-designed development plans that establish spending priorities, as a means of promoting budget credibility.

Second, oversight institutions such as supreme audit institutions have the potential to promote budget transparency and accountability through improved audits or publication of budgetary information. This can ensure that citizens have information on policy processes to hold politicians accountable. There is evidence showing that independent audits can reduce the misappropriation of public resources (Avis et al., 2018) and greater accountability can improve the quality of public services (Deininger and Mpuga, 2005).

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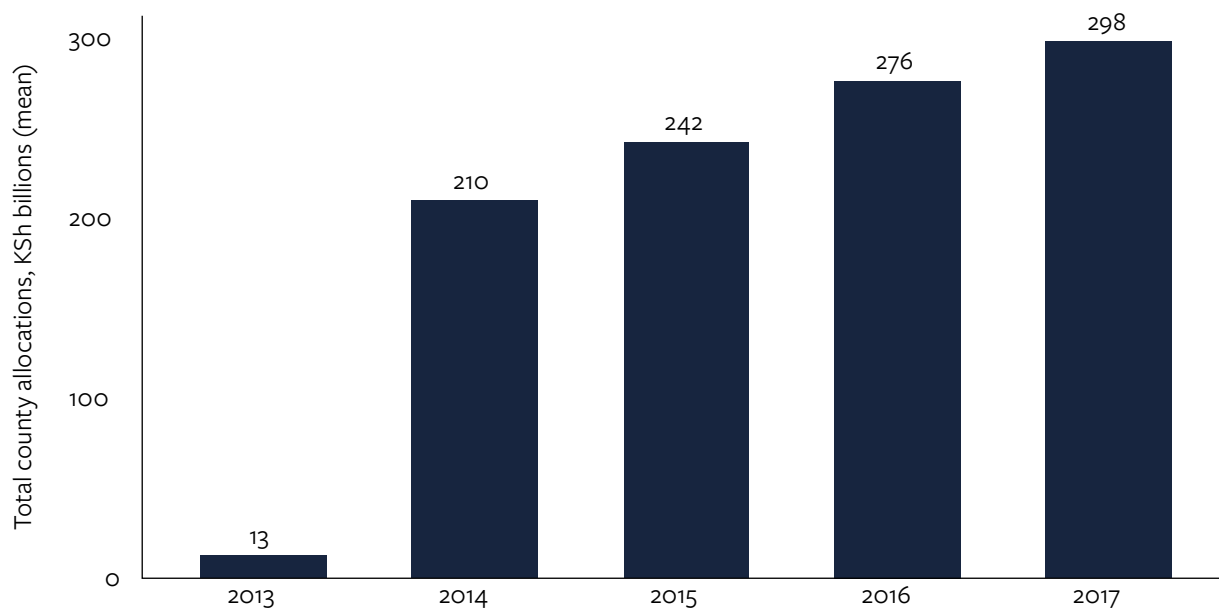
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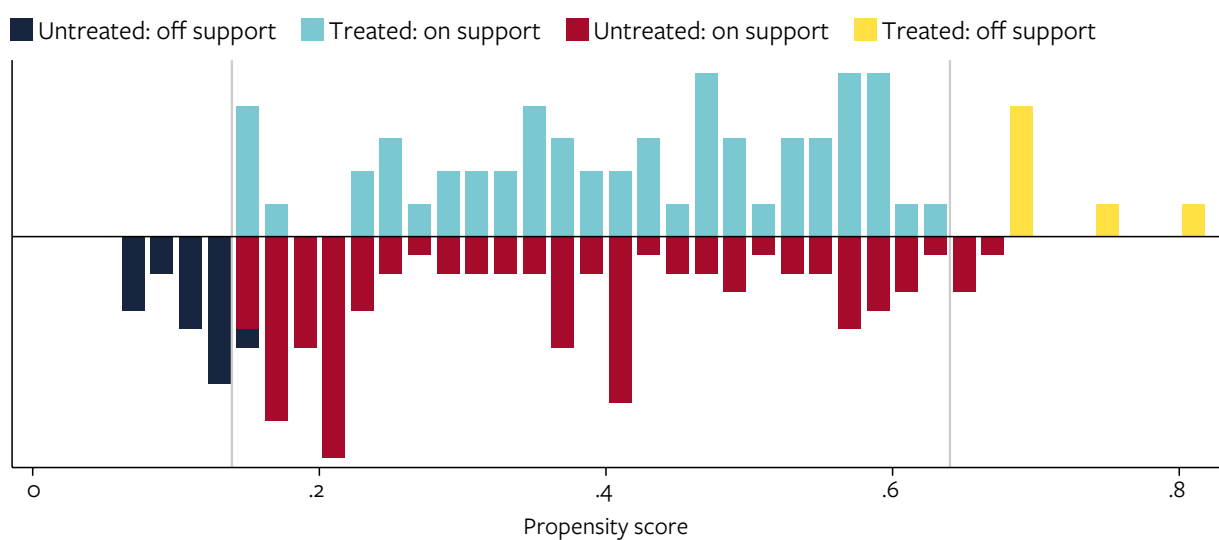
# Appendix

**Figure A1** Allocation of inter-governmental resources to counties in Kenya



Source: Author's own elaboration

**Figure A2** Common support: propensity scores for treated and untreated counties in the sample



Note: This figure shows a significant overlap of the propensity score for treated counties and non-treated counties in the sample.

Source: Author's own elaboration

**Table A1** Covariate balance test for treated and matched counties

Variable	Control (mean)	Treated (mean)	Difference	t-value	p-value
Gini coefficient (2009)	40.483	37.877	-2.606	1.36	0.1768
Population density (2009)	485.261	423	-62.261	0.25	0.8007
MPI (2009)	0.259	0.274	0.015	0.69	0.4912
Log per capita GDP	11.252	11.385	0.133	1.32	0.1919
Revenue per capita	2.786	4.664	1.878	0.83	0.407
Log population density	9.362	8.906	-0.456	1.87	0.0653

\*\*\* p<0.01; \*\* p<0.05; \* p<0.1

**Table A2** Summary of the distribution of bias in the sample**Panel A**

		Mean		% reduction			
Variable		Treated	Control	% bias	bias	t-test	p-value
Gini coefficient (2009)	Unmatched	37.877	40.483	-33.4		-2.18	0.031
	Matched	38.368	39.867	-19.2	42.5	-1	0.0321
Population density (2009)	Unmatched	423	485.26	-6		-0.4	0.687
	Matched	459.81	389.31	6.8	-13.2	0.33	0.74
MPI (2009)	Unmatched	0.2738	0.2591	15.5		1.1	0.271
	Matched	0.2534	0.2708	-18.3	-18.5	-0.97	0.335
Log per capita GDP	Unmatched	11.548	11.436	25.8		1.64	0.102
	Matched	11.578	11.656	-17.9	30.5	-0.88	0.38
Log population density	Unmatched	8.9641	9.406	-42.9		-2.9	0.004
	Matched	9.028	9.195	-16.2	62.3	-0.74	0.46

**Panel B**

Sample	Mean bias	Median bias	p>chi2	LR chi2	B	R	% variance
Unmatched	24.7	25.8	0	23.84	79.7*	0.69	60
Matched	15.7	17.9	0.235	6.81	51.7*	1.17	40

\* if B>25%, R outside [0.5; 2]