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Spatio-Temporal Analysis of the Determinants of Development Expenditure in County Governments in Kenya

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Abstract

Kenya's 2010 Constitution handed major fiscal responsibilities to 47 new county governments, the aim being to move public services and development spending closer to citizens. A decade on, how much counties actually spend on development still varies widely from one to the next, and the reasons behind that variation remain poorly understood. This study asks what drives actual development expenditure across the 47 counties, using a balanced panel of 517 county-year observations that covers fiscal years 2014/15 through 2024/25. All monetary variables are converted to constant fiscal-year-2014/15 Kenya shillings using the Kenya Consumer Price Index, and two common-time controls, a fiscal-year inflation rate and a linear time trend, are added alongside seven substantive explanatory variables: the wage bill (WB), own-source revenue (OSR), national government fiscal transfers (NGF), development partner funds (DPF), pending bills (PB), a political coalition indicator (PC), and a binary election-year indicator (EL) marking the fiscal years tied to Kenya's 2017 and 2022 general elections. Seven candidate spatial panel specifications were fitted and screened against a five-point assumption battery; the common-correlated-effects-augmented spatial autoregressive model with individual county fixed effects, SAR-FE (CCE), was retained as the final model on a tie-break by information criterion among three equally-scoring candidates. The spatial autoregressive parameter is positive and significant ($\lambda = .204$, $p < .001$), consistent with, though not uniquely identifying, spillovers in development spending across neighboring counties, since persistent cross-sectional dependence in the residuals means part of this parameter may also reflect shared national or regional shocks rather than spillovers alone. National transfers remain the strongest driver (total elasticity = 1.103), the wage bill (−.605) and pending bills (−.092) continue to be associated with lower development spending, and development partner funds are associated with higher development spending

(.314); own-source revenue and political coalition are not significant. The election year indicator had a positive significant correlation to fiscal year inflation ($r = 0.684$, $p < 0.001$). However, the final commen-time control CCE cross-sectional-mean augmented model showed that it had no significant effect on county government development expenditure (elasticity = −0.047, $p = 0.482$). The paper recommends a binding wage-bill ceiling, faster and steadier equitable-share disbursement, and a structured program to clear pending bills, while treating any election-year budget-execution safeguard as a precaution against a fragile, not yet firmly established, pattern.

Keywords

Development Expenditure, Spatial Panel Models, Fiscal Federalism, Devolution, Kenya Counties, Spatial Econometrics, Spillover Effects, Election-Year Effects, Electoral Cycle

1. Introduction

1.1. Background

Kenya's move to devolved government in 2013 was among the largest administrative overhauls the country has attempted since independence. The Constitution of Kenya (2010) created 47 county governments and gave each of them legislative and executive authority over a defined list of functions, among them agriculture, health, early childhood education, and local infrastructure. It also guaranteed that money would follow those functions: at least 15% of the most recently audited national revenue was to be shared among the counties as an equitable share, topped up by conditional and unconditional grants (Commission on Revenue Allocation (CRA), 2020).

The classic argument for this kind of arrangement comes from Oates (1972), whose decentralization theorem holds that public goods are provided more efficiently by the level of government closest to the people who use them, because that level knows local preferences better. Later writers built on the idea to bring in the role of own-source revenue in fiscal autonomy, the design of transfer systems, and the politics of decentralized spending (Bahl & Linn, 1992; Bird & Vaillancourt, 2008; Weingast, 2009). In Africa the case for devolution has rested on equity as much as efficiency, since one of its stated purposes is to narrow the historical gaps in public services between regions (Mwangi et al., 2023; Smoke, 2015).

By development expenditure we mean the capital and project spending counties undertake, as opposed to recurrent spending on wages and day-to-day operations. It is the spending that devolution was supposed to turn into better roads, clinics, water, and livelihoods, so it is the natural place to look if we want to know whether devolution is working. The record of the first decade is uneven. Development spending is spread very unequally across counties, and in many of them the share of the budget that actually goes to development has stayed below the 30% floor set in the Public Finance Management Act, 2012 (Controller of Budget (COB), 2023).

Plenty of empirical work has looked at what drives public spending in decentralized systems, but most of it treats each jurisdiction as an island and works at the national or cross-country level (Baskaran et al., 2016; Gadenne & Singhal, 2014). That assumption is hard to defend in Kenya, where neighboring counties share labor markets, road networks, supply chains, and political history. A fiscal decision in one county tends to spill over into the next, through competition for investment, joint infrastructure planning, or a drought or commodity-price swing that hits several counties at once. Samburu and Turkana are bound together economically and culturally, and the same holds for Kericho and Bomet, Elgeyo Marakwet and Baringo, Uasin Gishu and Nandi, the arid counties of the north, and the sugar-belt counties. Estimating a model that pretends these links do not exist gives biased and inefficient results (Anselin, 1988; Elhorst, 2014).

This paper takes that problem head on. It builds a balanced panel of all 47 counties observed every year from fiscal 2014/15 to 2024/25 and estimates the fiscal, institutional, political, and electoral-cycle determinants of development expenditure with spatial panel models that carry county fixed effects and a spatial lag. Because national-level shocks would otherwise be absorbed by year fixed effects and make an election-year indicator impossible to identify, the models are specified with individual (county) fixed effects rather than the two-way fixed effects common in earlier spatial panel work, leaving year-to-year national variation, including the electoral cycle, available for estimation. The payoff is estimates that are not distorted by the spatial dependence a conventional panel would sweep under the rug, and that speak directly to whether development spending itself moves with the electoral calendar.

1.2. Statement of the Problem

The Constitution guarantees equitable transfers and the law requires at least 30% of each county budget to go to development, yet spending on the ground looks nothing like a level playing field. It is spatially unequal, development funds are chronically under-absorbed, and the county-to-county gaps are wider than standard fiscal indicators can account for (Controller of Budget, 2023; Office of the Auditor General, 2022). Year after year the Controller of Budget reports that many counties spend well under the 30% threshold on development, even though total county revenue has climbed steadily since 2013 (Commission on Revenue Allocation, 2023).

Several things sit behind this pattern. Counties often run short of money for capital projects; citizens are rarely involved enough in budgeting to hold anyone to account; political horse-trading crowds out genuine development priorities; and each new administration tends to shelve the projects its predecessor started, so allocations lurch about from one cycle to the next. Many counties also lack the technical staff to plan and absorb a capital budget, which wastes money and lets unpaid bills pile up. What the evidence base still lacks is a careful account of why development spending differs across counties and over time, one that puts development expenditure itself on the left-hand side, takes the spatial links between counties seriously, and brings the fiscal, institutional, and political drivers into a single model. That is the account this paper sets out to provide.

1.3. Objectives of the Study

The broad objective of this study is to examine the spatio-temporal determinants of development expenditure in Kenya's 47 county governments over the period 2014/2015 to 2024/2025.

Specifically, the study seeks to:

1) identify the fiscal, institutional, political, and electoral-cycle determinants of county development expenditure.

2) estimate the degree of spatial interdependence in development spending across county borders.

3) decompose the total effect of each determinant into direct own-county and indirect cross-county spillover components.

2. Literature Review

2.1. Theoretical Framework

2.1.1. Fiscal Federalism Theory

The theoretical foundation for this study rests principally on Oates's (1972) Decentralization Theorem, which holds that decentralized provision of public goods is welfare-superior to centralized provision when local preferences are heterogeneous and when spillover effects across jurisdictions are absent or can be internalized through intergovernmental grants. In the absence of spillovers, each level of government should finance the goods it provides through its own revenues, a principle known as fiscal equivalence (Olson, 1969). The theorem predicts that own-source revenue, which reflects the fiscal effort and preference revelation of local residents, should be positively related to local public spending, including development expenditure.

The Musgravian framework of public finance (Musgrave, 1959) further distinguishes among the allocation, distribution, and stabilization functions of government. Under decentralization, county governments are primarily responsible for the allocative function of providing local public goods efficiently, while the distribution function is partly retained by the national government through equalization transfers designed to reduce horizontal fiscal imbalances. This distinction is critical for interpreting the role of national government fiscal transfers in this study: they are not merely revenue top-ups but instruments of national distributive policy operating within a devolved fiscal architecture.

More recent contributions have emphasized the political-economy dimensions of decentralized spending. Weingast (2009) argued that fiscal decentralization constrains predatory central governments and creates market-preserving incentives for subnational governments, while Rodden et al. (2003) showed that soft budget constraints, the expectation that higher-level governments will bail out fiscally irresponsible subnational units, can undermine the efficiency gains devolution is supposed to produce. The presence of large unconditional transfers to Kenyan counties, combined with weak own-source revenue mobilization in many of them, raises legitimate soft-budget concerns that are directly testable in the empirical framework.

2.1.2. Spatial Externality Theory

The standard fiscal federalism framework assumes that subnational jurisdictions are independent. This assumption breaks down when the provision of public goods in one jurisdiction generates positive or negative externalities for neighboring jurisdictions, a phenomenon studied extensively under the headings of fiscal competition, yardstick competition, and expenditure spillovers (Besley & Case, 1995; Brueckner, 2003; Tiebout, 1956).

Expenditure spillovers arise when the benefits of a public good provided by one jurisdiction spill over its borders. Road construction is an obvious example: a road built by one county that crosses into a neighboring county benefits resident of both. Infrastructure investment, agricultural extension services, and public health programs all exhibit varying degrees of spatial non-excludability. When spillovers are positive, local governments tend to underprovide the spillover-generating good relative to the socially optimal level, because they cannot capture the full return on their investment (Oates, 1972). Corrective grants from a higher level of government, precisely what national and development partner funds represent in the Kenyan context, are a standard theoretical remedy.

Spatial econometrics supplies the empirical tools to detect and measure these spillovers. Anselin (1988) formalized the distinction between a spatial lag process, in which the outcome in one unit depends directly on the outcomes of its neighbors, and a spatial error process, in which the disturbances across units are spatially correlated owing to omitted spatially clustered variables. The spatial Durbin model (LeSage & Pace, 2009) encompasses both by including spatially lagged regressors alongside the spatially lagged dependent variable, allowing total effects to be decomposed into direct own-county effects and indirect spillover effects.

2.1.3. The Flypaper Effect and Fiscal Illusion

A well-documented anomaly in the fiscal federalism literature is the flypaper effect: intergovernmental grants stimulate local public spending by more than an equivalent increase in local private income, as if money sticks where it lands (Hines & Thaler, 1995). The theoretical explanation remains contested, having been attributed to fiscal illusion, bureaucratic self-interest, and budget-process rigidities, but the empirical regularity is robust across a wide range of countries and institutional settings (Inman, 2008). In the Kenyan context, where equitable-share transfers are unconditional and constitute the majority of county revenue, the flypaper effect predicts that increases in national transfers should translate into more than proportionate increases in development expenditure, a prediction the empirical framework tests directly.

2.2. Empirical Literature

2.2.1. Determinants of Public Expenditure in Decentralized Systems

The empirical literature on the determinants of subnational public expenditure is vast; this review focuses on studies most directly relevant to the Kenyan context and to the spatial dimension of the research question. Baskaran et al. (2016), synthesizing several decades of empirical work in a meta-analysis, found the relationship between fiscal decentralization and economic growth to be inconsistent across studies, with roughly as many finding a positive association as a negative or null one, a pattern they attribute to differences in how decentralization is measured and to omitted institutional variables.

Gadenne and Singhal (2014) conducted a cross-country analysis of fiscal decentralization and found that local governments in low- and middle-income countries that rely heavily on intergovernmental transfers tend to display lower tax effort and lower development-spending efficiency than those with a more balanced revenue mix. Their findings suggest that the composition of county revenue, not merely its level, matters for development outcomes, which motivates the inclusion of both national transfers and own-source revenue as separate regressors in this study. In the African context, Smoke (2015) reviewed the experience of fiscal decentralization across sub-Saharan Africa and identified three recurring pathologies: insufficient own-source revenue, weak expenditure-management capacity, and political capture of local budget processes, all three of which are empirically relevant to Kenya (Office of the Auditor General, 2022).

For Kenya specifically, Mwangi et al. (2023) conducted interviews and document review in two counties, Kiambu and Nairobi City, and found that unpredictable disbursement schedules and weak absorption capacity, rather than the size of the equitable share itself, were the main constraints on service delivery. Their study is qualitative and does not test for spatial dependence or draw on panel data across the 47 counties, a gap the present study fills.

Cross-country evidence reinforces the point that different revenue sources behave differently. Hossain, Toufique, Smrity, and Kibria (2024) assembled a panel of 20 countries drawn evenly from the World Bank's four income groups over 1991 to 2018 and, using pooled mean group and dynamic fixed-effects estimators, found that national income, government revenue, and the volume of trade were all significant long-run drivers of government spending, though the strength of Wagner's law varied by income group. Trofimov (2023), working with a global panel and disaggregated expenditure data, likewise reported that the income–spending relationship is sensitive to how government size is measured and to the functional category of spending, cautioning against treating aggregate expenditure as a single homogeneous outcome. Khan, Ilyas, and Chattha (2023) reached a similar conclusion for Pakistan, where economic growth, revenue, and trade openness jointly explained the expansion of public expenditure. Between them, these studies make the case for entering national transfers, own-source revenue, and the wage bill as separate regressors rather than folding them into one fiscal aggregate.

The transfer-spending link at the heart of this study has been examined directly. Lago, Lago-Peñas, and Martinez-Vazquez (2024), in a comprehensive survey of the effects of intergovernmental grants, documented that the flypaper effect remains a robust empirical regularity across countries: subnational governments tend to expand expenditure rather than cut taxes when grants rise, and the effect differs systematically between conditional and unconditional transfers. Porto, Puig, and Vidal (2026) provided dynamic evidence from Argentina's multi-tier federation, showing that a positive shock to intergovernmental transfers produces a more-than-proportional short-run increase in subnational spending before budgets gradually return to balance. Both findings anticipate the strong, near-complete pass-through from the equitable share to development expenditure reported later in this study, and none of these studies allows for spatial dependence among the receiving governments.

2.2.2. County Finance and Development Outcomes in Kenya

The literature on county public finance in Kenya has grown quickly since devolution took effect in 2013. Frempong (2026) covered all 47 counties from 2013 to 2022 and estimated a dynamic fixed-effects panel model examining how revenue autonomy and the development-expenditure ratio jointly shape gross county product. The headline finding is that the development-expenditure ratio is positively and significantly associated with county economic performance in both the short and long run, while revenue autonomy carries a negative sign attributed to Kenya's redistributive equitable-share system. Critically, however, that study does not model spatial dependence and does not use development expenditure as the dependent variable, so the question of why some counties spend more on development than others remains unanswered.

Atetwe and Obora (2026) examined how fiscal decentralization relates to multidimensional poverty across all 47 counties from 2006 to 2019 and reported a nonlinear relationship: expenditure decentralization is initially negatively related to human development, but the squared term turns positive, implying that benefits materialize only after a county crosses a threshold. Despite its broad coverage, the study treats expenditure decentralization as a regressor rather than an outcome and does not apply spatial modeling. Akhonya et al. (2026) examined equitable-share distribution and the financial sustainability of county governments in Western Kenya, reporting a positive and significant association ($r = .578$, $p < .001$), and their qualitative evidence points to disbursement predictability, not transfer size, as the binding constraint; finance officials told them that National Treasury delays in the final quarter threw off cash-flow planning and pushed some development projects into later budget cycles. Daniel et al. (2026) shifted the lens to internal conditions, finding that risk mitigation explains 46.7% of the variance in development-expenditure utilization in public health facilities in Kakamega County, and thereby making the case that institutional capacity, not funding alone, determines whether money becomes infrastructure.

Nyaboga, Kengere, and Akims (2026) examined county government financial expenditures and local economic development in Kisii County, disaggregating spending into infrastructure, education and training, and agribusiness components, and found that capital-oriented expenditure was positively associated with local economic development, even though the translation of budgets into outcomes was constrained by weak public financial management. Like the studies before it, that analysis treats a single county and stops short of a national, spatially explicit account. Set alongside Frempong (2026), Atetwe and Obora (2026), Akhonya et al. (2026), and Daniel et al. (2026), none of this work models actual development expenditure across all 47 counties while allowing for cross-border spillovers.

The common thread across these studies is that money alone does not buy development performance, that fiscal relationships are often nonlinear, and that counties differ enormously. What none of them offers is a comprehensive spatial and longitudinal account of the fiscal drivers of actual development expenditure across all 47 counties, and that is the gap this paper fills.

2.2.3. Spatial Dimensions of Fiscal Policy

The application of spatial econometric methods to fiscal policy has grown rapidly since Anselin's (1988) seminal methodological contribution. Case et al. (1993) provided one of the earliest empirical demonstrations of spatial interdependence in U.S. state government expenditures, finding that a state's spending on welfare, education, and highways was positively correlated with spending in neighboring states, consistent with both yardstick competition and genuine spillover effects. Brueckner (2003) reviewed the theoretical literature on strategic interaction among local governments and concluded that spatial econometric methods are necessary for testing whether observed expenditure patterns reflect genuine strategic behavior rather than coincidental correlation; his framework has since been applied to European regional spending (Revelli, 2005), Brazilian municipal expenditures (Fiva & Rattsø, 2007), and Chinese county-level fiscal behavior (Akin et al., 2005).

In the African context, Caldeira et al. (2015) examined spatial interdependence in local government expenditure in Benin using a spatial lag model and found strong evidence of strategic interaction, with municipalities mimicking their neighbors' spending decisions, particularly in the social sectors.

Most of this work examines the consequences of public spending rather than its determinants, but it establishes the spatial machinery this study relies on. Closest in spirit to the present work, Adeosun (2026) applied a spatial Durbin model with a Queen-contiguity weights matrix to public spending and inclusive growth across 39 contiguous sub-Saharan African countries and confirmed significant positive spatial effects, so that total, education, and investment spending in one country influenced outcomes in its neighbors. Because that study also decomposes effects into direct, indirect, and total components, it validates, in an African setting, precisely the empirical strategy adopted here. Novitasari and Iskandar (2022) used a spatial Durbin model on 13 districts in Indonesia's South Kalimantan Province from 2010 to 2020 and found both significant spatial autocorrelation in outcomes and genuine spillovers from sectoral government expenditure, with education spending in one district affecting neighboring districts. Together, these developing-country studies establish that ignoring spatial dependence in subnational fiscal analysis produces biased inferences.

The same workflow recurs across a range of settings. Ma, Wu, Li, and Huang (2023) analyzed local higher-education expenditure across 30 Chinese provinces from 2000 to 2021 using static and dynamic spatial Durbin models and found positive spatial autocorrelation together with sizable spillovers whose sign varied by region. Li, Long, Ouyang, and Ma (2022) applied spatial econometric models to province-level panel data for China from 2013 to 2018 and found that digital financial inclusion raises local household consumption but produces a negative spillover on neighboring provinces' consumption, evidence that spatial interdependence in household spending can run through channels other than fiscal expenditure, while Sun and Wang (2021) used a spatial Durbin model to demonstrate that health expenditure itself exhibits significant cross-regional spillovers. Each of these studies constructs a contiguity-based weights matrix, tests for spatial autocorrelation with Moran's I, and decomposes effects into direct and indirect components, which is the exact sequence followed here. None of them, however, applies the approach to aggregate county development expenditure, and none combines it with common-correlated-effects augmentation to handle the strong cross-sectional dependence that characterizes a devolved fiscal system.

2.2.4. Political Economy of County Expenditure in Kenya

The political economy of fiscal decentralization in Kenya is shaped by the country's ethnically fragmented political landscape and the partisan dynamics of elections at both national and county levels. Cheeseman et al. (2016) documented that, in the first devolution cycle (2013 to 2017), county governors used their new authority to build independent political bases, at times resisting rather than deferring to the national ruling coalition, a dynamic that complicates any simple story of favoritism toward politically aligned counties. This finding motivates the inclusion of a political coalition indicator in the empirical framework, though it also cautions against assuming its sign in advance.

Prattay (2024) examined ethnic coalition formation in Kenya's 2022 general election and argued that ethnic group leaders back coalitions in anticipation of future resource distribution and government appointments, a pattern rooted in neopatrimonial habits carried over from the colonial and postcolonial periods; that analysis is qualitative and specific to a single election, whereas the present study uses a panel framework that controls for time-invariant county characteristics and time-specific national shocks. These findings reinforce the importance of the political coalition variable and suggest that the relationship between political alignment and development spending may be time-varying, a feature the panel framework is well positioned to capture.

The broader political-economy evidence complicates that early Kenyan picture.

Mbate (2021), in a study of Kenya's counties, found that electoral incentives shaped both the level and the composition of public spending: counties that free-rode on national transfers reallocated their budgets toward categories associated with patronage and clientelism, and these shifts intensified before elections and among long-serving incumbents. This within-Kenya evidence suggests that political effects may operate on the composition rather than the aggregate level of development expenditure, a distinction that helps interpret the insignificant political-coalition coefficient reported later in this study.

Cross-country evidence points in similar, if not fully consistent, directions. In a fixed-effects panel of 17 South Korean regions over 2013 to 2023, Lee (2026) found little support for alignment-based favoritism in intergovernmental transfers and identified electoral timing, rather than partisan congruence, as the dominant political driver. Lokshin, Rodríguez-Ferrari, and Torre (2024) documented pronounced electoral cycles in public spending during the pandemic across a large sample of countries, and Bury and Feld (2025) showed, for German municipalities, that the local spending of intergovernmental transfers responds to the timing of elections. Offering a countervailing African case, Brunnschweiler and Obeng (2025) found that political alignment was rewarded through more favorable fiscal outcomes in local government. The mixed weight of this evidence, with favoritism in some settings, electoral timing in others, and near-neutrality elsewhere, frames the null political-coalition finding reported here not as an anomaly but as one plausible outcome in a literature that has yet to reach consensus, and none of these studies embeds the political variable within a spatial panel of development expenditure.

2.2.5. Gaps in the Existing Literature

The review of both the theoretical and empirical literature reveals four gaps that this study addresses. First, despite a fast-growing spatial-fiscal literature (Adeosun, 2026; Li, Long, Ouyang, & Ma, 2022; Ma et al., 2023; Novitasari & Iskandar, 2022), no existing study has applied spatial panel econometric methods to aggregate development expenditure across all 47 Kenyan counties, leaving the question of spatial interdependence in county capital spending empirically unresolved. Second, the determinants literature continues to examine fiscal variables in isolation or in small combinations (Hossain et al., 2024; Khan et al., 2023; Lago et al., 2024; Trofimov, 2023), whereas this study estimates the joint effect of the wage bill, own-source revenue, national transfers, development partner funds, pending bills, political coalition, and election-year timing within a single multivariate spatial panel. Third, although the spillover decomposition has been performed in other settings, it has not been applied to Kenyan county development expenditure, making it impossible to trace how a fiscal or electoral shock in one county propagates to its neighbors. Fourth, the political-economy studies disagree on whether political alignment and electoral timing shape subnational spending (Bury & Feld, 2025; Lee, 2026; Lokshin et al., 2024; Mbate, 2021), yet none situates both a political-alignment indicator and an explicit election-year indicator inside a spatial panel model of development expenditure over a period that spans the post-COVID fiscal recovery and two full Kenyan electoral cycles, the 2017 and 2022 general elections. The present study fills these gaps simultaneously, treating the election-year indicator as a core regressor identified from the first panel specification onward rather than as an afterthought.

3. Econometric Methodology

3.1. Study Design and Data

This study uses a balanced panel of Kenya's 47 county governments observed over 11 fiscal years, from 2014/15 through 2024/25, giving a total of 517 county-year observations. Throughout the paper, a fiscal year is labeled by the calendar year in which it ends, so fiscal year 2014/15 (which closed on 30 June 2015) is written as 2015 and fiscal year 2024/25 as 2025. All financial data are drawn from the annual County Governments Budget Implementation Review Reports published by the Office of the Controller of Budget, supplemented where necessary by the audited financial statements of the Auditor-General. County boundaries for the spatial component were taken from the official Independent Electoral and Boundaries Commission shapefile. Only actual, rather than budgeted, figures were used, and every monetary variable is expressed in billions of Kenya shillings; for the regression analysis, these nominal figures are further converted to constant fiscal-year-2014/15 Kenya shillings, as described in Section 3.3.

3.2. Description and Measurement of Variables

The dependent variable is county development expenditure (DE), the actual amount spent on capital programs in a fiscal year. Nine explanatory variables capture the fiscal, political, electoral, and common-time environment, as summarized in Table 1: five fiscal variables, a political-coalition indicator, an election-year indicator, and, to give the model a transparent common-time control rather than relying on the election-year indicator alone, a fiscal-year inflation rate and a linear time trend. Because proper variable selection and measurement are prerequisites for a meaningful result, Table 1 reports how each variable is defined and the source from which its data were obtained.

Table 1. Description and measurement of variables.

Variable	Description and measurement	Source
DE	Actual development (capital) expenditure	COB
WB	Wage bill (total employee compensation)	COB
OSR	Own-source revenue raised internally by the county	COB
NGF	National government equitable-share transfer	COB
DPF	Development partner funds (conditional grants, donor funds)	COB
PB	Stock of pending bills outstanding at fiscal-year close	COB/AG
PC	Political coalition (=1 if governor aligned with ruling coalition)	IEBC

Continued

EL	Election-year indicator (=1 for the fiscal year ending in an August general-election year and the fiscal year immediately following it)	IEBC
INFL	Fiscal-year inflation rate (%), from the Kenya CPI deflator used to convert monetary variables to constant prices	KNBS/World Bank
TREND	Linear time index (1, ···, 11), one value per fiscal year	Constructed

COB = Controller of Budget; AG = Auditor-General; IEBC = Independent Electoral and Boundaries Commission. All monetary variables are measured in billions of Kenya shillings; PC is a binary indicator.

3.3. Variable Transformation

All monetary variables in this study were originally recorded in nominal Kenya shillings, so part of any measured relationship, including the election-year coefficient discussed in Section 4.11.6, could reflect nationwide price growth rather than a genuine fiscal or electoral relationship. To guard against this, every monetary variable, DE, WB, OSR, NGF, DPF, and PB, is first converted to constant fiscal-year-2014/15 Kenya shillings using a Kenya Consumer Price Index series (Kenya National Bureau of Statistics; cross-checked against World Bank national accounts data), with each fiscal year's CPI taken as the average of the CPI in the two calendar years the fiscal year spans and rebased so the panel's first fiscal year equals 100. The monetary variables used everywhere in the paper, including Table 2 and every regression reported in Section 4, are these constant-price series, denoted with a subscript r where the distinction matters.

$$y_{it} = \ln\left(1 + \mathrm{DE}_{it}\right) \tag{1}$$

The constant-price monetary variables remain strongly right-skewed, and pending bills in particular is dominated by a single extreme observation (Nairobi City). To compress these long right tails while preserving the zero values present in some series, each continuous variable was transformed using the natural logarithm of one plus the value, $\ln(1 + x)$. The same transformation was applied to WBr, OSRr, NGFr, DPFr, and PBr. Before transformation, pending bills was Winsorized at its 99th percentile (a cap of KES 46.53 billion in constant terms, affecting six observations) so that the Nairobi outlier would not dominate estimation. The binary indicators PC and EL, and the two common-time controls (INFL, TREND; Section 3.4), were left untransformed. Because both sides of the model enter in $\ln(1 + x)$ form rather than plain logarithms, the slope coefficients on the five continuous regressors are close to, but not exactly, elasticities in the underlying monetary variables: the $\ln(1 + x)$ transformation is not scale-invariant the way $\ln(x)$ is, so a given coefficient corresponds to a slightly different percentage change in DEr depending on the level of DEr at which it is evaluated, and the approximation is least accurate for observations near zero. For the two binary indicators, PC and EL, the coefficient is better read not as an approximate percentage change but as a transformed predicted difference: the model implies $(1 + \mathrm{DEr}|\text{indicator} = 1) = (1 + \mathrm{DEr}|\text{indicator} = 0) \times \exp(\text{coefficient})$, so the predicted difference in constant-KES development expenditure from switching the indicator from 0 to 1, at any chosen baseline level of DEr, is (1 + baseline) × (exp(coefficient) − 1). Section 4.8 and Section 4.11.6 report this transformed predicted difference for EL and PC directly, evaluated at the panel's mean constant-KES development expenditure, rather than treating the coefficient itself as a percentage effect.

3.4. Model Specification

The empirical strategy builds up in three steps. A pooled specification ignores the panel structure entirely; an individual (county) fixed-effects model then absorbs unobserved heterogeneity across counties, as in Equation (2):

$$y_{it} = x_{it}'\beta + \mu_i + \varepsilon_{it} \tag{2}$$

where y_{it} is log development expenditure in county i and year t, x_{it} is the vector of nine regressors (five log-transformed fiscal variables, the political-coalition and election-year binary indicators, and a fiscal-year inflation rate, INFL_t, and a linear time trend, TREND_t), μ_i is a county fixed effect, and ε_{it} is the disturbance. Individual, rather than two-way, fixed effects are used throughout because a year fixed effect would absorb exactly the year-to-year national variation that identifies EL, since every county shares the same election-year status, inflation rate, and trend value in a given fiscal year; retaining that variation is what lets the electoral-cycle association be estimated at all. INFL_t and TREND_t are included directly, rather than as year fixed effects, for the same reason: they give the model an explicit, transparent common-time control, absorbing the smooth, economy-wide part of any nationwide shock, without fully re-absorbing EL's identifying variation the way a complete set of year dummies would. This is a partial remedy, not a complete one. EL is identical across all 47 counties in each of the four fiscal years it flags, and, once INFL and TREND are added, EL correlates with the fiscal-year inflation rate at r = .684 (Section 4.2), so what remains of EL's coefficient after these controls reflects whatever nationwide shock in an election-year fiscal year is not already smooth inflation or trend, a residual that could still include other common shocks unrelated to elections; the coefficient is accordingly reported and discussed throughout Section 4 as an association rather than a causal electoral-cycle effect. Because the fixed-effects model treats each county as fiscally independent, an assumption the data reject, the preferred specification adds a spatial lag of the dependent variable, yielding the spatial autoregressive fixed-effects (SAR-FE) model in Equation (3):

$$y_{it} = \lambda \sum_{j=1}^{N} w_{ij} y_{jt} + x_{it}'\beta + \mu_i + \varepsilon_{it} \tag{3}$$

where w_{ij} is the spatial weight linking counties i and j, and λ is the spatial autoregressive parameter measuring how strongly a county's development expenditure moves with that of its neighbors. In stacked matrix form the model is given by Equation (4):

$$y = \lambda\left(I_T \otimes W\right)y + X\beta + \mu + \varepsilon \tag{4}$$

where W is the $N \times N$ spatial weights matrix, I_T is the identity matrix of order T, and $\otimes$ denotes the Kronecker product.

Because WB, NGF, DPF, and PB are entered contemporaneously, in the same fiscal year as DE, Equations (2)-(4) rest on an implicit timing assumption: that each of these four regressors is determined, or at least budgeted and substantially committed, before or independently of that year's development-expenditure decisions, rather than being simultaneously chosen alongside them. This assumption is more credible for some regressors than others. National transfers (NGF) follow a constitutional revenue-sharing formula set in advance of the fiscal year by the Commission on Revenue Allocation, and development partner funds (DPF) are governed by donor commitments and disbursement schedules agreed before the fiscal year begins, so both are plausibly close to pre-determined with respect to that year's development spending. The wage bill (WB) and pending bills (PB), by contrast, are jointly determined with the same year's overall budget execution: a county under fiscal stress could in principle cut development spending to protect payroll, or let bills go unpaid, in the same year those pressures arise, rather than the wage bill or pending bills mechanically driving development spending on their own. Because this study does not have an instrument or another source of genuinely exogenous variation in the wage bill or pending bills, the coefficients on all five continuous regressors, and on PC and EL, are reported and interpreted throughout Section 4 as conditional associations, the relationship between each regressor and development expenditure holding the other regressors fixed, rather than as estimated causal fiscal effects, and the discussion in Section 4.11 is written on those terms.

3.5. Spatial Weights Matrix

Spatial relationships are encoded through a Queen contiguity matrix, in which two counties are treated as neighbors when they share either a common border or a single boundary point, as defined in Equation (5):

$$w_{ij} = \begin{cases} 1 & \text{if counties } i \text{ and } j \text{ are contiguous} \\ 0 & \text{otherwise} \end{cases}, \text{ with } w_{ii} = 0 \tag{5}$$

The matrix was then row-standardized so that each row sums to one, as in Equation (6):

$$w_{ij}^{*} = \frac{w_{ij}}{\sum_{j=1}^{N} w_{ij}} \tag{6}$$

Row standardization allows the spatial lag to be read as a weighted average of neighboring counties' values and keeps λ comparable across counties with different numbers of neighbors. Under this scheme the 47 counties have 5.19 neighbors on average. Because the choice of weights can influence spatial estimates, the final model was re-estimated under two alternatives, *k*-nearest neighbors (k = 5) and Rook contiguity, as a robustness check.

3.6. Estimation and Model Selection

All spatial panel models were estimated by maximum likelihood using the splm package in R (Millo & Piras, 2012). Three classical tests guided the panel specification: an F test comparing pooled OLS against fixed effects, a Breusch-Pagan Lagrange multiplier test comparing pooling against random effects, and a Hausman test comparing fixed against random effects. Because these tests ignore spatial structure, the spatial Hausman test of Mutl and Pfaffermayr (2011) was used as the decisive criterion once spatial dependence was established. Whether a spatial model was needed, and of what type, was assessed with Moran's I on the fixed-effects residuals, computed for each year as in Equation (7):

$$I = \frac{N}{S_0} \cdot \frac{\sum_{i=1}^{N} \sum_{j=1}^{N} w_{ij} e_i e_j}{\sum_{i=1}^{N} e_i^2}, \quad S_0 = \sum_{i=1}^{N} \sum_{j=1}^{N} w_{ij} \tag{7}$$

where e_i is the residual for county i. The conditional Lagrange multiplier tests of Baltagi et al. (2003) and the Rao score tests of Anselin et al. (1996) then distinguished a spatial-lag process from a spatial-error process. Seven candidate models were fitted in total: spatial-lag and spatial-error specifications under both fixed and random effects, a spatial Durbin fixed-effects model, a Kapoor-Kelejian-Prucha random-effects model that jointly handles spatial and serial correlation, and a common-correlated-effects (CCE) augmented spatial-lag fixed-effects model following Pesaran (2006). Rather than selecting on the information criteria alone, each candidate was screened against the five diagnostic requirements set out in Section 3.10, and the candidate satisfying the most requirements was retained as the final model, with the Akaike and Bayesian information criteria used only to break ties among candidates satisfying an equal number of requirements.

3.7. Cross-Sectional Dependence and the CCE-Augmented Estimator

County finances share exposure to national policy shifts, common macroeconomic conditions, and nationwide programs, which induces cross-sectional dependence in the residuals. This was tested with the Pesaran CD statistic in Equation (8):

$$\text{CD} = \sqrt{\frac{2T}{N(N-1)}} \left(\sum_{i=1}^{N-1} \sum_{j=i+1}^{N} \hat{\rho}_{ij} \right) \tag{8}$$

where $\hat{\rho}_{ij}$ is the pairwise correlation of residuals between counties i and j. Because the raw panel showed strong dependence, the model was augmented following the common correlated effects approach of Pesaran (2006), which adds year-specific cross-sectional averages of the regressors as proxies for the unobserved common factors, as in Equation (9):

$$y_{it} = \lambda \sum_{j=1}^{N} w_{ij} y_{jt} + x_{it}' \beta + h_t' \gamma + \mu_i + \varepsilon_{it} \tag{9}$$

This CCE-augmented specification was fitted as one of the seven candidates evaluated in Section 3.6. As reported in Section 4.6, it achieves the best information criteria among all seven candidates and, once the constant-KES conversion and the INFL/TREND common-time controls (Section 3.4) are in place, it ties two other candidates on the five-point assumption scorecard; among the tied candidates it has the lowest AIC and is therefore the final model carried forward through Section 4. The CCE augmentation still does not resolve the residual cross-sectional dependence it targets (Section 4.6), so, as in every candidate, the group- and time-clustered HAC standard errors described in Section 3.8 remain the basis for reported inference.

3.8. Robust Inference

Diagnostic testing showed that serial correlation and cross-sectional dependence remained in the final model even though it cleared the spatial-autocorrelation check. To keep inference valid, standard errors were computed using the heteroskedasticity- and autocorrelation-consistent (HAC) estimator of Arellano (1987), clustered by county, and separately clustered by fiscal year following Thompson (2011); both clustered estimates, not the county-clustered estimate alone, are reported for every regressor. Reported significance levels for the final model rest on these two robust standard errors alongside the maximum-likelihood standard error, all three of which are presented side by side in Section 4.8, since vcovHC-style clustered estimators are not directly available for spml objects and are instead computed on the equivalent individual-fixed-effects panel form of the final model.

3.9. Direct, Indirect, and Total Effects

In a spatial autoregressive model the estimated coefficients are not the full marginal effects, because a change in a regressor in one county feeds back through the spatial multiplier to every other county. The complete set of responses is captured by the partial-derivative matrix in Equation (10), following LeSage and Pace (2009):

$$\frac{\partial y}{\partial x_k} = \left(I_N - \lambda W\right)^{-1} \beta_k \tag{10}$$

The average of the diagonal elements gives the direct effect (the impact of a county's own regressor on its own spending), the average of the off-diagonal row sums gives the indirect (spillover) effect, and their sum gives the total effect. Standard errors, 95% confidence intervals, and p values for these quantities were obtained from 1000 Monte Carlo draws of the parameter vector (β, λ) from its estimated sampling distribution, with the full decomposition recomputed on each draw.

3.10. Diagnostic Checking

Every candidate model was subjected to the same five-point battery of residual diagnostics, and the candidate satisfying the most of the five was retained as the final model (Section 3.6). Normality was assessed with the Shapiro-Wilk statistic on the model residuals, together with the histogram and normal quantile-quantile plot;

given the sample of 517 observations, the central limit theorem also ensures that the estimators are approximately normally distributed even under mild departures from normality. Homoskedasticity was assessed with the Breusch-Pagan test, freedom from residual spatial autocorrelation with Moran's I computed separately for every fiscal year, freedom from serial correlation with the Wooldridge (2002) test for panels, and freedom from cross-sectional dependence with the Pesaran CD statistic. Multicollinearity was screened using variance inflation factors with the conventional threshold of 10, and panel stationarity was examined with the Im-Pesaran-Shin and Levin-Lin-Chu tests as a screening check, though the short time dimension of the panel ($T = 11$) prevented these particular tests from converging.

4. Results and Discussion

4.1. Descriptive Statistics

Table 2 presents descriptive statistics for all study variables in nominal terms, in constant fiscal-year-2014/15 Kenya shillings, and, for the continuous fiscal variables, in log-transformed form; a separate panel reports the deflator index, the fiscal-year inflation rate, and the linear trend used as common-time controls (Section 3.4). In nominal terms, development expenditure averaged KES 2.18 billion per county-year (SD = 1.20 billion); in constant FY2014/15 terms the same series averages KES 1.63 billion (SD = 1.00 billion), the gap between the two reflecting the roughly 80% cumulative price growth (deflator = 180.41 by FY2024/25) the panel spans. Pending bills carries the most extreme distributional problem in either currency basis, with a raw skewness of 8.94 and a nominal maximum of KES 118.32 billion driven largely by Nairobi City's accumulated obligations. The ln(1 + x) transformation, combined with Winsorization of the constant-KES pending-bills series at the 99th percentile (a cap of KES 46.53 billion), brought skewness down substantially, from 1.912 to .712 for development expenditure and from 8.940 to 3.475 for the log-Winsorized pending-bills series, though both remain somewhat elevated because deflating stretches the earliest, lowest-price years relative to the most recent ones. Election-year fiscal years (EL = 1) account for 188 of the 517 county-year observations (36.4%), covering the four fiscal years, 2017, 2018, 2022, and 2023, that bracket Kenya's 2017 and 2022 general elections; the fiscal-year inflation rate over the panel averaged 6.14% (SD = .96 percentage points, range 4.27% - 7.67%).

Table 2. Descriptive statistics for nominal, constant FY2014/15-KES, and log-transformed variables, with common-time controls ($N \times T = 517$).

Variable	*N*	*M*	SD	Min	Max	Skewness
Panel A: Nominal variables (KES billions)						
DE	517	2.180	1.202	.100	11.910	1.912
WB	517	3.538	2.140	.650	18.300	—
OSR	517	.872	1.761	.027	13.530	—
NGF	517	6.689	2.682	1.750	20.180	—

Continued

DPF	517	.543	.460	.000	3.701	—
PB	517	2.228	10.324	.000	118.320	8.940
PC	517	.476	.500	.000	1.000	—
EL	517	.364	.482	.000	1.000	—
Panel B: Constant FY2014/15-KES (real, CPI-deflated) variables						
DEr	517	1.628	1.000	.083	11.910	—
WBr	517	2.556	1.501	.631	13.920	—
OSRr	517	.633	1.304	.024	11.500	—
NGFr	517	4.901	1.834	1.722	13.698	—
DPFr	517	.390	.335	.000	2.761	—
PBr	517	1.525	6.654	.000	68.387	—
Panel C: Common-time controls						
DEFLATOR	517	137.549	25.750	100.000	180.412	—
INFL	517	6.141	.960	4.275	7.667	—
TREND	517	6.000	3.165	1.000	11.000	—
Panel D: Log-transformed variables (ln(1 + x) of constant-KES values)						
lDE	517	.912	.316	.079	2.558	.712
lWB	517	1.212	.312	.489	2.703	—
lOSR	517	.367	.405	.024	2.526	—
lNGF	517	1.732	.289	1.002	2.688	—
lDPF	517	.305	.210	.000	1.325	—
lPB	517	.535	.532	.000	3.861	3.475

M = mean; SD = standard deviation. Monetary variables are in billions of Kenya shillings; the r subscript denotes constant FY2014/15-KES (deflated) values. Log-transformed variables use $\ln(1 + x)$ applied to the constant-KES series; PBr is Winsorized at the 99th percentile before transformation. DEFLATOR is the fiscal-year CPI deflator (Section 3.3), rebased so FY2014/15 = 100; INFL is the fiscal-year inflation rate (%); TREND is a linear time index (1, ⋯, 11).

4.2. Correlation Analysis

Table 3 reports Pearson correlations for the log-transformed and common-time-control variables. The equitable share (lNGF) has the strongest bivariate association with log development expenditure (lDE), r = .715, followed by the wage bill (lWB), r = .379, and own-source revenue (lOSR), r = .294. The election-year indicator (EL) is significantly and negatively correlated with lDE (r = −.172, $p < .001$), but Table 3 also shows why that raw correlation cannot, on its own, be read as an electoral-cycle effect: EL correlates with the fiscal-year inflation rate at r = .684 ($p < .001$), a very high correlation for a panel spanning only 11 distinct fiscal years, and lDE itself correlates with the linear time trend at r = −.217 ($p < .001$) and negligibly with inflation (r = −.008, ns). In other words, election years in this sample happen to coincide with a distinctive part of the inflation path, so a raw or even a fixed-effects-only EL coefficient will pick up part of that coincidence along with, or instead of, any genuine electoral-cycle response; Section 3.4 and Section 4.11.6 return to this point directly. The correlations between lWB and lOSR (r = .766) and between lWB and lNGF (r = .749) remain fairly high, though variance inflation factors confirm that multicollinearity stays within acceptable bounds throughout, with a maximum value of 5.405, for lWB, well below the common threshold of 10 (Hair et al., 2019); EL's own VIF is a modest 2.142 despite its high pairwise correlation with INFL, because a VIF, unlike a raw correlation, accounts for all the other regressors in the model at once. Political coalition shows near-zero correlations with all fiscal variables and with EL (r = .004), foreshadowing the null finding reported later in the regression results.

Table 3. Pearson correlation matrix for log-transformed variables ($N \times T = 517$).

Variable	lDE	lWB	lOSR	lNGF	lDPF	lPB	PC	EL	INFL	TREND
lDE	1.000									
lWB	.379***	1.000								
lOSR	.294***	.766***	1.000							
lNGF	.715***	.749***	.518***	1.000						
lDPF	.218***	.436***	.183***	.318***	1.000					
lPB	.154**	.504***	.567***	.415***	.093*	1.000				
PC	−.126**	−.089*	−.007	−.208***	−.034	−.076	1.000			
EL	−.172***	.021	−.066	.062	−.123**	.026	.004	1.000		
INFL	−.008	−.079	−.070	.040	−.314***	−.016	−.046	.684***	1.000	
TREND	−.217***	.126**	.034	−.113*	.167***	.094*	.088*	.000	−.312***	1.000

*$p < .05$. **$p < .01$. ***$p < .001$.

4.3. Spatial Distribution and Temporal Evolution

Figures 1-5 map the spatial footprint of development expenditure, national fiscal transfers, the wage bill and development partner funds across all 47 counties over the 2015 to 2025 period, and trace log development-expenditure trends for the six highest-spending counties.

Figure 1 traces the geography of development spending across the 11 fiscal years. Two features stand out. First, the darkest shading in the early years sits in the northern and northeastern counties (Turkana, Marsabit, Mandera, and Garissa), where 2015 development budgets were the largest in the country, consistent with an equitable-share formula that channels larger allocations to sparsely populated, historically marginalized counties. Second, the maps lighten and even out over time, the intense reds of 2015 and 2016 fading to more uniform oranges and yellows from 2018 onward, which points to gradual convergence as the biggest early spenders came down toward the national average while smaller counties edged up. The one striking late-period exception is Kilifi on the coast, which turns deep red in 2025; its development expenditure of KES 6.71 billion was the highest single value anywhere in the panel, reflecting the absorption of large donor-financed coastal infrastructure projects.

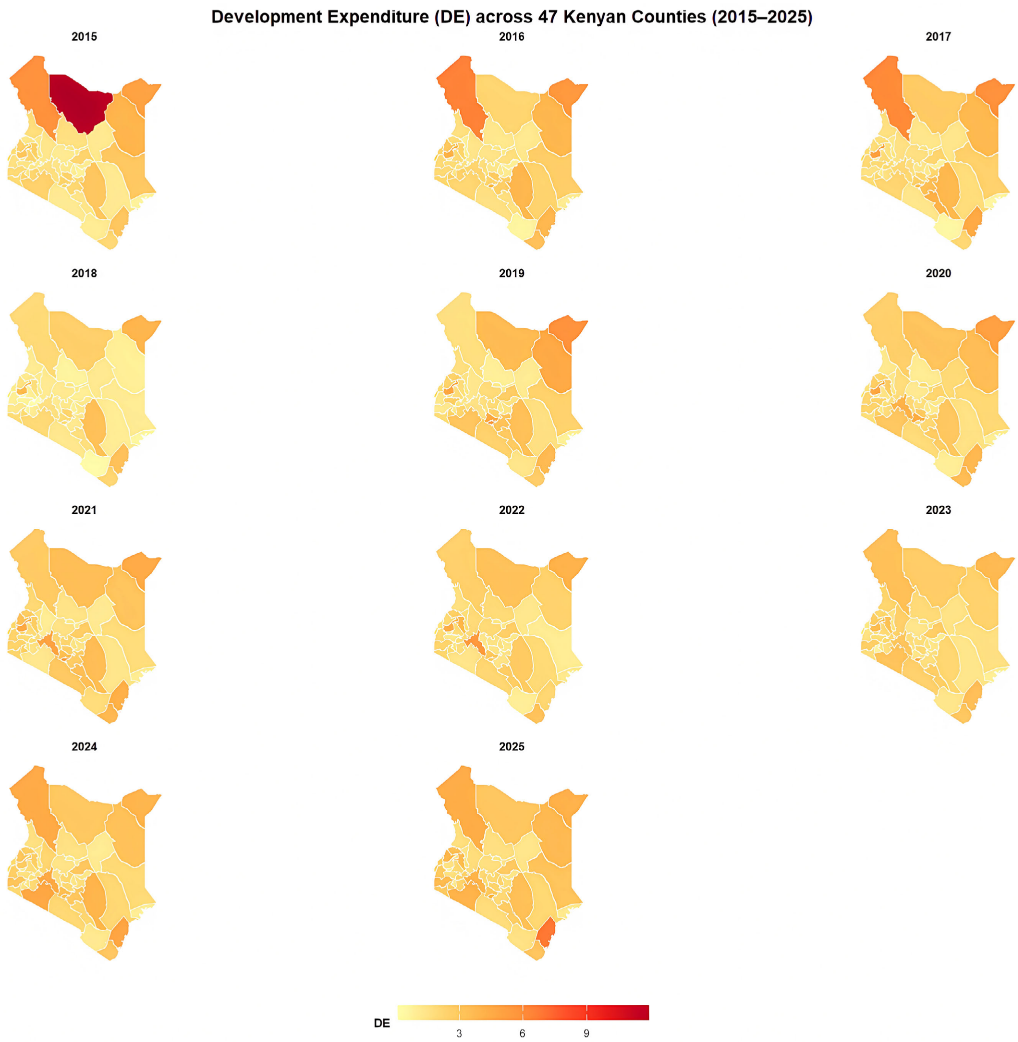

Figure 1. Development expenditure (DE) across 47 Kenyan counties, 2015-2025.

Figure 2 shows the equitable share, the single largest revenue stream for most counties. The spatial pattern is remarkably stable across all 11 fiscal years: the large northern counties consistently receive the deepest shading, while the small central-highland counties receive the least. This persistence is expected, since the Commission on Revenue Allocation formula rests on slow-moving factors such as population, poverty, land area, and a basic equal share. What does change is the overall intensity, as the maps darken gradually toward 2025, mirroring the growth of the national equitable-share pool over the devolution decade.

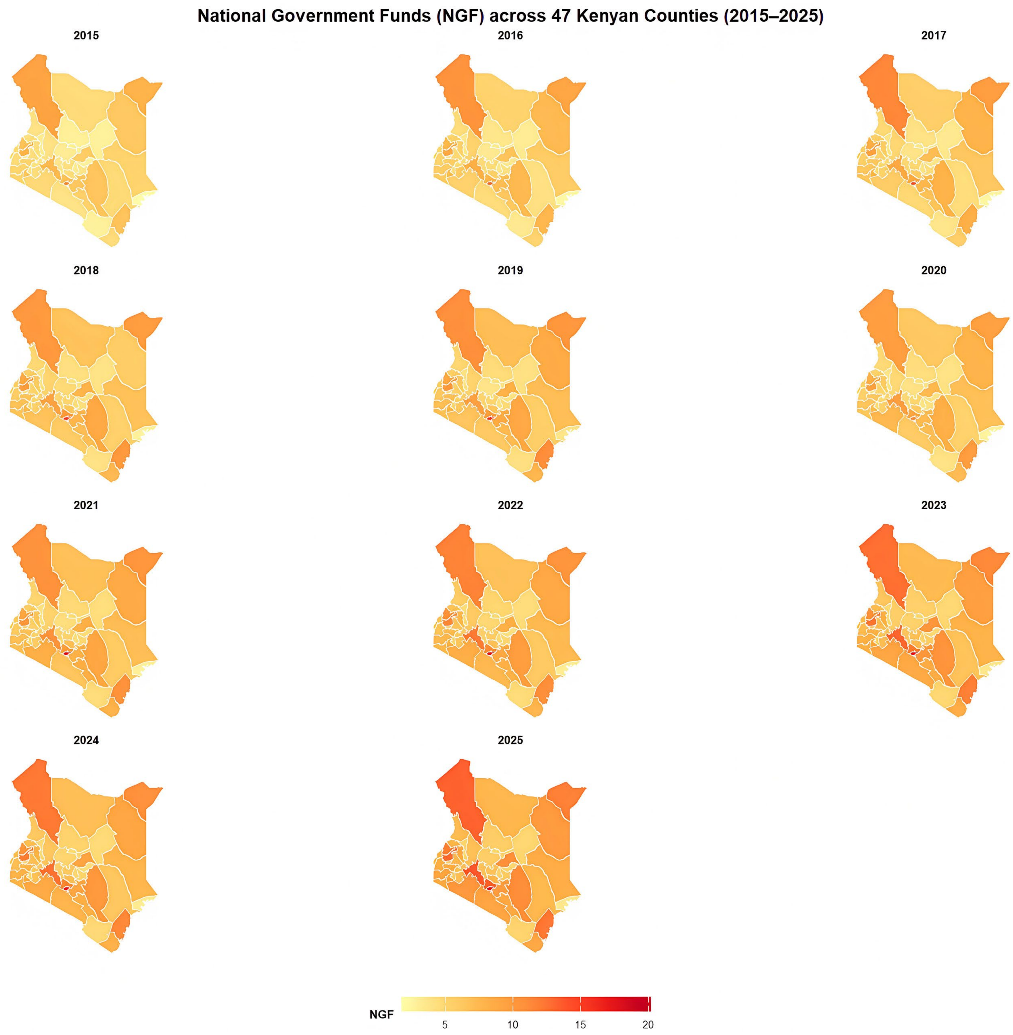

Figure 2. National government fiscal transfers (NGF) across 47 Kenyan counties, 2015-2025.

Figure 3 maps the wage bill, and its dominant visual message is steady, nationwide growth. In 2015 most of the map is pale, indicating modest payrolls; as the panels advance, shading deepens almost everywhere, with the pace accelerating visibly from around 2021 onward, consistent with public-sector salary reviews and the absorption of large numbers of health workers onto county payrolls. Read alongside **Figure 1**, this figure previews the central tension of the study: as the wage-bill map darkens over time, the development-expenditure map lightens, hinting at the crowding-out relationship that the regression results confirm formally.

Figure 4 maps development partner funds, the conditional grants and donor financing counties receive. The striking feature is how uneven and lumpy this stream is, both across counties and across years. Unlike the smooth, formula-driven equitable share, donor funds appear as isolated bright patches that shift location from year to year, reflecting the project-specific and time-bound nature of donor commitments. This volatility is precisely why donor funds, though smaller in total than the equitable share, contribute genuine additional variation to development spending rather than simply tracking the other revenue streams.

Figure 3. Wage bill (WB) across 47 Kenyan counties, 2015-2025.

Figure 5 follows the six highest-spending counties over time on the log scale. The lines are volatile rather than smooth, which is itself informative: development expenditure swings sharply from year to year as multi-year capital projects start, stall, and complete. Marsabit is the clearest example, opening as the single highest spender in 2015 and then dropping steeply the following year, a fall that reflects the completion or suspension of a large project rather than a policy shift. From the middle of the period, the six lines converge into a tighter band, echoing the convergence seen in the choropleth maps, before Kilifi climbs steadily to become the top spender by 2025 and Kakamega falls sharply over the last two fiscal years.

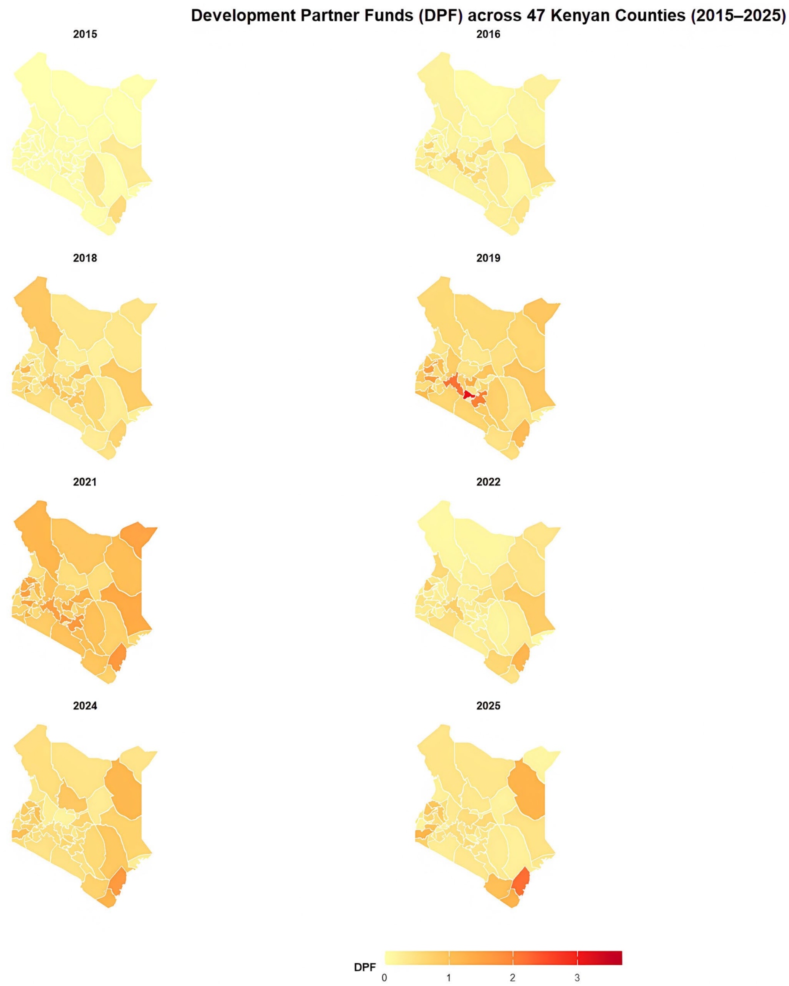

Figure 4. Development partner funds (DPF) across 47 Kenyan counties, 2015-2025.

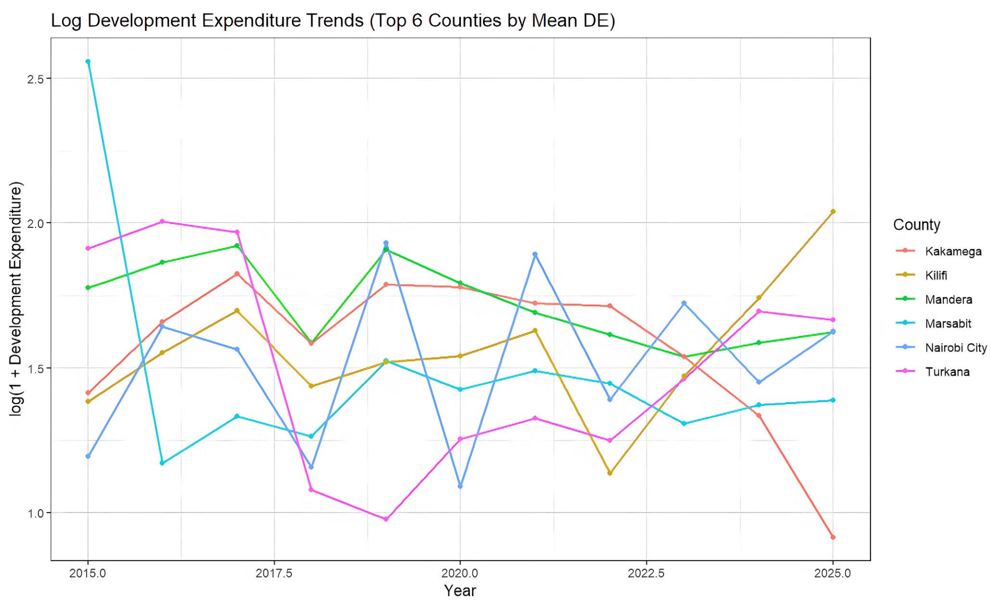

Figure 5. Log development expenditure trends for the top six counties by mean DE, 2015-2025.

4.4. Panel Specification Tests

Table 4 summarizes the three specification tests used to determine the appropriate panel framework before spatial structure was introduced. Both the F test (F = 2.733, df1 = 46, df2 = 461, $p < .001$) and the Breusch-Pagan LM test ($\chi^2 = 36.438$, df = 1, $p < .001$) decisively reject pooled OLS in favor of a panel model with county-specific effects. With the constant-KES conversion and the INFL/TREND common-time controls now in the model, neither the standard Hausman test ($\chi^2 = 9.254$, df = 9, $p = .414$) nor the spatial Hausman test of Mutl and Pfaffermayr (2011) ($\chi^2 = 13.186$, df = 9, $p = .154$) rejects random effects, a materially weaker result than in an earlier nominal-only specification, where the spatial Hausman test rejected random effects overwhelmingly. This does not change which specifications are carried forward: fixed effects are retained across all seven candidate models not because the Hausman evidence demands it here, but because individual (county) fixed effects, as opposed to a random-effects or two-way fixed-effects structure, are what keeps EL, INFL, and TREND identified in the first place (Section 3.4), and because the diagnostic-based selection rule described in Sections 3.6 and 3.10, and applied in Section 4.6, the paper's pre-committed rule for choosing among candidates, continues to select fixed-effects specifications among the models satisfying the most diagnostic requirements. The weaker Hausman result is itself informative: it suggests that once common nationwide price and trend movements are explicitly modeled, county-specific effects explain relatively less of the remaining cross-sectional heterogeneity than they did in the nominal specification.

Table 4. Panel specification tests.

Test	Statistic	Df	p	Decision
F test: pooled OLS vs. fixed effects	F = 2.733	46, 461	<.001	Fixed effects
Breusch-Pagan LM: pooled vs. random effects	$\chi^2 = 36.438$	1	<.001	Random effects
Hausman: fixed vs. random effects (non-spatial)	$\chi^2 = 9.254$	9	.414	Random effects (not decisive; see text)
Spatial Hausman: SAR-FE vs. SAR-RE	$\chi^2 = 13.186$	9	.154	Random effects (not decisive; see text)

4.5. Spatial Dependence Diagnostics

Table 5 reports the suite of spatial-dependence tests applied to the panel residuals. Moran's I is significantly positive in 2015 (I = .164, $p = .007$), the clearest evidence that development expenditure was spatially correlated at the start of the devolution period; it is not significant in any other fiscal year. The Baltagi-Song-Koh conditional test confirms spatial-lag dependence given random effects (CLMλ = 5.197, $p < .001$) and the random-effects component given spatial dependence (CLMμ = 5.535, $p < .001$). The Rao score tests point to a spatial-lag rather than a spatial-error process (robust RS lag = 1.917, $p = .166$, versus robust RS error = .023, $p = .879$), supporting the SAR specification; these robust RS/LM tests are computed, as in Section 3.6, on a simple time-averaged cross-section using the original nominal fiscal variables and are unaffected by the constant-KES conversion or the INFL/TREND controls.

Table 5. Spatial dependence diagnostic tests on non-spatial fixed-effects residuals.

Test/year	Statistic	*P*	Inference
Moran's I 2015	.164	.007	Significant autocorrelation
Moran's I 2016	−.087	.764	Not significant
Moran's I 2017	−.109	.831	Not significant
Moran's I 2018	.055	.197	Not significant
Moran's I 2019	.065	.166	Not significant
Moran's I 2020	.091	.105	Not significant
Moran's I 2021	.080	.121	Not significant
Moran's I 2022	−.090	.776	Not significant
Moran's I 2023	.029	.278	Not significant
Moran's I 2024	−.118	.857	Not significant
Moran's I 2025	.018	.324	Not significant
BSK CLMλ (lag|RE)	5.197	<.001	Spatial lag confirmed
BSK CLMμ (RE|lag)	5.535	<.001	Random effects confirmed
RS lag test	3.347	.067	Marginal lag signal
RS error test	1.454	.228	Not significant
Robust RS lag	1.917	.166	Lag weakly dominant
Robust RS error	.023	.879	Not significant

4.6. Model Comparison

Table 6. Spatial panel model comparison (maximum likelihood estimation).

Model	Log-likelihood	*K*	AIC	BIC	Note
SAR-FE	208.487	10	−396.974	−354.493	—
SEM-FE	−673.534	10	1367.069	1409.549	Poor fit
SAR-RE	157.674	10	−295.349	−252.868	—
SEM-RE	158.062	10	−296.125	−253.644	—
SDM-FE	—	11	—	—	Did not converge
KKP-RE	158.749	10	−297.499	−255.018	—
SAR-FE (CCE)	219.430	15	−408.859	−345.139	Best Model

Table 6 compares the seven spatial panel models estimated by maximum likelihood on the revised, constant-KES specification with INFL and TREND added, including a common-correlated-effects (CCE) augmented variant. Judged by information criteria alone, the CCE-augmented SAR-FE model remains the strongest fit, and by a wider margin than in an earlier nominal-only draft: AIC = −408.859

and BIC = −345.139, both better than the plain SAR-FE model's −396.974 and −354.493. The spatial Durbin fixed-effects model again did not converge under the SARAR specification and was excluded from further comparison. Information criteria alone, however, say nothing about whether a model's residuals actually satisfy the assumptions needed for valid inference, so each surviving candidate was also screened against the five-point diagnostic battery described in Section 3.10 before a final model was chosen. All seven candidates pass the normality screen using the Shapiro-Wilk W statistic itself (W > .95) rather than its *p*-value, which is highly sensitive to small departures from exact normality at this sample size (all $p < .001$, so W is used as the practical threshold, as in Section 3.10). The plain SAR-FE model, the spatial Durbin fixed-effects model, and the CCE-augmented SAR-FE model are the only three candidates that also clear the spatial-autocorrelation check, with Moran's I insignificant in 100% of fiscal years for all three; none of the seven, however, clears the homoskedasticity check under the revised specification (Breusch-Pagan $p = .039$, .033, and .013 for these same three, respectively), and none clears the serial-correlation or cross-sectional-dependence checks outright. On this count, SAR-FE, SDM-FE, and SAR-FE (CCE) are tied at two of five diagnostics satisfied; among these three, SAR-FE (CCE) has the lowest AIC (−408.859, against −396.974 for SAR-FE and a non-converging fit for SDM-FE) and is therefore the final model carried forward through the rest of Section 4.

4.7. Post-Estimation Diagnostics

Figure 6 presents the residual diagnostic plots for the final SAR-FE (CCE) model. The residuals-versus-fitted plot shows points scattered around zero without a strong funnel shape, consistent with the approximate linearity of the log-log specification. The Moran scatterplot for the last fiscal year shows a shallow, near-flat fitted slope, consistent with residual spatial randomness rather than clustering, which Table 7 confirms numerically. The scale-location plot, though, shows a mild upward drift consistent with the Breusch-Pagan result in Table 8: the model does not clear the homoskedasticity check even with the constant-KES conversion and the INFL/TREND controls included. The mean-residuals-over-time plot hovers around zero for most of the panel, with excursions around the fiscal years immediately following Kenya's 2017 and 2022 general elections; because EL, INFL, and TREND are already regressors in this model, these excursions reflect within-year variation that the common time controls cannot fully absorb, rather than a straightforward omitted election effect. The histogram of residuals is bell-shaped and reasonably symmetric, and the normal quantile-quantile plot follows the reference line closely through the middle of the distribution, departing somewhat at the extreme upper tail. The formal Shapiro-Wilk test rejects exact normality (W = .958, p < .001), which is unsurprising given the sample size of 517 observations; the central limit theorem ensures that the sampling distribution of the estimators is still approximately normal, so inference remains protected, and the group- and time-clustered HAC standard errors reported in Table 9 provide a further layer of protection against the remaining heteroskedasticity, serial correlation, and cross-sectional dependence.

Table 7 reports Moran's I on the final SAR-FE (CCE) model's residuals for every fiscal year in the panel. In all 11 years the statistic is small and statistically insignificant (all $p > .05$, and most $p > .5$), confirming that the spatial autoregressive term has fully absorbed the spatial dependence that was present in the raw panel before any spatial model was fitted.

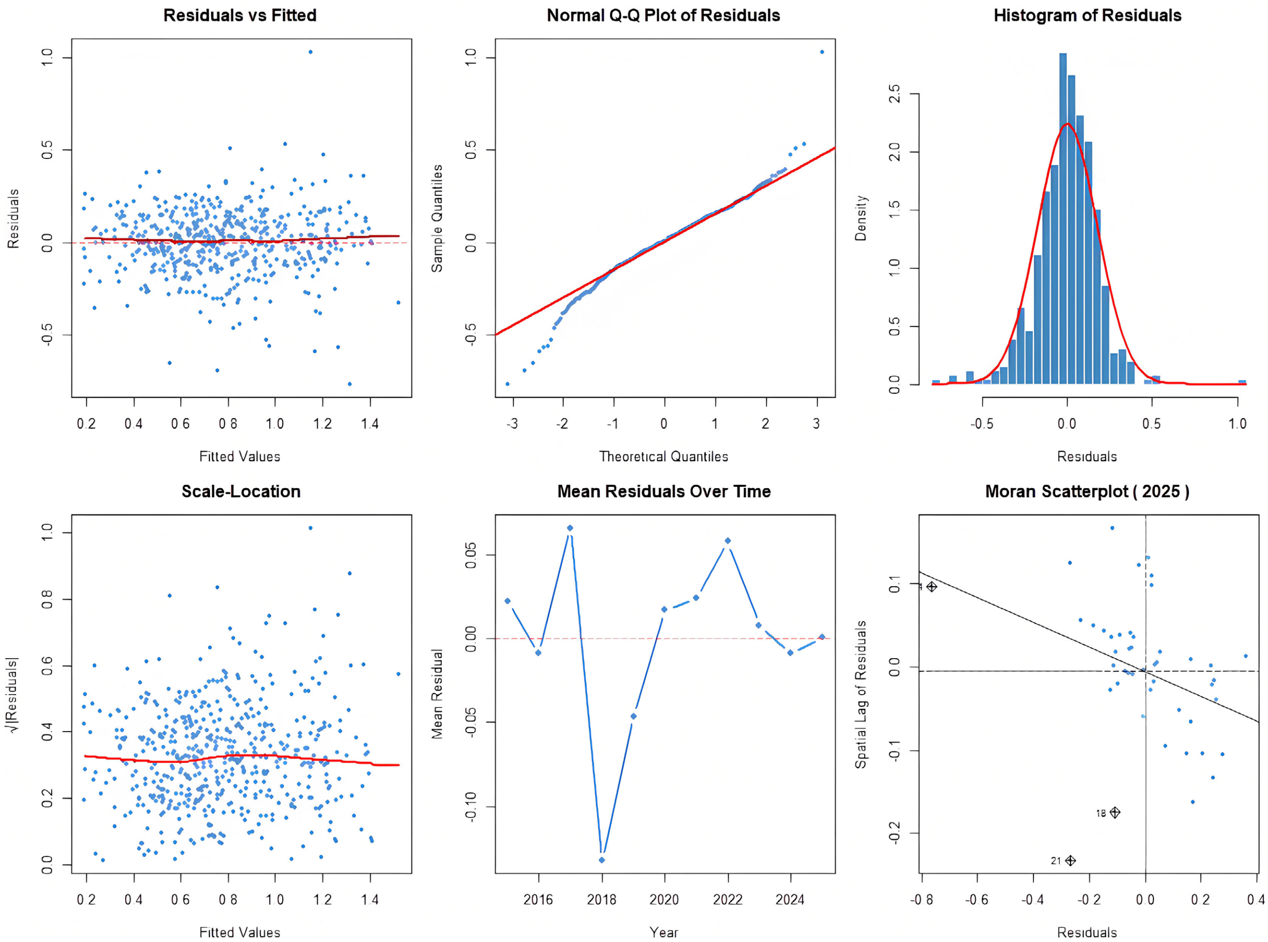

Figure 6. Residual diagnostic plots for the final SAR-FE model.

The remaining assumptions, reported in Table 8, are ones for which a plot is not decisive. Cross-sectional dependence is present in the final model's residuals (Pesaran CD = 6.891, $p < .001$) and is not resolved by the CCE cross-sectional-mean augmentation, despite that augmentation being specifically designed to address it; serial correlation is also present (Wooldridge AR(1) = 9.975, $p = .002$), and homoskedasticity is not satisfied either (Breusch-Pagan = 20.838, df = 9, $p = .013$). Rather than treating any one of these as decisive on its own, all three are handled through the group- and time-clustered HAC-robust standard errors reported in Table 9. Multicollinearity is not a concern, with a maximum VIF of

5.405 (for lWB); EL's own VIF of 2.142 is unremarkable, even though its raw correlation with INFL is high (r = .684, Table 3), because variance inflation factors, unlike a pairwise correlation, account for the other seven regressors also in the model.

Table 7. Moran's I on final SAR-FE (CCE) model residuals, by fiscal year.

Fiscal year	Moran's I	*P*	Spatial autocorrelation?
2015	.090	.074	No
2016	−.141	.907	No
2017	−.212	.983	No
2018	−.042	.588	No
2019	−.011	.453	No
2020	.009	.365	No
2021	−.023	.506	No
2022	−.146	.917	No
2023	−.046	.608	No
2024	−.123	.868	No
2025	−.132	.904	No

Table 8. Test-based diagnostics where a plot is not definitive.

Assumption	Test	Statistic	*p*	Handling
Heteroskedasticity	Breusch-Pagan	BP = 20.838, df = 9	.013	HAC SE applied
Serial correlation	Wooldridge AR (1)	stat = 9.975	.002	HAC SE applied
Cross-sectional dependence	Pesaran CD	stat = 6.891	<.001	HAC SE applied
Multicollinearity	VIF (maximum)	5.405	—	<10; acceptable

4.8. The Final Model: SAR-FE (CCE)

Table 9 presents the final SAR-FE (CCE) model's estimates with maximum-likelihood, group-clustered HAC, and time-clustered HAC standard errors reported side by side. The spatial autoregressive parameter, $\lambda = .204$ ($p < .001$) in the final model, is positive and significant, though smaller than the $\lambda = .270$ estimated by the plain SAR-FE candidate; Section 4.11.1 discusses what this parameter can and cannot be read as showing. Because the model is specified in $\ln(1 + x)$ form, the five continuous regressors' coefficients are close to, but not exactly, elasticities (Section 3.3); the two binary indicators, PC and EL, are reported below as coefficients and, where useful, as transformed predicted differences rather than approximate percentage changes. Four variables are significant under maximum likelihood and both HAC estimators, namely lWB (negative), lNGF (positive), lDPF (positive), and lPB (negative); lOSR is only marginally significant under maximum likelihood ($p = .089$) and loses significance entirely under either HAC estimator. Political coalition (PC) is insignificant under every specification tested.

INFL and TREND, the two common-time controls, are themselves significant under maximum likelihood (INFL: β = −.063, p = .024; TREND: β = .018, p = .029) and remain significant, or close to it, under HAC inference (Table 9), confirming that they are capturing real common-time variation rather than adding noise. EL is the coefficient with the least stable interpretation across specifications: under the final model it is small and statistically indistinguishable from zero (β = −.035; ML p = .505; group-clustered HAC p = .450; time-clustered HAC p = .753). Evaluated as a transformed predicted difference at the panel's mean constant-KES development expenditure (KES 1.628 billion), this coefficient corresponds to a predicted difference of about −KES .091 billion, not statistically distinguishable from zero. Under the plain SAR-FE candidate, estimated identically but without the CCE cross-sectional-mean augmentation (a supplementary check, not separately tabulated here since Table 9 reports the final model only), by contrast, EL remains negative and significant (β = −.135, p < .001 under maximum likelihood), a predicted difference of about −KES .332 billion at the same baseline; the fact that this result appears or disappears depending on whether the CCE cross-sectional-mean augmentation, which absorbs much of the same year-to-year common variation that EL, INFL, and TREND rely on for identification, is included is itself the clearest evidence in this paper that the election-year coefficient is not robustly separable from other nationwide shocks in these same fiscal years, and it is discussed on those terms, as an association rather than a settled causal electoral-cycle effect, throughout Section 4.11.6.

Table 9. Final SAR-FE (CCE) model estimates of the determinants of county development expenditure, with group- and time-clustered hac standard errors.

Variable	ML est.	ML SE	ML p	Grp-HAC est.	Grp-HAC SE	Grp-HAC p	Time-HAC est.	Time-HAC SE	Time-HAC p
lWB	−.474	.091	<.001	−.500	.115	<.001	−.500	.125	<.001
lOSR	.123	.072	.089	.120	.144	.407	.120	.098	.223
lNGF	.870	.144	<.001	.904	.241	<.001	.904	.148	<.001
lDPF	.247	.063	<.001	.246	.079	.002	.246	.066	<.001
lPB	−.072	.020	<.001	−.077	.021	<.001	−.077	.028	.006
PC	.003	.031	.928	−.004	.031	.901	−.004	.035	.914
EL	−.035	.053	.505	−.039	.052	.450	−.039	.125	.753
INFL	−.063	.028	.024	−.079	.021	<.001	−.079	.035	.024
TREND	.018	.008	.029	.023	.008	.006	.023	.018	.195
λ (spatial AR)	.204	.059	<.001						
Log-likelihood	219.430								
AIC	−408.859								
BIC	−345.139								
N × T	517								

4.9. Direct, Indirect, and Total Spatial Effects

Because a change in any regressor in one county propagates through the spatial multiplier matrix to every other county, the raw coefficients in Table 9 understate the full fiscal impact for the five continuous regressors and PC/EL. Table 10 decomposes each coefficient of the final SAR-FE (CCE) model into direct (own-county), indirect (cross-county spillover), and total (system-wide) effects, with standard errors, 95% confidence intervals, and significance levels drawn from 1000 Monte Carlo draws of the parameter vector. National transfers do the most work: the direct effect of .879 is highly significant, the indirect spillover of .224 is significant on its own, and the two sum to a total system-wide elasticity of 1.103, 95% CI [.709, 1.546]. Because the interval lies entirely above unity, the pass-through of equitable-share increases into aggregate development spending remains near-complete, indeed slightly more than complete, once cross-county spillovers are counted. The wage bill carries a significant negative total effect of −.605, with both its direct (−.483) and indirect (−.123) components significant. Development partner funds carry a significant positive total effect of .314, and pending bills a significant negative total effect of −.092; both again split into significant direct and indirect components. Political coalition remains insignificant across every effect, its confidence intervals straddling zero (total = .004, 95% CI [−.073, .083]). The election-year indicator, by contrast, is no longer a significant regressor in this decomposition: its direct effect of −.038 and indirect effect of −.009 combine into a total system-wide elasticity of −.047, 95% CI [−.183, .090], an interval that straddles zero comfortably. A supplementary decomposition of the plain SAR-FE candidate, not subject to the CCE augmentation and not separately tabulated here, finds a direct effect of −.137 and a total effect of −.185 for EL, both far more negative than in the final model; the size of this swing across two candidates that satisfy the same diagnostic checks (Section 4.6) is, on its own, a reason to treat the election-year effect as fragile rather than settled, and it is discussed on those terms in Section 4.11.6.

Table 10. Direct, indirect, and total spatial effects with simulated standard errors: final SAR-FE (CCE) model.

Variable	Effect	Estimate	SE	95% CI	Sig.
lWB	Direct	−0.483	0.093	[−0.667, −0.297]	***
lWB	Indirect	−0.123	0.050	[−0.240, −0.046]	***
lWB	Total	−0.605	0.125	[−0.869, −0.370]	***
lOSR	Direct	0.125	0.073	[−0.026, 0.264]	
lOSR	Indirect	0.032	0.022	[−0.006, 0.082]	
lOSR	Total	0.156	0.092	[−0.031, 0.332]	
lNGF	Direct	0.879	0.145	[0.591, 1.157]	***
lNGF	Indirect	0.224	0.091	[0.084, 0.442]	***
lNGF	Total	1.103	0.204	[0.709, 1.546]	***

Continued

lDPF	Direct	0.251	0.065	[0.125, 0.376]	**
lDPF	Indirect	0.064	0.028	[0.020, 0.127]	**
lDPF	Total	0.314	0.085	[0.151, 0.482]	**
lPB	Direct	−0.073	0.021	[−0.115, −0.031]	***
lPB	Indirect	−0.019	0.009	[−0.040, −0.006]	***
lPB	Total	−0.092	0.027	[−0.147, −0.039]	***
PC	Direct	0.003	0.031	[−0.059, 0.066]	
PC	Indirect	0.001	0.008	[−0.015, 0.019]	
PC	Total	0.004	0.039	[−0.073, 0.083]	
EL	Direct	−0.038	0.055	[−0.148, 0.070]	
EL	Indirect	−0.009	0.015	[−0.039, 0.019]	
EL	Total	−0.047	0.069	[−0.183, 0.090]	

CI = confidence interval. *$p < .05$. **$p < .01$. ***$p < .001$.

4.10. Spatial Weights Robustness

A recurring concern with spatial models is that the estimated dependence might be an artifact of the particular weights matrix chosen. Table 11 therefore re-estimates the final SAR-FE (CCE) model under k-nearest neighbors ($k = 5$, close to the Queen average of 5.19 neighbors) and Rook contiguity; the inverse-distance weights matrix could not be constructed for this shapefile and is omitted. The results are reassuringly stable: the spatial parameter ranges only from .201 (Rook) to .253 (k-NN), bracketing the Queen baseline of .204, and every fiscal elasticity is essentially unchanged, with lWB near −.474, lNGF near .88, lDPF near .247, and lPB near −.072 across all three weights matrices. The election-year coefficient is also stable across weights matrices, at −.033 to −.035, but stability across weights matrices is not the concern Section 4.6 and Section 4.11.6 raise about EL; that concern is that its identification depends on how common time factors, not spatial weights, are modeled, and it is unaffected by this check.

Table 11. Spatial weights robustness: final SAR-FE (CCE) model under alternative weights matrices.

Weights matrix	λ	lWB	lNGF	lDPF	lPB	EL
Queen (baseline)	.204	−.474	.870	.247	−.072	−.035
k-nearest neighbors ($k = 5$)	.253	−.474	.882	.247	−.074	−.033
Rook contiguity	.201	−.475	.870	.247	−.072	−.035

4.11. Discussion of Findings

4.11.1. Spatial Interdependence in County Development Spending

The spatial parameter, $\lambda = .204$ in the final model (.270 in the plain SAR-FE candidate), says that counties do not set their capital budgets in a vacuum: a county's development spending tracks what its neighbors are doing, and that link survives once we control for the equitable share, the wage bill, political coalition, and the common-time controls. What this parameter can be read as showing, however, is limited. Persistent cross-sectional dependence in the residuals (**Table 8**; Pesaran CD = 6.891, $p < .001$, even in the CCE-augmented model designed to address it) means that neighboring counties' development budgets can move together for reasons that have nothing to do with a genuine fiscal spillover, in particular shared national policy shifts, common macroeconomic shocks, or regional weather and commodity-price events that hit adjacent counties simultaneously; the spatial autoregressive coefficient does not, on its own, distinguish a true spillover, where one county's spending causally affects a neighbor's, from this kind of shared exposure. At least three stories are consistent with a positive λ, only the first of which is a spillover in the strict sense. Roads, water systems, and markets built in one county spill across the border into the next, so the county that pays does not capture all the benefit (Anselin & Florax, 1995). Governors may watch each other and feel pressure to match visible projects (Besley & Case, 1995), which would show up as correlated spending without any physical spillover. And counties in the same drought belt or economic corridor are simply hit by the same shocks at the same time, which the spatial lag can also pick up even absent any genuine cross-county interaction. Because the residual cross-sectional dependence documented in **Table 8** was not eliminated by any of the seven candidates, including the one built specifically to address it, this paper reports λ as evidence of spatial interdependence in the broad sense, counties' development spending moves together, without asserting that it identifies a specific spillover mechanism or its magnitude; a model ignoring the interdependence entirely, whatever its source, would still get the fiscal multipliers wrong.

4.11.2. The Equitable Share: Strong Pass-Through with System-Wide Reach

Of all the drivers, national transfers do the most work. The direct elasticity of .879 and the total of 1.103 for lNGF say that nearly every extra shilling of equitable share, in constant terms, ends up in the development budget, and the spatial multiplier then spreads part of that effect across the county network. A total elasticity just above one simply means that a 10% increase in the real equitable share pool is associated with system-wide development spending rising by a little more than 10% once the spillovers have run their course. Economists have a name for this pattern: the flypaper effect, the tendency for grant money to stick where it lands (Hines & Thaler, 1995; Inman, 2008). It is reassuring for policy, given the running argument over whether counties should get a bigger slice of national revenue. It also lines up with Frempong's (2026) result that budget composition matters for county economic performance, and it goes a step further by showing where in the fiscal chain the pass-through actually happens, and that this pass-through survives even after converting to constant prices and adding explicit inflation and trend controls.

4.11.3. The Wage Bill: A Structural Drag with Spatial Consequences

The wage-bill result (direct = −.483; total = −.605) is the one with the sharpest policy edge, and it is, if anything, slightly larger in this constant-price specification than in an earlier nominal-only draft. Payroll has eaten a growing share of county budgets since 2013, with the combined wage bill across the 47 counties passing KES 160 billion in nominal 2025 terms (Controller of Budget, 2025), and the model shows that this growth, even after converting to constant prices and controlling separately for the fiscal-year inflation rate and a linear trend, is associated with lower development spending. The logic is not complicated. Once most of a county's exchequer release is already committed to salaries, there is little left for development contracts, and contractors then sit behind the payroll queue waiting to be paid, which slows absorption and feeds the pending-bills pile. There is a spatial twist too: the indirect wage-bill effect of −.123 means a county with a runaway payroll is associated with lower development spending in its neighbors as well, though, as Section 4.11.1 notes, part of any such spillover could also reflect shared regional fiscal pressure rather than a direct county-to-county channel. Smoke (2015) noticed the same dynamic across sub-Saharan Africa, and this is the first time it has been put to numbers at the Kenyan county level.

4.11.4. Development Partner Funds and Pending Bills

Donor grants behave the way you would hope: development partner funds carry a positive direct elasticity of .251 and a total effect of .314, which rules out the worry that counties simply pocket donor money and cut back their own development spending. Instead the grants add to county capital programs. That is a happier result than Mogues and Benin (2012) found in Ghana, where external grants seemed to substitute for local effort, and it probably reflects the more project-tied, performance-conditioned nature of the grants Kenyan counties receive. Pending bills work in the opposite direction (direct = −.073; total = −.092). The elasticity looks small on its own, but the sheer size of the problem makes it bite: unpaid bills across the 47 counties stood at KES 176.8 billion in nominal June 2025 terms, close to 46% of the entire annual equitable share pool (Controller of Budget, 2025).

4.11.5. Own-Source Revenue and Political Coalition

Two variables come up empty, and it is worth being clear about why. Own-source revenue is only marginally significant under maximum likelihood (elasticity = .123, p = .089) and loses significance entirely once HAC-robust errors are applied (p = .407 clustered by county; p = .223 clustered by time). That does not mean own revenue is unimportant to county finances. It most likely means that the county-to-county variation in revenue effort that survives after controlling for the equitable share, the wage bill, and the other fiscal, electoral, and common-time variables no longer predicts development spending on its own, because both revenue and development spending are driven by the same underlying thing, county economic size. Political coalition alignment is flat across every specification (p between .90 and .93, **Table 9**). A few readings make sense. The equitable share runs on a constitutional formula, which leaves little room for playing favorites; a single yes-or-no coalition dummy may miss the shifting politics of three presidential administrations; and even if alignment tilts the composition or timing of discretionary grants, the total development budget can come out unchanged if counties just shuffle money between project lines. These nulls sit against Cheeseman et al.'s (2016) qualitative account of governor-level politics and Prattay's (2024) account of ethnicity-driven coalition bargaining, in which alignment reflects an expectation of future resource distribution rather than a measured transfer premium, an expectation that need not show up as a detectable effect once county fixed effects are in place. Political coalition and the election-year indicator behave similarly to each other in the final model, in a way that is itself informative: neither moves the development budget once the common-time controls and CCE augmentation are in place, though EL remains significant in the plain SAR-FE candidate, a supplementary, non-tabulated check discussed in full in Section 4.11.6.

4.11.6. The Election-Year Effect on Development Expenditure

This paper reads the election-year coefficient as an association that is sensitive to modeling choices, rather than a settled causal electoral-cycle effect, for two reasons. First, EL is identical across all 47 counties within each of the four fiscal years it flags, and, once a fiscal-year inflation rate and a linear time trend are added as explicit common-time controls, EL correlates with the inflation rate at r = .684 (**Table 3**), a very high correlation given that the panel spans only 11 distinct fiscal years. This means EL's coefficient in any specification without full year fixed effects, which every specification in this paper deliberately omits so that EL can be estimated at all, is at risk of capturing whatever else was distinctive about nationwide conditions in 2017, 2018, 2022, and 2023, not necessarily the electoral calendar specifically. Second, this risk shows up directly in the results: in the final model, SAR-FE (CCE), whose cross-sectional-mean augmentation absorbs additional year-to-year common variation on top of INFL and TREND, EL is small and statistically indistinguishable from zero ($\beta = -.035$, a transformed predicted difference of about −KES .091 billion at the panel's mean development expenditure, $p > .45$ under every standard error reported in **Table 9**). In the plain SAR-FE candidate, which satisfies the same diagnostic checks as SAR-FE (CCE) (Section 4.6) and differs from it only in whether that additional cross-sectional-mean augmentation is included, EL remains negative and significant ($\beta = -.135$, $p < .001$, a transformed predicted difference of about −KES .332 billion). A result that appears or disappears depending on this one modeling choice, between two candidates that are otherwise statistically tied, is not a result this paper can responsibly describe as robust. Rather than choose whichever specification produces significance, this paper reports both, following the final model's own selection rule, and treats the election-year coefficient throughout as a conditional association: development expenditure in election-year fiscal years is lower in some specifications and not distinguishable from zero in others, and the difference tracks how much of the common year-to-year variation, price growth, secular budget trends, and other nationwide shocks in those same years, the model is allowed to absorb. Three plausible mechanisms, procurement slowdowns from stretched administrative capacity, statutory restrictions on tendering ahead of a general election, and deliberate reprioritization toward recurrent spending, remain consistent with Mbate's (2021) finding that Kenyan counties reallocate budgets around election timing and with the broader electoral-cycle literature (Bury & Feld, 2025; Lokshin et al., 2024). None of them, however, can be confirmed or ruled out by this panel, which cannot separate an election-specific mechanism from any other nationwide shock concentrated in the same fiscal years without a longer time series or a genuinely exogenous source of timing variation. Establishing the electoral-cycle interpretation on firmer ground would require either a much longer panel, so that year fixed effects become feasible without eliminating all of EL's identifying variation, or a research design that isolates election-year timing from other nationwide shocks directly, for instance by exploiting variation in the number of months a fiscal year overlaps with the pre-election restricted period. Until then, this paper's own results, not just a general methodological caution, are the reason to treat the election-year finding as an open question rather than a settled one.

5. Conclusion and Policy Recommendations

This paper set out to explain what drives development spending across Kenya's 47 counties over eleven fiscal years (2015 to 2025), using a spatial autoregressive panel model with individual county fixed effects and cluster-robust HAC errors, now estimated in constant fiscal-year-2014/15 Kenya shillings with an explicit fiscal-year inflation rate and linear time trend added as common-time controls, and selected from seven candidate specifications on the basis of which ones satisfied the most of five diagnostic assumptions rather than on information criteria alone. Five things stand out. Development spending is spatially interdependent across county lines ($\lambda = .204$, $p < .001$ in the final model), so a panel model that treats counties as independent will misstate the fiscal multipliers, though persistent cross-sectional dependence in the residuals means this parameter should be read as evidence of interdependence broadly rather than a precise estimate of spillovers alone. National transfers are the main driver, with a total elasticity of 1.103 that puts the flypaper effect on display in the Kenyan case. The wage bill is a real drag on development spending (total elasticity = −.605), and that drag reaches into neighboring counties. Development partner funds add to development spending rather than substituting for it (total elasticity = .314), and pending bills add a smaller but genuine drag (−.092); at KES 176.8 billion in nominal June 2025 terms, the outstanding stock is a legacy weight on the whole county development program. The election-year indicator is more fragile: it is negative and significant under a plain fixed-effects specification but not under the final, CCE-augmented specification that additionally absorbs common year-to-year variation, and its own high correlation with the fiscal-year inflation rate (r = .684) means the two specifications cannot be cleanly separated from each other on the evidence in this panel. This paper therefore reports the election-year association, not effect, as unresolved, and treats any policy response to it as a precaution against a plausible but unconfirmed pattern rather than as a response to an established structural feature of county budget execution.

The findings point to a handful of concrete steps. Because almost every extra shilling of equitable share ends up in development spending, delays in releasing that money translate directly into stalled projects, so the National Treasury should move to a statutory monthly disbursement calendar with automatic release triggers and drop the discretionary hold-backs that push counties into deferring contracts and racking up pending bills; the finance officers interviewed by Akhonya et al. (2026) said much the same thing. On the wage bill, the Salaries and Remuneration Commission and the Treasury should set and actually enforce a ceiling on payroll as a share of county revenue, with real consequences for breaching it and workforce-rationalization plans required of counties already over the line. For the pending-bills overhang, the country needs a national settlement framework, with independent verification of what is genuinely owed, ring-fenced sinking funds, and phased five-year clearance schedules, plus a bonus for counties that bring their bills below 15% of revenue. Since money spent in well-connected counties is associated with higher system-wide returns, the Commission on Revenue Allocation could add a spatial connectivity grant that rewards neighboring counties for co-investing in cross-boundary infrastructure. Because donor funds are associated with genuine additions to development spending, the Treasury and development partners should put money into county procurement capacity so that the counties with the weakest absorption can actually spend what they are given. And because development spending may run lower in election-year fiscal years, even though this paper's own evidence for that pattern is mixed across otherwise statistically tied specifications, the Public Procurement Regulatory Authority and the Controller of Budget could still build a light-touch electoral-cycle contingency into the county budget-execution calendar as a precaution, front-loading procurement and contract-award timelines ahead of a general election and ring-fencing already-approved development projects from mid-year electoral-period freezes, without treating the underlying pattern as confirmed.

Author Contributions

Conceptualization, methodology, software and formal analysis, T.J.B.; validation, literature review and introduction C.M. and S.W., Data collection P.A.; writing—original draft preparation, writing review and editing, D.M. All authors have read and agreed to the published version of the manuscript.

Conflicts of Interest

The authors declare no conflicts of interest regarding the publication of this paper.

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