



Evaluating the impact of targeted decentralization on household consumption: Evidence from marginalized and privileged regions in Kenya

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ARTICLE INFO

Keywords:

Decentralization
Sub-national government
Kenya Finaccess household survey
Household consumption
Marginalization

ABSTRACT

This paper evaluates the impact of Kenya's 2010 constitutional reform, which established 47 semi-autonomous counties and implemented a nationwide decentralization framework with redistributive fiscal transfers favoring 14 historically marginalized regions through a revenue-sharing formula and Equalization Fund. While decentralization is widely promoted as a tool for improving citizen welfare, evidence on its distributional consequences—particularly when exposure varies across regions—remains limited. Using household-level data from the 2009–2018 Kenya FinAccess Survey, this study exploits differential exposure to targeted fiscal transfers following the 2013 decentralization rollout to examine effects on six categories of household consumption: mobile airtime, education, household bills, medical expenses, rent, and family transfers. A difference-in-differences framework reveals significant post-reform increases in spending across several categories in more heavily exposed (marginalized) regions relative to less exposed counties (privileged). Quantile regressions show that these gains were unevenly distributed: poorer households experienced larger relative increases in total consumption, while richer households saw stronger growth in education and medical expenditures. Causal mediation analysis indicates that higher county-level budget allocations and household income partially explain these effects. A comprehensive set of robustness checks—including parallel trends verification, entropy balancing, falsification tests, alternative treatment definitions, and sensitivity to inference choices—supports the internal validity of the findings. Overall, the results suggest that targeted fiscal decentralization, when embedded within a nationwide reform, can reduce regional disparities while generating heterogeneous welfare gains across income groups.

1. Introduction

Decentralization reforms have become a central pillar of governance restructuring in many developing countries, motivated by the promise of improved service delivery, reduced regional disparities, and enhanced local accountability. The theoretical rationale for decentralization is grounded in the principles of fiscal federalism and public choice theory, which posit that devolving authority to subnational governments enhances allocative efficiency, accountability, and responsiveness to local preferences (Oates, 1999; Tiebout, 1956). Yet, as Rodden (2004) and Smoke (2015) observe, decentralization is inherently multifaceted and context-dependent reform, and its outcomes often diverge from theoretical expectations.

Empirical evidence on decentralization's effects remains mixed. In Latin America, decentralization has been shown to improve education and health outcomes in contexts with strong local capacity (Faguet and Sánchez, 2008, 2014), while other studies document limited or

regressive effects in countries where local governance institutions remain weak (Cavalieri and Ferrante, 2016; Dwicaksono and Fox, 2018). Comparative analyses attribute these divergent outcomes to variations in institutional quality, administrative capacity, and the design of intergovernmental transfer systems (Bellofatto and Besfamille, 2021; e.g., Di Novi et al., 2019; Ezcurra and Pascual, 2008; Kyriacou et al., 2017; Lessmann, 2009; Sepulveda and Martinez-Vazquez, 2011). A growing literature further emphasizes that the distributive design of fiscal transfers, whether equalizing or politically motivated, plays a crucial role in determining who benefits from decentralization (Alderman, 2002; Banful, 2011; Liu et al., 2017; Tohari et al., 2019).

Beyond aggregate outcomes, targeted decentralization, where fiscal transfers prioritize historically disadvantaged regions, has emerged as a mechanism for spatial equity. Empirical evidence from diverse contexts demonstrates its potential to improve welfare when effectively designed and implemented. For instance, in China, targeted poverty alleviation and spatially differentiated fiscal transfers have reduced regional

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<https://doi.org/10.1016/j.pirs.2026.100140>

Received 1 July 2025; Received in revised form 14 January 2026; Accepted 20 February 2026

Available online 21 February 2026

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inequality and improved rural welfare (Guo et al., 2022; Liu et al., 2017). Similarly, geographic and formula-based targeting schemes in Latin America have enhanced poverty reduction efficiency (Baker and Grosh, 1994), while local discretion in Albania enabled better targeting of assistance to the poor (Alderman, 2002). However, the success of such approaches depends heavily on administrative capacity and political incentives: formula-based systems may still be manipulated for electoral purposes, as found in Ghana's intergovernmental transfers (Banful, 2011). These studies collectively highlight that redistributive decentralization can reduce regional disparities when allocation rules are transparent and linked to objective needs indicators.

Despite this growing literature, micro-level evidence on how targeted decentralization affects household welfare, particularly consumption, is limited. Most studies focus on service delivery, local

governance, or aggregate fiscal outcomes, overlooking how fiscal transfers translate into household-level spending behavior. Moreover, evidence from Sub-Saharan Africa remains sparse, even though the region has undergone some of the world's most ambitious decentralization reforms. This paper fills this gap by examining Kenya's 2013 decentralization reform as a case of constitutionally mandated, equity-based fiscal devolution, assessing its effects on household consumption across six expenditure categories.

Kenya's 2010 Constitution introduced a radical restructuring of the state, establishing 47 semi-autonomous county governments and mandating a revenue-sharing system guided by equity principles. A key component of this framework was the Equalization Fund, designed to channel additional resources to 14 counties officially designated as "marginalized" by the Commission on Revenue Allocation (CRA), as

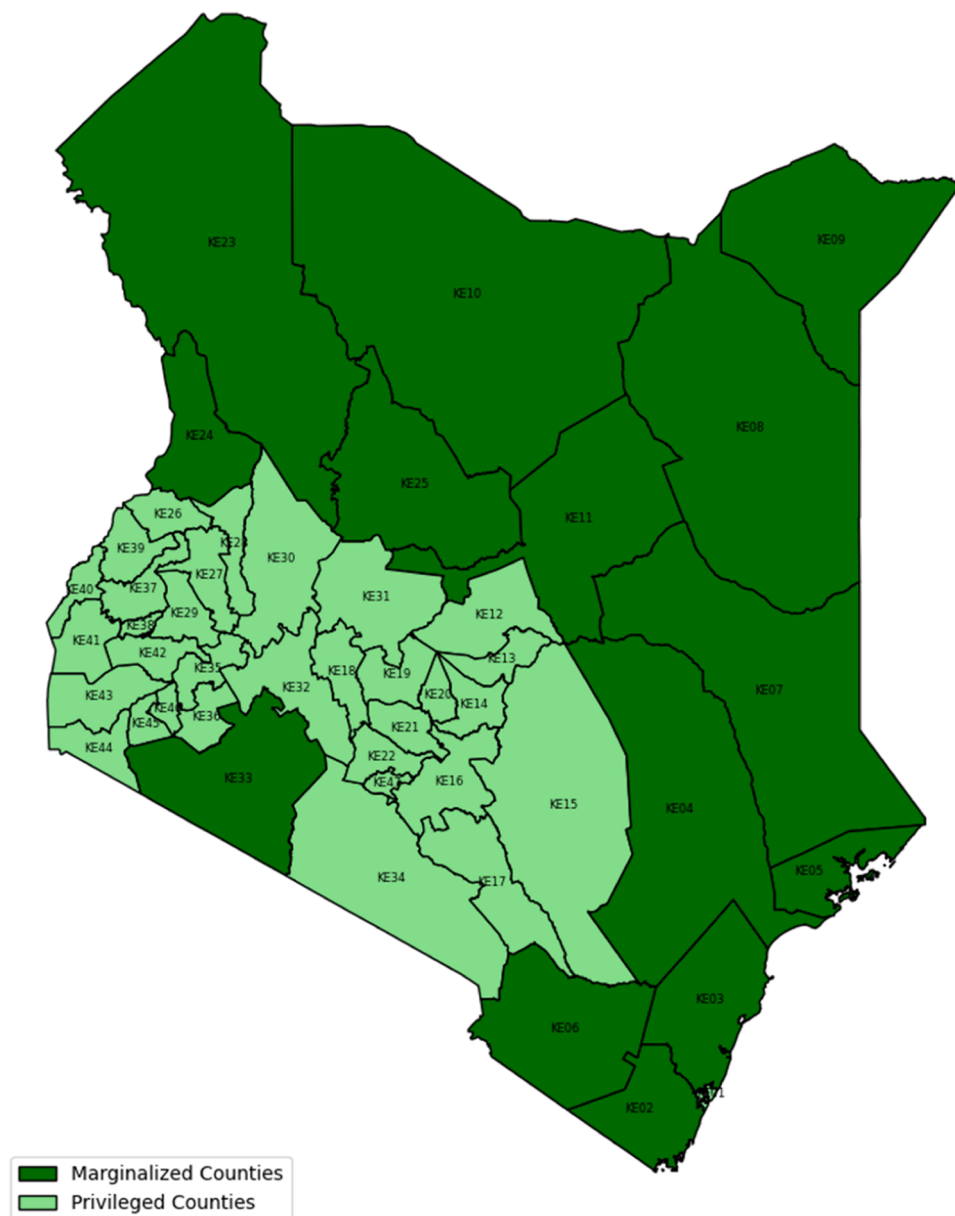


Fig. 1. Kenya counties classified by marginalization, Note: The figure displays counties of Kenya classification by marginalization, identified by their official codes, include KE01 (Mombasa), KE02 (Kwale), KE03 (Kilifi), KE04 (Tana River), KE05 (Lamu), KE06 (Taita Taveta), KE07 (Garissa), KE08 (Wajir), KE09 (Mandera), KE10 (Marsabit), KE11 (Isiolo), KE12 (Meru), KE13 (Tharaka-Nithi), KE14 (Embu), KE15 (Kitui), KE16 (Machakos), KE17 (Makueni), KE18 (Nyandarua), KE19 (Nyeri), KE20 (Kirinyaga), KE21 (Murang'a), KE22 (Kiambu), KE23 (Turkana), KE24 (West Pokot), KE25 (Samburu), KE26 (Trans Nzoia), KE27 (Uasin Gishu), KE28 (Elgeyo-Marakwet), KE29 (Nandi), KE30 (Baringo), KE31 (Laikipia), KE32 (Nakuru), KE33 (Narok), KE34 (Kajiado), KE35 (Kericho), KE36 (Bomet), KE37 (Kakamega), KE38 (Vihiga), KE39 (Bungoma), KE40 (Busia), KE41 (Siaya), KE42 (Kisumu), KE43 (Homa Bay), KE44 (Migori), KE45 (Kisii), KE46 (Nyamira), and KE47 (Nairobi).

shown in Fig. 1. The CRA's formula incorporates population, land area, poverty incidence, and income distance to ensure that marginalized counties (MCs), mostly arid, sparsely populated, and historically neglected, receive proportionally higher per capita transfers than privileged counties (PCs). This structure embeds a redistributive logic directly into Kenya's fiscal architecture, creating a quasi-natural experiment to study the welfare impacts of targeted decentralization.

The causal pathway linking decentralization to household consumption operates through multiple, interrelated mechanisms. Increased fiscal transfers expand local government spending on public employment, service delivery, and infrastructure, generating both income and substitution effects. Employment creation in county administrations and devolved sectors such as health and education raises household earnings (Alesina et al., 2000; Mattos and França, 2011), while improved service delivery reduces out-of-pocket expenditures on private education and healthcare (Bjørnskov et al., 2008; Faguet and Sánchez, 2014). Counties also channel resources toward infrastructure and local enterprise development, through mechanisms such as business funds and agricultural programs, stimulating local economic activity (Kis-Katos and Sjahrir, 2017; Medeiros et al., 2021). Together, as discussed in detail later, these channels create a multiplier effect, enhancing disposable income and consumption, particularly in regions previously constrained by underinvestment and poor service access.

The study exploits this institutional setting to identify the causal impact of decentralization on household consumption. Using data from the 2009–2018 Kenya FinAccess Surveys, which include two pre-reform (2009, 2013) and two post-reform (2015, 2018) waves, I estimate a difference-in-differences (DiD) model comparing outcomes between treated counties (the 14 CRA-designated marginalized counties) and control counties (the remaining 33). The analysis focuses on six categories of household expenditure: education, medical, household bills, rent, mobile airtime, and family transfers. This design leverages both temporal and spatial variation in exposure to increased fiscal transfers, controlling for county- and year-specific effects. Exploiting differential exposure to constitutionally mandated, redistributive fiscal transfers within Kenya's nationwide decentralization reform, this paper estimates how targeted increases in county-level resources affected household consumption patterns.

The results show that Kenya's decentralization reform significantly increased household spending on education, medical, rent, and household bills, with smaller but positive effects on mobile airtime and family transfers. Quantile regressions reveal that poorer households experienced larger relative gains in total consumption, while higher-income households benefited more from increased education and medical spending. Mediation analysis confirms that these effects were partly driven by increased county allocations and household income growth. A battery of robustness tests, including pre-trend verification, entropy balancing, falsification exercises, alternative inference procedures, redefinitions of treatment and outcome variables, and heterogeneous and spillover effects, corroborate the stability and internal validity of these findings.

This paper makes three primary contributions. First, it provides novel micro-level evidence linking targeted fiscal decentralization to household consumption, an underexplored but policy-relevant dimension of welfare. Second, it offers a spatially disaggregated assessment distinguishing historically marginalized and privileged regions, thereby illuminating the redistributive effectiveness of Kenya's equity-oriented decentralization framework. Third, it extends the literature on poverty-targeted and formula-based transfers (Alderman, 2002; Baker and Grosh, 1994; Banful, 2011; Guo et al., 2022; Liu et al., 2017) by demonstrating how a constitutionally embedded equalization mechanism can generate tangible welfare gains under credible institutional arrangements.

Together, these findings contribute to debates in regional and development economics on whether decentralization can serve as an effective instrument for inclusive and spatially balanced growth. The

evidence suggests that Kenya's targeted fiscal reform improved household welfare in marginalized regions, though its benefits varied across expenditure types.

The remainder of the paper is organized as follows. Section 2 outlines Kenya's decentralization framework; Section 4 reviews the theoretical and empirical literature on the welfare effects of decentralization; Section 4 describes the data and empirical strategy; Section 5 presents the results and robustness analysis; and Section 6 concludes with policy implications.

2. The Kenyan context: constitutional reforms and the structure of devolution

Kenya's 2010 Constitution marked a turning point in governance, adopted in the aftermath of the 2007/08 post-election crisis that exposed deep-seated grievances over political exclusion, regional inequality, and historical marginalization (Kenya Law Report, 2019). The new constitutional order sought to promote equity, improve public service delivery, and empower historically disadvantaged communities through a devolved system of government.

Kenya operates a unitary but devolved governance structure comprising two levels: the national government and 47 semi-autonomous county governments (D'Arcy, 2020). Unlike classical federations such as the United States or Germany (Rodden, 2004; Voigt and Blume, 2012), Kenya's counties are not sovereign entities but constitutionally recognized subnational units responsible for local service delivery in health, agriculture, infrastructure, and local administration. The national government retains authority over core functions such as defense, foreign policy, and monetary affairs. This hybrid structure blends fiscal federalism principles with centralized coordination under the doctrine of cooperative government (Article 6(2), Constitution of Kenya).

The 47 counties became operational after the March 2013 general elections, based on the 1992 district boundaries to prevent political gerrymandering and preserve administrative continuity. Prior to devolution, Kenya had undergone several rounds of administrative restructuring (Smoke, 1993), as shown in Fig. 2. In 1986 there were 41 districts, including the capital city, Nairobi. By 1999, this number had increased to 70, and it further rose to 254 by 2009, largely through executive decrees. The 2010 Constitution reversed this fragmentation by consolidating the districts into 47 constitutionally recognized counties, each with its own elected governor and county assembly. This consolidation was guided by the First Schedule of the Constitution, which formalized the county boundaries. The Independent Electoral and Boundaries Commission (IEBC) was tasked with delineating these boundaries based on historical districts, population size, and geographic considerations.

The governance system was highly centralized: districts operated under the Provincial Administration, and service delivery was handled by 175 local authorities—municipal, town, and county councils—under the Ministry of Local Government. By the time of the reform, there were 175 local authorities: 67 county councils, 43 municipal councils, 62 town councils, and 3 city councils (Nairobi, Mombasa, and Kisumu). While districts coordinated national government functions such as security and development planning, local authorities handled service delivery in areas like water, sanitation, local roads, and markets. Fiscal autonomy was limited: local authorities relied heavily on transfers such as the Local Authority Transfer Fund (LATF) and road maintenance levies, while development priorities were coordinated through District Development Committees chaired by District Commissioners. This arrangement created overlapping mandates, weak accountability, and inefficiencies in service delivery (Smoke, 1993), issues the 2010 Constitution sought to resolve by abolishing local authorities and replacing them with county governments endowed with clear constitutional powers and guaranteed revenue shares.

A cornerstone of this decentralization reform was to improve the efficiency and equity of public service delivery, redistribute national

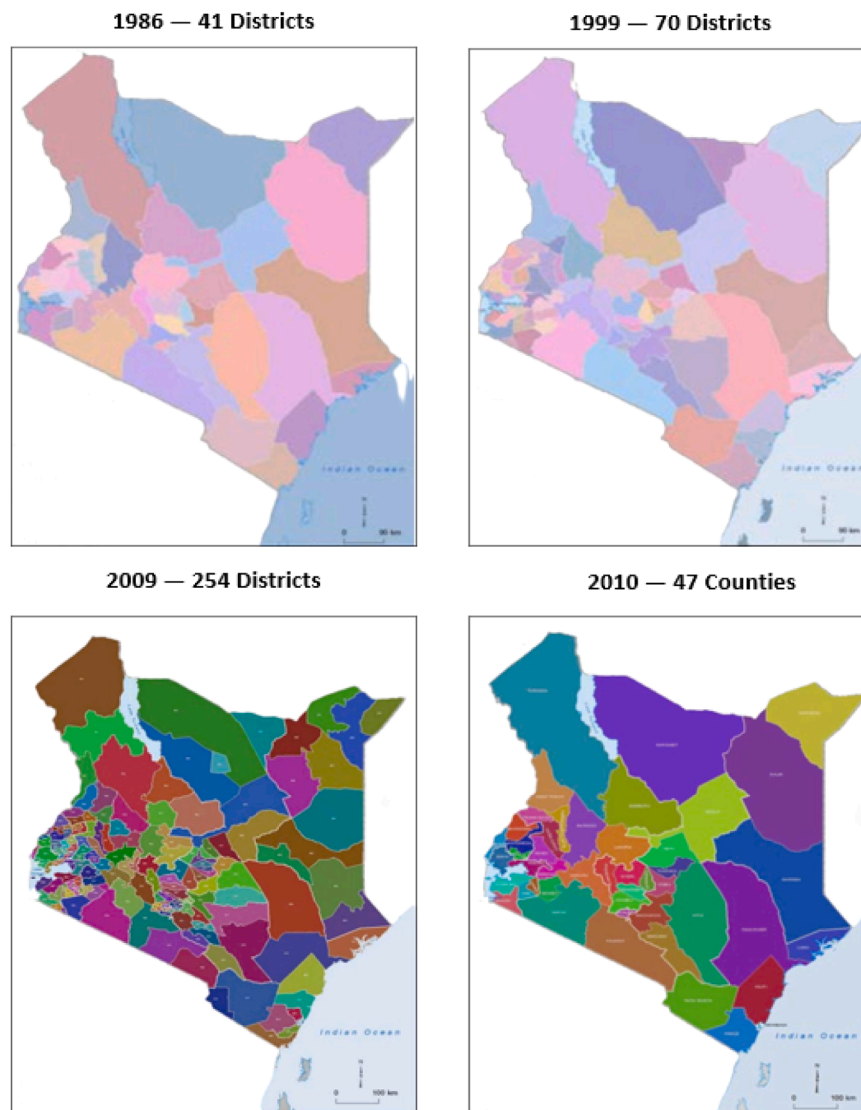


Fig. 2. Evolution of Kenya districts between 1963 and 2010, Note: The figure shows the evolution of Kenya's (adapted from Kenya National Bureau of Statistics statistical reports).

resources more fairly, and redress the marginalization of various groups and communities. The constitution defines a marginalized group as one that has been disadvantaged by past or present laws or practices based on one or more grounds outlined in Article 27(4), such as ethnicity, geographic location, or socio-economic status (Kenya Law Report, 2019, p. 163). A marginalized community is described as one that, due to its small population size or other factors, has been unable to fully participate in Kenya's integrated social and economic life (Kenya Law Report, 2019, p. 162–3). These include traditional and indigenous communities, as well as pastoralist groups, many of whom reside in regions historically underserved by the state.

To operationalize this constitutional mandate, the CRA, through its inaugural policy, identified the fourteen marginalized counties shown in Fig. 1 using three criteria: (i) a County Development Index capturing disparities in health, education, and infrastructure; (ii) evidence of historical injustices such as land dispossession or legal exclusion; and (iii) citizen perceptions of marginalization (CRA, 2014). Counties meeting at least two criteria qualified as “marginalized,” most of them being arid, sparsely populated, and pastoralist regions historically underserved by the state. This process aimed to ensure that the Equalization Fund mandated by Article 204 would raise basic service levels, such as water, roads, health facilities, and electricity, in these counties to

those generally enjoyed by the rest of the country, thereby promoting equity and addressing long-standing regional disparities (Kenya Law Report, 2019, p.123–4).

In addition to the Equalization Fund, the CRA designed a revenue-sharing formula (Articles 203, 216, 217), reviewed every five years by the Senate. Under the current framework, known as the Fourth Basis for Revenue Sharing, allocations are determined using five weighted criteria: population (42 %), basic equal share (22 %), poverty index (14 %), land area (9 %), and income distance index (13 %). This combination balances service delivery needs with redistributive goals. The income distance index measures the gap between a county's per capita income and the national average, ensuring poorer counties receive proportionally more funding. The basic equal share guarantees a minimum allocation to every county, while the poverty index and land area account for structural disadvantages in service provision. The formula thus embeds affirmative action within Kenya's intergovernmental fiscal framework.

Although neither the Constitution nor CRA policy documents articulate an explicit *theory of change*, the implied causal pathway can be summarized as: constitutional reform → increased fiscal transfers to marginalized counties (Equalization Fund and equitable share) → higher county-level spending on services and employment → improved

household welfare. The concept of a *theory of change* is widely used in development economics to unpack causal pathways and to evaluate complex, multi-layered policy interventions, particularly in institutional and governance reforms (see, e.g., Bunte et al., 2018; Hout et al., 2022; Hughes et al., 2020; McCarthy and Krause, 2024). In this paper, the term *theory of change* is used descriptively to denote this policy-implied causal sequence, which guides the empirical analysis rather than constituting a formal behavioral model.

To evaluate this link, I compiled and analyzed data from multiple sources: pre-reform LATF allocations from the Kenya Gazette (2005/06–2012/13) (Kenya Gazette, 2012), post-reform county budgets and actual spending from the Office of the Controller of Budget (2013/14–2023/24) (Office of the Controller of Budget, 2023), and population data from the 2009 and 2019 censuses (Kenya National Bureau of Statistics, 2010, 2019). First, budget data reveal a sharp and sustained increase in resources directed to marginalized counties. As shown in Fig. 3, both total and per capita allocations show a clear discontinuity at the reform's onset, with MCs surpassing PCs in per capita terms, reflecting the operationalization of constitutional provisions favoring equity. Importantly, this reallocation occurred alongside substantial national budget growth—from KSh 1.58 trillion in FY 2014/15 to KSh 3.7 trillion in FY 2023/24—while county allocations nearly doubled (KSh 226.7 billion to KSh 442.1 billion) and Equalization Fund rose more than threefold (KSh 3.4 billion to KSh 10.9 billion) (Kenya National Treasury, 2014, 2023). These increases were additive, not redistributive: marginalized counties gained substantially without reducing allocations to others.

Second, actual spending data from the Office of the Controller of Budget (2023) confirm that counties converted these transfers into real fiscal activity (see Fig. 4). On a per capita basis, marginalized counties spent approximately 2.5 times more than privileged counties across personnel emoluments, operations and maintenance, and development expenditures. Development spending signals long-term investments in infrastructure and public goods, while personnel emoluments and operations and maintenance reflect expanded service delivery capacity. Finally, evidence from the 2009 and 2019 population census data suggests that increased fiscal transfers had tangible labor market effects: marginalized counties experienced a 3.1-fold increase in per capita paid employment, compared to 1.5-fold in privileged counties (See Table A1 in the Supplementary Information). These patterns support the interpretation that enhanced fiscal transfers stimulated local economic activity and expanded public employment—key preconditions for household welfare gains.

Taken together, these findings establish a strong first-stage causal link: Kenya's decentralization reform substantially increased fiscal transfers to marginalized counties, which translated into higher spending and expanded employment. While Kenya's reforms share general principles of fiscal decentralization with other Sub-Saharan African countries, comparative experiences suggest that institutional design shapes outcomes. For example, Uganda's multi-tier decentralization produced mixed results due to recentralized control, funding shortfalls, and administrative capacity constraints (Green, 2015), while Ghana's District Assemblies' Common Fund illustrates how opaque formulas and weak local revenue incentives can limit the effectiveness of transfers (Aye, 1995). These cases highlight that Kenya's combination of a clear Equalization Fund, transparent revenue-sharing formulas, and targeted support for historically marginalized counties may have facilitated more pronounced fiscal and welfare effects.

3. Why decentralization could stimulate household consumption in marginalized regions

Kenya's constitutional reform institutionalized fiscal transfers to historically marginalized counties to redress spatial inequalities in service delivery and opportunity. The underlying theory of change posits a causal sequence: (i) increased fiscal transfers to local governments → (ii)

expanded spending on infrastructure, services, and employment → (iii) improved access to public goods and income opportunities → (iv) enhanced household welfare → (v) increased household consumption. In this paper, the term *theory of change* is used in a pragmatic sense to denote this structured causal framework, which serves as an organizing narrative for identifying and empirically examining the mechanisms linking decentralization to household-level outcomes, rather than as a formal theoretical model. This logic operates through direct and indirect transmission channels. Direct channels are defined as mechanisms through which decentralization affects households without market intermediation, operating primarily via public-sector actions that directly alter household resources or reduce out-of-pocket costs (e.g., public employment and improved service delivery). Indirect channels, by contrast, operate through market intermediation, including infrastructure investment, private-sector employment responses, and expanded financial inclusion that stimulate local economic activity. Empirical evidence on the relative importance of these channels, particularly at the household level in marginalized regions, remains fragmented. This section therefore reviews four key mechanisms through which decentralization may influence household consumption in Kenya's marginalized regions.

3.1. Public employment as a redistributive channel

Public employment is a primary transmission mechanism through which decentralization can affect household welfare. Governments often use public sector hiring as a redistributive tool to channel income toward poorer or politically disadvantaged areas (e.g. Alesina et al., 2000; Marqués and Rosselló, 2004; Mattos and França, 2011). Fiscal decentralization amplifies this effect by enabling subnational governments to recruit locally for administrative and service delivery roles (Martinez-Vazquez and Yao, 2009).

Evidence from Europe and Latin America suggests such expansions can yield positive local employment multipliers as higher public wages stimulate demand in non-tradable sectors (Becker et al., 2021; Jofre-Monseny et al., 2020; Roupakias, 2022). However, these effects depend on context: while Jofre-Monseny et al. (2020) find crowding-in effects in Spain, Becker et al. (2021) report wage inflation and crowding-out in Germany. In Sub-Saharan Africa, where informal employment dominates, public hiring may substitute for rather than complement private activity (Jaimovich and Rud, 2014; Simson, 2019). Kenya's post-reform experience—marked by rapid growth in county-level employment in marginalized counties—suggests decentralization expanded local labor markets. Yet whether these jobs translate into sustained welfare and consumption gains remains an open empirical question, underscoring the need for micro-level analysis linking fiscal transfers, employment creation, and household spending.

3.2. Infrastructure investment as a productivity driver

A second mechanism through which decentralization may influence welfare is infrastructure development. By bringing decision-making closer to citizens, decentralization is expected to align public investment with local priorities, thereby enhancing productivity and welfare (Alegre, 2010; Aray, 2019; Kalyvitis and Vella, 2011, 2015; Kis-Katos and Sjahrir, 2017; Lenz et al., 2017; Medeiros et al., 2021). Empirical evidence shows that local infrastructure, such as roads, electrification, and irrigation, can raise income and consumption (Aggarwal, 2018; Charlery et al., 2016; Gehrke and Hartwig, 2018; Saing, 2018). However, outcomes vary widely. Implementation challenges such as political capture, maintenance neglect, and capacity constraints often limit impact (Bock and Blemings, 2024). Moreover, most studies focus on intermediate outcomes like income or employment rather than household consumption, leaving a gap in direct welfare evidence.

Findings from Sub-Saharan Africa are mixed. Public works in Malawi yielded limited welfare effects (Beegle et al., 2017), whereas Ethiopia's

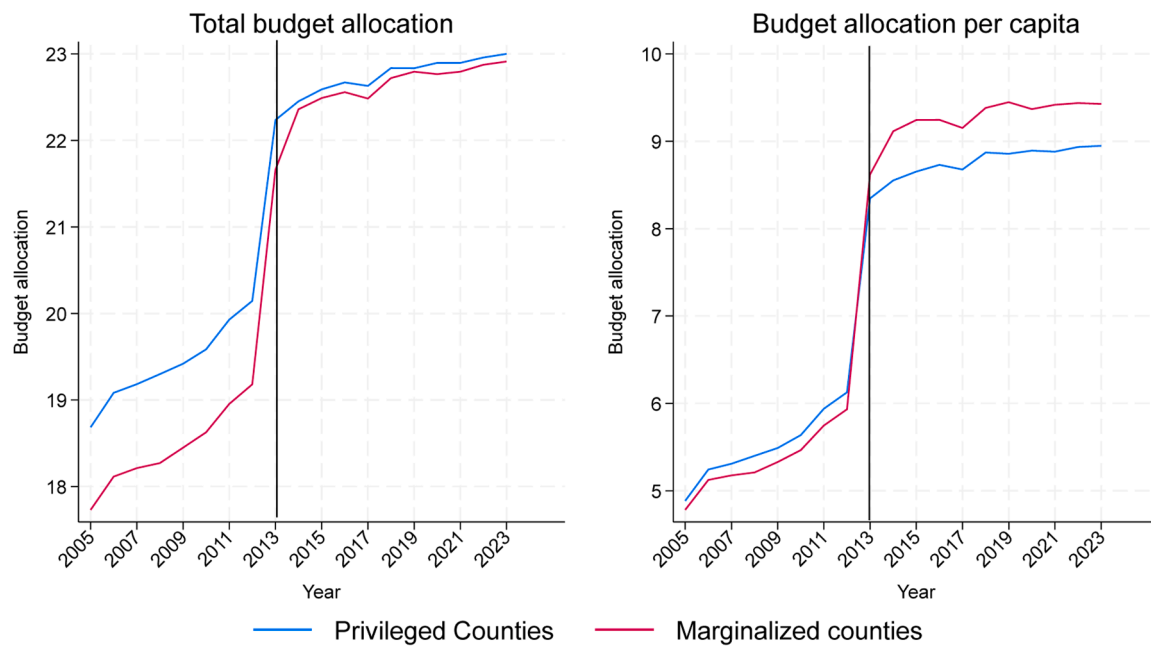


Fig. 3. Evolution of budget allocation between 2005 and 2023, Note: The figure displays the evolution of the total budget allocation between Privileged Counties (PCs) and Marginalized Counties (MCs). The black vertical line indicates the reform date – the period before the reform, 2005 – 2012, and afterward, 2013–2023. The blue-shaded line is for PCs while the red-shaded line is for MCs.

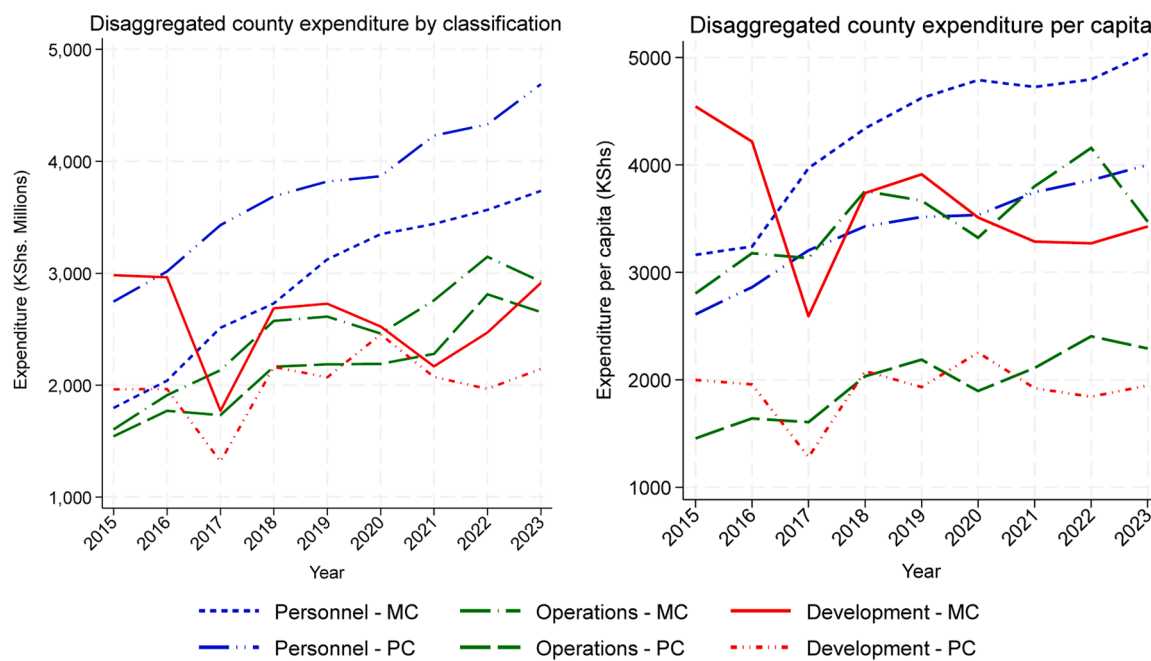


Fig. 4. Disaggregated county expenditures by marginalization status, Note: This figure illustrates disaggregated county expenditures in Kenya from 2015 to 2023, comparing marginalized counties (MC) and privileged counties (PC) across three key expenditure categories: personnel emoluments, operations and maintenance, and development expenditure. The left panel presents total expenditures in Kenyan Shillings (KShs) millions, while the right panel shows per capita expenditures. Line styles and colours differentiate between county types and expenditure categories, as detailed in the legend. The data is sourced from the County Governments Budget Implementation Review Report produced by the Office of the Controller of Budget.

Productive Safety Net Program showed substantial gains (Hoddinott et al., 2012). Similarly, Teklu and Asefa (1999) report that labor-intensive public works in rural Botswana and Kenya improved income for the poorest participants but suffered from weak targeting and limited sustainability. Gehrke and Hartwig (2018) offer a broader conceptual critique of public works programs, arguing that productive effects depend on program reliability, duration, and whether

infrastructure generated has substantial growth impacts. Importantly, they highlight the paucity of evidence on whether infrastructure built under such schemes delivers long-term welfare improvements.

These differences underscore the role of institutional capacity and program design. Pal and Wahhaj (2017) find that stronger governance structures in Indonesia promote socially oriented investment, while China's Province-Managing-County reform boosted agricultural

productivity through targeted transfers (Ma et al., 2025). Such findings reinforce the importance of earmarked transfers, a principle embedded in Kenya's Equalization Fund. Since 2013, marginalized counties have allocated substantial resources to capital development and operations and maintenance for roads, irrigation, and health facilities, addressing historical deficits linked to ethnic favoritism and geographic isolation (Burgess et al., 2015; Harris and Posner, 2019). While these investments plausibly reduce transaction costs and improve access to services, rigorous evidence on whether they translate into higher household consumption remains scarce.

3.3. Public services, wellbeing, and household spending

Decentralization also aims to improve public service delivery, enhancing welfare both materially and subjectively. Fiscal transfers to subnational units can reduce reliance on costly private services, thereby freeing household resources for other consumption needs. However, empirical evidence on these effects is also inconclusive. Faguet and Sánchez (2014) and Khaleghian (2004) find that decentralization improved access to education and healthcare, while Dwicaksono and Fox (2018) highlight uneven results tied to institutional capacity. Galiani et al. (2008) and Bold et al. (2015) show that education decentralization can deepen inequalities when affluent households opt out of public systems, as seen after Kenya's 2003 abolition of primary school fees (Bold et al., 2015).

In Kenya, marginalized counties have used devolved resources to substantially expand health and education infrastructure, potentially lowering out-of-pocket costs. Increased funding for healthcare has enabled county governments to construct clinics and upgrade facilities, reducing reliance on costly private services. Similarly, investments in public education, such as classroom construction, school meal programs, and bursaries, have eased household education costs and improved retention rates. For example, Turkana County has deployed devolved funds to build new health centers and equip them with medical personnel and supplies, reducing the need for residents to travel long distances or pay for private care (see, e.g., Turkana County Government, 2018, 2023). These interventions suggest that decentralization can indirectly influence household spending patterns by lowering the financial burden of essential services. However, most existing studies focus on aggregate enrollment or health outcomes rather than household expenditure, leaving open questions about the extent to which improved public services translate into measurable changes in consumption behavior.

Decentralization may also influence wellbeing through non-material channels. Cross-country evidence shows that both fiscal and political decentralization are associated with higher life satisfaction and trust in government (Bjørnskov et al., 2008; Diaz-Serrano and Rodríguez-Pose, 2012; Flavin et al., 2014; Gao et al., 2014). These studies suggest that fiscal decentralization not only improves service delivery but also indirectly raises life satisfaction by enabling households to redirect spending toward preferred goods and experiences. Nonetheless, as Rodríguez-Pose and Ezcurra (2010) caution, decentralization can exacerbate spatial inequality when poorer regions lack administrative capacity to effectively manage devolved funds.

Beyond social sectors, decentralization can enhance the local business environment by supporting local enterprise development, strengthening household income stability and diversification (e.g. Canavire-Bacarreza et al., 2020; Kyriacou et al., 2015; Sobel et al., 2013). Marginalized counties have used devolved funds to improve irrigation systems, provide farm inputs, and deliver training programs tailored to local needs, stimulating income-generating activities that indirectly raise consumption. Given the strong link between consumption and subjective well-being, these economic spillovers represent an additional channel through which decentralization can improve life satisfaction.

3.4. Financial inclusion and digital access

Financial inclusion represents a key channel through which decentralization can enhance household welfare, particularly in settings where formal banking access is limited. By expanding liquidity and facilitating local market transactions, improved financial access enables households to smooth consumption, invest in education and health, and respond more resiliently to shocks (Abiona and Koppensteiner, 2022; Munyegera and Matsumoto, 2016; Suri et al., 2021).

Evidence from East Africa underscores these effects. In Tanzania, the rapid expansion of mobile money networks allowed households to maintain consumption and human capital investment during rainfall shocks, demonstrating that digital finance can serve as an informal insurance mechanism (Abiona and Koppensteiner, 2022). In Kenya, Suri and Jack (2016) find that the spread of M-Pesa lifted roughly 2 % of households out of poverty and increased female financial independence, underscoring the long-run welfare implications of digital financial access. Complementary evidence from firm-level data shows that mobile money adoption by SMEs in Kenya, Uganda, and Tanzania significantly increased fixed-asset investment, largely through improved liquidity and creditworthiness (Islam et al., 2018).

However, the diffusion of financial inclusion remains spatially unequal. Johnen et al. (2025) show that digital and formal financial access in Kenya is concentrated in urban and wealthier areas, reflecting persistent structural inequalities. Determinants such as education, income, and mobile ownership remain decisive (Balliester Reis, 2022; Demirgüç-Kunt and Singer, 2017). Moreover, institutional quality mediates these effects—provinces with stronger governance achieve greater access to credit and savings services (Tran and Dinh, 2021).

Despite these insights, most studies treat financial inclusion as an independent phenomenon rather than examining its interaction with decentralization. Kenya's devolution may indirectly promote inclusion by increasing local liquidity and stimulating economic activity, but empirical evidence on this link is scarce. This paper addresses this gap by exploring whether fiscal transfers and local development under decentralization amplify financial inclusion and, in turn, household consumption.

3.5. Summary

In sum, decentralization can influence household consumption through multiple, interlinked channels—public employment, infrastructure, service delivery, and financial inclusion. Kenya's equity-based reform explicitly sought to activate these mechanisms to redress regional disparities. However, cross-country evidence reveals that outcomes depend heavily on local institutional quality, governance capacity, and spending composition. The next section outlines the empirical strategy used to test these theoretical linkages using household-level data from the Kenya FinAccess Surveys (2009–2018).

4. Materials and methods

4.1. Data and descriptive statistics

This study utilizes data from the Kenya FinAccess Household Survey (FHS), a nationally representative dataset collected triennially across all 47 counties (FinAccess, 2024). The survey captures detailed information on household consumption, financial behavior, income, and demographic characteristics. Seven waves of the survey were conducted between 2006 and 2024. However, the analysis includes only the 2009, 2013, 2015, and 2018 waves. The 2006 wave is excluded due to the absence of monetary values for consumption categories, and the 2021 wave is excluded because it reports only indicator-based data for four expenditure categories—food, airtime, transport, and rent—without monetary values, limiting comparability. Although the 2024 wave restores broader consumption indicators, it is also excluded to avoid

discontinuity in the time series caused by the unusable 2021 data.

The dependent variables include the natural logarithm of total household consumption and six disaggregated expenditure categories: mobile airtime, household bills, education, medical, rent, and transfers to family members. “Household bills” refer to regular payments for utilities such as electricity, water, and cooking fuel, while “transfers to family members” capture financial support sent to relatives outside the household, including remittances and informal assistance. Following Kai-yuen (1998), consumption is used as a proxy for household welfare, as it reflects both current income and the ability to smooth consumption through savings or borrowing.

The treatment group comprises households in the 14 counties officially designated as marginalized by the CRA, while the control group includes households in the remaining 33 counties. The key treatment variable is an interaction between a marginalization dummy (1 for marginalized counties, 0 otherwise) and a reform dummy (1 for post-reform years—2015 and 2018; 0 for pre-reform years—2009 and 2013). Although the Constitution was enacted in 2010, the decentralization reform was implemented following the March 2013 general elections, with county governments and budget allocations taking effect in the 2013/2014 fiscal year. The FinAccess 2013 survey was conducted between October 2012 and February 2013, prior to the elections and the rollout of county governments. Accordingly, and in line with the FinAccess survey’s naming convention, the 2013 wave is classified as part of the pre-reform period because data collection occurred before the elections and the operationalization of county governments.

The model includes a set of household-level covariates commonly used in the literature (e.g. Changwony et al., 2021; French and McKillop, 2016; Kramer, 2012; Stolper, 2018; Van Rooij et al., 2011; Von Gauder, 2015), including household size; financial literacy categorical variable derived from a standard interest rate question; access to financial advice (formal or informal); gender; age; and education qualifications.¹

Table 1 presents descriptive statistics for household consumption and expenditure patterns, disaggregated by county type (privileged vs. marginalized) and reform period (pre- vs. post-2013 decentralization). The table is organized into two panels. Panel A compares mean values across county types before and after the reform, while Panel B reports within-group changes over time, highlighting mean differences between the pre-reform and post-reform periods for each group.

Panel A of Table 1 highlights clear pre-reform disparities in household consumption between privileged and marginalized counties. Before 2013, households in privileged regions spent significantly more across most expenditure categories—particularly on education, rent, and family transfers—reflecting their higher income levels and better access to goods and services. In contrast, households in marginalized counties allocated a larger share of spending to medical expenses, consistent with limited access to public healthcare and greater reliance on out-of-pocket payments. These differences were statistically significant in nearly all categories, underscoring the structural inequalities that the decentralization reform sought to address. Following the reform, the consumption gap between the two groups narrows considerably. Differences in total consumption and education spending become statistically insignificant, suggesting convergence in living standards. Notably, marginalized counties record higher mean medical and household bill expenditures, indicating expanded access to services but also potential cost pressures linked to improved utilization.

Panel B reveals strong within-group dynamics. In marginalized counties, total household consumption more than doubles after the reform, driven by large and significant increases in education, medical, and communication (airtime) expenditures. These trends point to rising

welfare and improved service access in regions historically excluded from public investment. By contrast, privileged counties exhibit smaller or mixed changes, with modest gains in education and communication spending but little movement in total consumption. Overall, the observed mean differences are indicative of a redistributive effect of Kenya’s decentralization, suggesting that fiscal transfers and devolved spending disproportionately benefited households in marginalized counties—consistent with the hypothesis that targeted devolution can promote welfare gains in economically lagging regions.

Additional socioeconomic characteristics reported in Table A2 of the [Supplementary Information](#) reveal pronounced structural disadvantages in marginalized counties. Households in these regions exhibit significantly lower income, financial inclusion, financial literacy, and educational attainment. For instance, only 5 % of individuals in marginalized counties have tertiary education compared to 11 % in privileged ones, and formal employment is markedly less common (10 % vs. 17 %). Instead, households in marginalized areas rely more heavily on farming and informal support networks, reflecting limited access to wage employment and credit markets. Financial inclusion indicators further illustrate development gaps. Households in MCs score significantly lower on the financial inclusion index (−0.24 vs. 0.05) and are less likely to rely on formal or semi-formal financial advice (11 % vs. 21 %). Instead, they depend more on personal or family-based decision-making, reflecting limited institutional reach. Differences in financial literacy perceptions mirror this pattern: only 26 % of MC respondents “agree” that they are financially literate, compared to 42 % in PCs.

These patterns further illustrate the multidimensional nature of regional inequality—spanning income, education, and financial infrastructure—and underscore why the constitutional design of Kenya’s decentralization explicitly prioritized equity-based transfers. The descriptive statistics thus establish a clear pre-reform disparity baseline, against which the subsequent analysis evaluates whether fiscal devolution helped to narrow these gaps, particularly in education and healthcare-related consumption.

4.2. Estimation strategy

To estimate the causal impact of Kenya’s 2013 decentralization reform on household consumption, I employ a difference-in-differences (DiD) estimation strategy that exploits variation in exposure to targeted fiscal transfers within a nationwide decentralization reform. While decentralization was implemented uniformly across all counties, historically marginalized counties received disproportionately larger fiscal transfers through the CRA allocation formula and the Equalization Fund. The empirical strategy therefore compares changes in household consumption between marginalized (high-exposure) and privileged (lower-exposure) counties before and after the reform, isolating the differential effect of targeted redistribution embedded in decentralization, rather than the effect of decentralization per se.

The baseline model is specified as follows

$$\log(C_{hct}) = \alpha + \beta \cdot \text{Policy}_{ct} + \lambda \cdot X_{hct} + \mu_c + \delta_t + \varepsilon_{hct} \quad (1)$$

Where: $\log(C_{hct})$ is the natural logarithm of household consumption for household h in sub-national region c at time t . This includes total consumption and six disaggregated categories: mobile airtime, education, household bills, medical, rent, and family transfers spending. Policy_{ct} is the treatment variable, defined as the interaction between a marginalization dummy (1 for marginalized counties, 0 otherwise) and a post-reform dummy (1 for 2015 and 2018, 0 for 2009 and 2013). This variable captures the average treatment effect on the treated (ATET), interpreted as the additional effect of preferential fiscal transfers to marginalized counties relative to counties subject only to baseline decentralization. X_{hct} is a vector of household-level covariates, including financial literacy, financial advice, age, gender, household size, education level, marital status, land and home ownership, and rural/urban

¹ Analysis including the logarithm of income and a financial inclusion index based on twelve questions on whether a household owns or has never owned six formal financial products do not alter our conclusions.

Table 1
Descriptive statistics and mean differences.

Panel A: Between MCs and PCs								
	All households		Privileged regions		Marginalized regions		t-tests	
	Mean	SD	Mean	SD	Mean	SD	b	t
Consumption – Pre-reform								
Total consumption	5710.31	14388.15	6148.57	15267.83	3250.46	7314.70	2898.12***	(13.20)
Mobile airtime expenses	567.87	6401.81	621.14	6941.78	268.82	664.45	352.32***	(5.21)
Education expenses	2311.55	7656.65	2520.32	8057.61	1139.77	4653.54	1380.55***	(10.64)
Household bills	692.36	2157.89	686.00	2180.48	728.01	2026.54	-42.01	(-0.84)
Medical expenses	654.74	4468.35	689.69	4696.35	458.56	2863.24	231.14**	(2.95)
Rent/mortgage expenses	778.01	3878.03	882.26	4186.88	192.90	810.45	689.36***	(15.75)
Supporting family	705.79	2597.87	749.15	2655.76	462.39	2230.41	286.76***	(5.10)
Observations	13047		11074		1973		13047	
Consumption – post-reform								
Total consumption	8289.09	94065.93	8525.91	107527.11	7548.77	19105.19	977.14	(0.99)
Mobile airtime expenses	878.23	1592.71	916.19	1665.05	757.72	1330.41	158.47***	(6.24)
Education expenses	4314.31	92481.63	4409.44	105807.89	4016.65	16890.91	392.78	(0.41)
Household bills	550.53	1645.91	522.17	1490.29	639.11	2055.23	-116.94***	(-3.40)
Medical expenses	784.29	3110.53	683.74	2966.38	1093.99	3499.84	-410.25***	(-6.77)
Rent/mortgage expenses	862.60	2863.98	1006.65	3057.91	411.56	2083.10	595.09***	(14.21)
Supporting family	987.28	8242.66	1074.53	9320.08	715.82	2997.04	358.70***	(3.80)
Observations	17334		13133		4201		17334	
Panel B: Within MCs and PCs								
	All households		Pre-reform		Post-reform		t-tests	
	Mean	SD	Mean	SD	Mean	SD	b	t
Privileged counties								
Total consumption	7438.35	79878.55	6148.57	15267.83	8525.91	107527.11	-2377.33*	(-2.50)
Mobile airtime expenses	780.41	4867.52	621.14	6941.78	916.19	1665.05	-295.04***	(-4.37)
Education expenses	3542.39	78022.32	2520.32	8057.61	4409.44	105807.89	-1889.12*	(-2.03)
Household bills	597.47	1841.74	686.00	2180.48	522.17	1490.29	163.83***	(6.69)
Medical expenses	686.52	3870.78	689.69	4696.35	683.74	2966.38	5.96	(0.11)
Rent/mortgage expenses	949.63	3619.84	882.26	4186.88	1006.65	3057.91	-124.39**	(-2.60)
Supporting family	924.34	7074.50	749.15	2655.76	1074.53	9320.08	-325.38***	(-3.79)
Observations	24207		11074		13133		24207	
Marginalized counties								
Total consumption	6175.18	16415.11	3250.46	7314.70	7548.77	19105.19	-4298.31***	(-9.67)
Mobile airtime expenses	598.68	1179.05	268.82	664.45	757.72	1330.41	-488.89***	(-15.42)
Education expenses	3092.96	14228.13	1139.77	4653.54	4016.65	16890.91	-2876.88***	(-7.43)
Household bills	667.67	2046.31	728.01	2026.54	639.11	2055.23	88.90	(1.59)
Medical expenses	887.96	3320.00	458.56	2863.24	1093.99	3499.84	-635.43***	(-7.02)
Rent/mortgage expenses	341.44	1780.01	192.90	810.45	411.56	2083.10	-218.66***	(-4.50)
Supporting family	634.19	2775.67	462.39	2230.41	715.82	2997.04	-253.44***	(-3.34)
Observations	6174		1973		4201		6174	

Note: Table presents outcome variables descriptive statistics and mean differences for the full sample and sub-samples of marginalized and endowed regions. Panel A presents descriptive statistics for key consumption and expenditure categories, comparing households in marginalized counties (MCs) and privileged counties (PCs) before and after the decentralization reform. Mean difference tests (*t*-tests) assess whether the differences between the two groups are statistically significant. Panel B reports within-group comparisons for MCs and PCs, showing changes in household consumption and expenditures from the pre-reform to post-reform periods. Mean difference tests evaluate the significance of changes over time within each group. All monetary values are in Kenyan Shillings (KES). Statistical significance is denoted as follows: *** $p < .01$, ** $p < .05$, * $p < 1$.

residence. μ_c and δ_t are county and year fixed effects, respectively, controlling for time-invariant regional characteristics and common time shocks. ε_{hct} is the error term.

The dependent variables are log-transformed to reduce skewness and allow for interpretation of coefficients as approximate percentage changes in consumption. Standard errors are clustered at the county level to account for potential intra-county correlation in unobserved shocks, which is appropriate given the repeated cross-sectional nature of the data.

This specification relies on the parallel trends assumption, which posits that in the absence of the reform, consumption trends in marginalized and privileged counties would have evolved similarly. To assess the validity of this assumption, I conduct formal parallel-trend tests and Granger causality tests. The parallel-trends procedure evaluates the joint significance of interaction terms between the treatment group and pre-reform years (2009 and 2013), testing whether the trends in outcomes differed prior to the intervention. A failure to reject the null hypothesis supports the credibility of the DiD design by indicating that the groups followed similar trajectories before the reform. The Granger causality test evaluates whether future treatment status predicts past outcomes. This helps rule out anticipatory effects or reverse causality

that could bias the estimates.

As a complementary check, I estimate a regression model using only the pre-reform survey waves (2009 and 2013), following the approach proposed by [Autor \(2003\)](#) and further developed by [Schmidheiny and Siegloch \(2023\)](#). In this specification, each outcome variable—total household consumption and the disaggregated expenditure categories—is regressed on a time dummy for 2013, a treatment dummy identifying marginalized counties, and their interaction. The model also includes relevant household-level control variables, with standard errors clustered at the county level to account for intra-county correlation. An insignificant coefficient on the interaction term indicates that treated and control counties followed statistically similar pre-reform trajectories, supporting the parallel trends assumption and reinforcing the credibility of the DiD identification strategy. To further assess the validity of the parallel-trends assumption, I additionally implement an event-study analysis using the group-time average treatment effects framework of [Callaway and Sant'Anna \(2021\)](#). This approach allows treatment effects to vary across cohorts and over time and provides a transparent visualization of pre-treatment dynamics, enabling a direct test for differential trends between treated and control counties prior to the reform.

To enhance the robustness of the analysis, I implement a comprehensive set of sensitivity checks. First, I estimate quantile difference-in-differences regressions to assess heterogeneity in treatment effects across the distribution of household consumption expenditures, estimating effects at selected conditional quantiles with county and year fixed effects and county-clustered standard errors. Second, I apply entropy balancing to reweight control counties and ensure covariate balance on observed characteristics prior to treatment. Third, to address concerns that contemporaneous household controls may induce post-treatment bias, I re-estimate the model using pre-reform (baseline) county-level covariates interacted with year dummies, rather than contemporaneous values. Fourth, I examine alternative estimands and variable definitions, including continuous per-capita transfers in place of the marginalization indicator and alternative constructions of consumption outcomes. Fifth, I implement falsification tests and alternative inference procedures, including placebo assignments and corrections for few clusters. Finally, I conduct spatial robustness checks, including tests for adjacency-driven spillovers, distance-based spatial heterogeneity, and models with spatially lagged treatment exposure to account for cross-county interactions. Collectively, these robustness analyses reinforce the validity, consistency, and interpretability of the estimated effects.

5. Results

5.1. Main results

5.1.1. Average treatment effects on the treated

The results from the difference-in-differences estimation, presented in [Table 2](#), indicate that Kenya's 2013 decentralization reform had a statistically significant and positive impact on household consumption across all expenditure categories.² The ATET, captured by the interaction between the marginalization and post-reform dummies, is positive and statistically significant in all seven models. The largest effect is observed in medical expenses, which increased by approximately 43.4 %, suggesting that the reform may have improved access to healthcare services and facilitated the uptake of health insurance in marginalized counties. Total household consumption rose by 43.3 %, confirming the reform's broad positive impact on household welfare.

Rent expenditures increased by 38.6 %, likely reflecting increased demand for housing in areas where infrastructure and public services improved. Education spending rose by 36.7 %, indicating enhanced access to schooling and greater household investment in education. Household bills, which include utilities and other recurring expenses, increased by 29.0 %, consistent with improved service delivery. Mobile airtime expenses rose by 16.4 %, reflecting the widespread use of mobile phones and mobile money services such as M-Pesa. The smallest effect was observed in family transfers, which increased by 12.8 % and were statistically significant at the 5 % level. This may reflect reduced reliance on informal financial support networks as public services became more accessible.

The models also control for a range of household characteristics, including financial literacy, financial advice sources, gender, education level, marital status, age group, household size, home ownership, rural residence, and year fixed effects. Notably, higher education levels, formal financial advice, and urban residence are consistently associated with higher consumption across categories.

To assess the validity of the parallel trends assumption underlying the difference-in-differences strategy, both formal statistical tests and graphical diagnostics were employed. [Table 2](#) reports the results of pre-trend (Granger causality) tests for each consumption category. For most categories—mobile airtime ($F = 1.81$, $p = 0.1848$), education ($F = 1.04$,

$p = 0.3140$), medical expenses ($F = 1.56$, $p = 0.2180$), rent ($F = 0.13$, $p = 0.7176$), and family transfers ($F = 1.31$, $p = 0.2578$)—the F-statistics are low and the associated p-values exceed conventional significance thresholds, suggesting no statistically significant differences in pre-treatment trends between marginalized and non-marginalized counties. These results support the credibility of the identification strategy.

However, the tests reveal statistically significant pre-treatment differences for household bills ($F = 7.20$, $p = 0.0101$) and total consumption ($F = 7.04$, $p = 0.0109$), indicating potential deviations from the parallel trends assumption. [Fig. 5](#) complements these findings by plotting observed means and fitted values from a linear-trends model for both treatment and control groups over time. The visual evidence generally supports parallel trends prior to the reform, but some divergence is observed for household bills and total consumption. These exceptions suggest potential violations of the parallel trends assumption and warrant cautious interpretation of the estimated effects for these categories.

5.1.2. Quantile treatment effects

To examine heterogeneity in the effects of decentralization across households, [Table 3](#) reports quantile treatment effects (QTEs) estimated over the distribution of household consumption expenditures. Quantiles are defined with respect to household consumption, such that lower (upper) quantiles correspond to relatively poorer (richer) households in consumption terms. The analysis covers total consumption as well as the six disaggregated expenditure categories. [Fig. 6](#) complements [Table 3](#) by graphically presenting the QTEs across quantiles with 95 % confidence intervals. Together, these results provide a detailed view of how the reform affected different segments of the consumption distribution.

For total household consumption, the estimated effects are positive and statistically significant across all quantiles, with the largest impacts occurring at the lower end of the distribution. The ATET is largest at the lower quantiles and declines toward the upper end of the distribution, falling from 0.355 at the 10th percentile to 0.280 at the 90th percentile. This pattern indicates that poorer households experienced proportionally larger increases in overall consumption, consistent with the reform easing binding resource constraints and improving access to basic services in marginalized counties.

The distributional patterns differ across expenditure categories. Mobile airtime expenditures show modest but positive treatment effects across the distribution, with statistically significant gains concentrated at the lower quantiles and again toward the upper tail, and weaker or insignificant effects around the median. Household bills exhibit the strongest effects at the lower and middle quantiles, tapering off toward the top of the distribution, suggesting that improvements in access to utilities and basic services disproportionately benefited lower-consumption households. Rent expenditures show positive and statistically significant effects across most quantiles, peaking in the middle-to-upper quantiles and becoming insignificant at the 90th percentile, consistent with increased housing demand or improved housing quality among middle-income households.

In contrast, education and medical expenditures display increasing effects across the consumption distribution. For education, the ATET rises steadily from 0.233 at the 10th percentile to 0.585 at the 90th percentile, while medical expenditures show negligible effects at the bottom but large and significant increases at higher quantiles. These patterns indicate that higher-consumption households adjusted their expenditure composition more strongly along these margins, even though their total consumption gains were smaller than those of poorer households.

This heterogeneous response is consistent with standard consumption-smoothing frameworks, such as the Permanent Income Hypothesis and the Life-Cycle model, though alternative explanations cannot be ruled out. Poorer households appear to respond primarily by increasing overall consumption when resources and service access

² Table A3 in the [Supplementary Information](#) presents the extended version of [Table 2](#), reporting the effects of all control variables.

Table 2

Average Treatment Effects on the Treated (ATET) for Total Household Consumption and Expenditure Categories.

	Mobile airtime	Education	Household bills	Medical	Rent	Family transfers	Total consumption
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATET (Marginalization dummy $\times$ reform dummy)	0.164* (1.79)	0.367*** (3.57)	0.290* (1.87)	0.434*** (2.74)	0.386*** (3.30)	0.128 (1.33)	0.433*** (3.54)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	5.834*** (94.47)	6.293*** (85.74)	5.512*** (63.36)	5.683*** (84.17)	6.609*** (77.28)	6.331*** (90.47)	6.673*** (97.20)
Parallel-trends test							
F-statistic	1.81	1.04	7.20	1.56	0.13	1.31	7.04
Prob>F	0.1848	0.3140	0.0101	0.2180	0.7176	0.2578	0.0109
Granger causality test							
F-statistic	1.81	1.05	7.19	1.56	0.13	1.31	7.02
Prob>F	0.1848	0.3116	0.0102	0.2176	0.7230	0.2592	0.0110
Observations	22042	17207	12573	14857	6849	12815	27607

Note: This table presents the estimated Average Treatment Effects on the Treated (ATET) from the difference-in-differences specification, capturing the impact of the decentralization reform on total household consumption and six disaggregated expenditure categories. The key coefficient of interest is the interaction term between the marginalization dummy and the post-reform dummy. All models include control variables that are unlikely to be affected by the reform, with income and financial inclusion excluded to avoid post-treatment bias. Standard errors are clustered at the county level. Results from parallel trends and Granger causality tests are reported below each model. Full regression results, including coefficients for control variables, are provided in Table A4 in the appendix. Statistical significance: *** $p < .01$, ** $p < .05$, * $p < 1$.

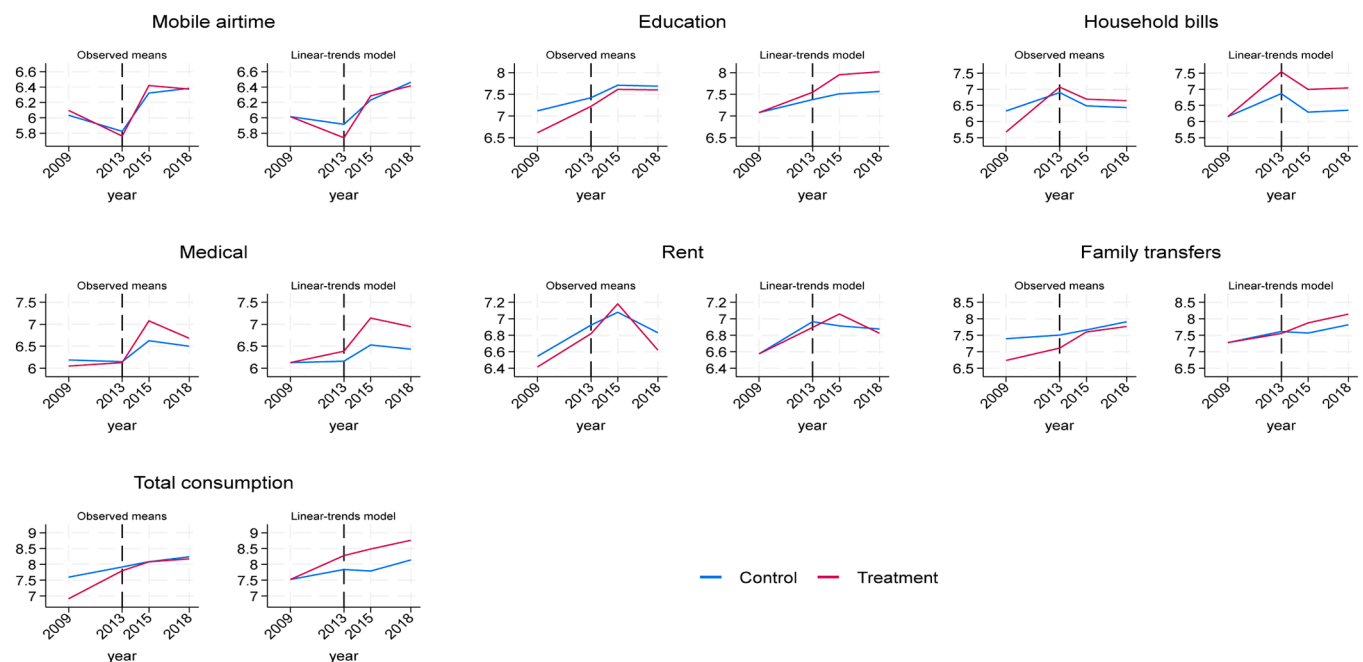


Fig. 5. Graphical diagnostics for the parallel trends assumption across consumption categories. Each panel compares observed means (left) and fitted values from a linear-trends model (right) for treatment (marginalized counties, red line) and control (privileged counties, blue line) groups from 2009 to 2018. The visual evidence supports the parallel trends assumption for most categories prior to the 2013 reform, with some divergence observed in household bills and total consumption.

improve, while less constrained households exhibit larger reallocations toward education and health—categories often interpreted as longer-term investments. At the same time, the decline in family transfers toward the top of the distribution—statistically significant from the 70th percentile onward and largest at the 90th percentile—may reflect a reduced reliance on informal private insurance as public provision improves, but could also be driven by changes in remittance behavior, migration, or household composition. Importantly, these interpretations are suggestive rather than causal: the quantile estimates document systematic heterogeneity in expenditure responses, but the data do not allow the underlying behavioral mechanisms to be separately identified.

Overall, the quantile analysis shows that decentralization generated broad-based consumption gains, while also reshaping expenditure patterns across the distribution. The reform appears to have delivered immediate welfare improvements for lower-consumption households

through higher total spending and basic services, while enabling higher-consumption households to adjust spending toward education and health. These findings underscore the importance of distributional analysis in evaluating fiscal decentralization reforms and caution against interpreting average treatment effects as representative of all households.

5.1.3. Causal mediation analysis

The effects of decentralization on household consumption may operate through multiple channels, with county budget allocation and household income being the most prominent. In this section, the analytic goal is not a full decomposition of all possible mechanisms, but an assessment of the relative importance of two policy-proximate channels that are directly measurable in the data. These mechanisms correspond to the final stages of the causal chain outlined before, whereby fiscal

Table 3
Quantile treatment effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Models	0.10	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
Mobile airtime expenses									
ATET	0.221*** (0.0768)	0.232*** (0.0662)	0.0964* (0.0532)	0.0793 (0.0597)	0.0985** (0.0470)	0.133** (0.0517)	0.0738 (0.0580)	0.114* (0.0655)	0.125* (0.0747)
Observations	22,042	22,042	22,042	22,042	22,042	22,042	22,042	22,042	22,042
Education expenses									
ATET	0.233** (0.0922)	0.310*** (0.0730)	0.336*** (0.0749)	0.421*** (0.0705)	0.454*** (0.0849)	0.394*** (0.0705)	0.448*** (0.0801)	0.433*** (0.0890)	0.585*** (0.103)
Observations	17,207	17,207	17,207	17,207	17,207	17,207	17,207	17,207	17,207
Household bills									
ATET	0.215*** (0.0569)	0.358*** (0.0599)	0.318*** (0.0587)	0.355*** (0.0584)	0.314*** (0.0569)	0.198*** (0.0630)	0.111* (0.0598)	0.146** (0.0746)	0.216** (0.0860)
Observations	12,573	12,573	12,573	12,573	12,573	12,573	12,573	12,573	12,573
Medical expenses									
ATET	0.0123 (0.0430)	-0.0175 (0.0623)	0.0982** (0.0435)	0.263*** (0.0453)	0.569*** (0.0379)	0.607*** (0.0493)	0.660*** (0.0688)	0.731*** (0.0888)	0.745*** (0.0933)
Observations	14,857	14,857	14,857	14,857	14,857	14,857	14,857	14,857	14,857
Rent expense									
ATET	0.169 (0.111)	0.190* (0.110)	0.0893 (0.0778)	0.106 (0.0985)	0.189*** (0.0692)	0.142** (0.0689)	0.218*** (0.0827)	0.233*** (0.0840)	0.0580 (0.112)
Observations	6849	6849	6849	6849	6849	6849	6849	6849	6849
Family transfers									
ATET	-0.0960 (0.112)	-0.0615 (0.0861)	-0.0621 (0.0729)	-0.0897 (0.0684)	-0.0550 (0.0649)	-0.122 (0.0806)	-0.132* (0.0749)	-0.159** (0.0796)	-0.218*** (0.0827)
Observations	12,815	12,815	12,815	12,815	12,815	12,815	12,815	12,815	12,815
Total consumption									
ATET	0.355*** (0.0936)	0.428*** (0.0614)	0.472*** (0.0523)	0.498*** (0.0546)	0.456*** (0.0469)	0.393*** (0.0452)	0.338*** (0.0431)	0.230*** (0.0453)	0.280*** (0.0606)
Observations	27,607	27,607	27,607	27,607	27,607	27,607	27,607	27,607	27,607

Note: Table reports the estimates from difference-in-differences quantile regressions, using the specifications in Table 2. Standard errors are in parentheses: *** $p < .01$, ** $p < .05$, * $p < 1$.

devolution increases local resource flows and economic opportunities that, in turn, shape household welfare. Budget allocation captures the extent to which devolved fiscal resources translate into county-level spending capacity, while household income reflects the downstream effects of decentralization on labor markets, transfers, and private economic opportunities.

To formally assess these pathways, I conducted a causal mediation analysis following the potential outcomes framework of Imai et al. (2010). The mediation exercise is intended to test specific mechanisms and assess their relative contribution to the total effect, rather than to provide an exhaustive account of all channels through which decentralization may operate. I decomposed the total effect of decentralization into the average direct effect (ADE) and the average causal mediation effect (ACME) for each consumption category. The a priori hypotheses are that both mediators transmit positive indirect effects, with county budget allocation accounting for a larger share of the total effect than household income, particularly for public-goods-intensive expenditures such as education and health. Both mediator and outcome equations were estimated using linear regression models with robust standard errors, under the assumption of no unobserved confounding in the treatment–mediator, mediator–outcome, and treatment–outcome relationships. This sequential ignorability assumption is particularly strong in the Kenyan context, given the potential influence of unobserved county-level factors, such as governance quality, political connections, and administrative capacity, on both the mediators and household consumption outcomes. Consequently, the mediation results should be interpreted as suggestive rather than strictly causal, and are intended to complement, rather than replace, the reduced-form DiD estimates. I explicitly examine the robustness of the results through sensitivity analysis, which varies the correlation parameter (ρ) between mediator and outcome disturbances to gauge how strong an unobserved confounder would need to be to overturn the estimated effects.

To ensure parsimony and avoid overfitting, I selected covariates using lasso regression with cross-validation and the “one-standard-error” rule, which retains the simplest model within one standard error

of the minimum cross-validated error. Robustness to unmeasured confounding was assessed through a sensitivity analysis that varied the correlation parameter (ρ) between the mediator and outcome disturbances (Hicks and Tingley, 2011).³ Figs. 7 and 8 plot ACME against ρ , while Table 4 reports the threshold ρ at which ACME would be nullified and the implied strength of a hypothetical omitted confounder (expressed as R_M^2 , R_Y^2). Larger absolute values of ρ and higher R^2 products indicate greater robustness. Higher absolute values of these parameters indicate that stronger unobserved confounding would be required to overturn the mediation results.

County budget allocation (Panel A) emerges as a strong mediator. ACMEs are positive and statistically significant across most outcomes, especially medical (0.291), education (0.256), and total consumption (0.285), with 52–70 % of the total effect transmitted through this channel. Sensitivity analysis confirms that these indirect effects are reasonably robust: nullifying ACME would require residual correlations on the order of $\rho \approx 0.12$ – 0.23 , corresponding to an omitted confounder explaining roughly 2–5 % of the residual variance in each equation (or 8–23 % jointly). Fig. 7 shows that education, medical, and total consumption maintain positive ACME until high values of ρ , while rent—and to a lesser extent mobile airtime—exhibit steeper declines, indicating greater sensitivity. Household bills exhibit a negative ACME and a negative share mediated, implying a suppression effect; in this case, ACME would reach zero at $\rho \approx -0.06$, consistent with the pattern observed in the sensitivity plot.

Household income (Panel B) also mediates part of the reform’s impact, though to a smaller extent for most consumption categories. ACMEs are positive and significant across all categories, particularly medical (0.077) and total consumption (0.102), with approximately 13–26 % of the total effect mediated for these categories. An important exception is family transfers, for which income accounts for a

³ Estimation was implemented in Stata using the *medeff* command for causal mediation analysis and the *medsens* procedure for sensitivity diagnostics.

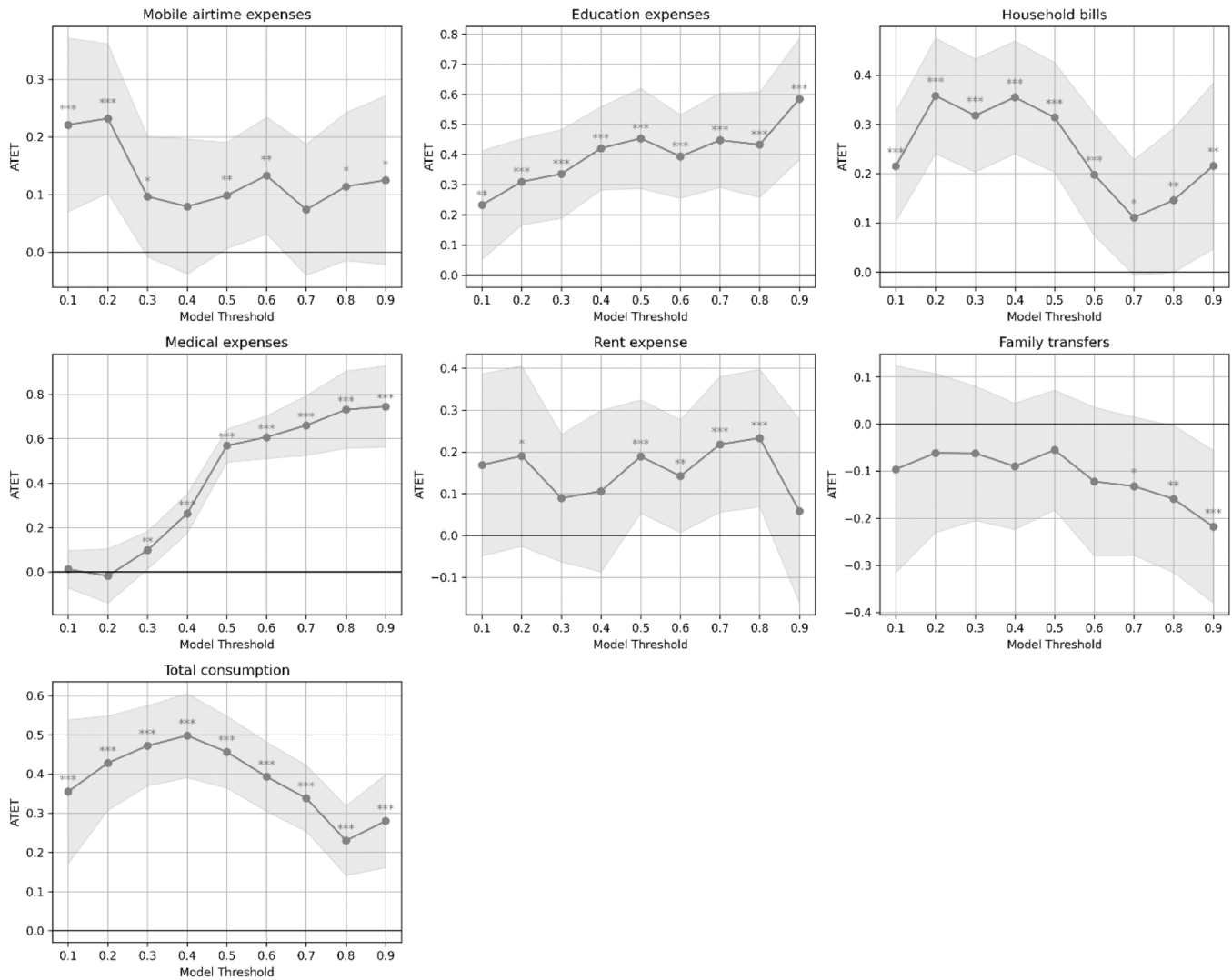


Fig. 6. Quantile treatment effects, Note: This figure presents quantile treatment effects (ATET) of targeted decentralization on household expenditure across the distribution (10th to 90th percentiles). Each subplot corresponds to a specific expenditure category and displays point estimates with 95 % confidence intervals.

substantially larger share of the effect, suggesting that private redistribution responds strongly to income gains induced by decentralization. Fig. 8 indicates that ACME remains positive until ρ value of approximately 0.10–0.26, implying that an omitted confounder would need to explain 10–26 % of the residual variance in both the mediator and outcome equations to eliminate the estimated indirect effects. Education and household bills display comparatively greater robustness, whereas rent exhibits wider confidence intervals and crosses zero at lower values of ρ , signaling more fragile mediation estimates.

Overall, these findings support the hypothesized causal chain: decentralization increased county fiscal capacity and household income, which in turn contributed to higher household consumption, particularly in historically marginalized regions. While the mediation results should be interpreted cautiously given the strength of the identifying assumptions, the sensitivity diagnostics indicate that the main indirect effects, especially those operating through county budget allocation, are unlikely to be driven entirely by modest levels of unobserved confounding. At the same time, the analysis appropriately flags outcomes for which inference is more sensitive, underscoring the heterogeneous strength of the underlying mechanisms.

5.2. Robustness analysis

To evaluate the reliability and internal validity of the estimated treatment effects, I implement a series of robustness checks that address potential threats to identification and inference. These checks assess whether the main findings persist under alternative empirical strategies, inference methods, and measurement choices. Specifically, I assess the parallel-trends assumption using pre-reform data and event-study analyses, improve covariate balance through entropy weighting, and implement falsification tests to rule out confounding from contemporaneous policies or macroeconomic shocks. I further evaluate the robustness of the results to the limited number of clusters, alternative definitions of treatment and control groups, different operationalizations of the treatment and outcome variables, and potential spatial heterogeneity and spillover effects. Collectively, these analyses provide a comprehensive assessment of robustness and confirm that the observed improvements in household welfare are not artifacts of modeling assumptions or data construction.

5.2.1. Parallel trends assumption checks

A credible DiD design rests on the premise that, in the absence of the reform, treated and control counties would have evolved along parallel trajectories. Because our setting features multiple periods and a single

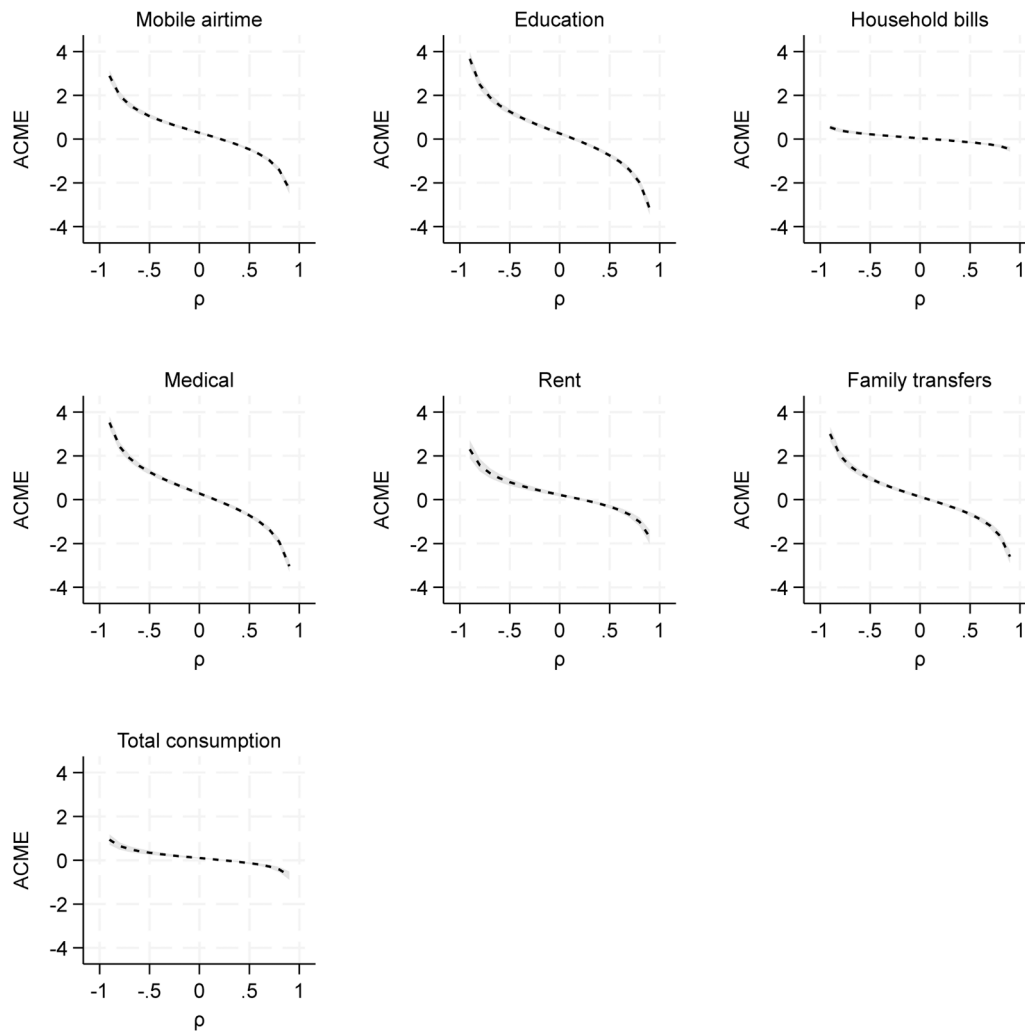


Fig. 7. Sensitivity of the Average Causal Mediation Effect (ACME) to unmeasured confounding for county budget allocation, Note: Each panel plots the estimated Average Causal Mediation Effect (ACME) of county budget allocation per capita on different categories of household consumption as a function of the sensitivity parameter (ρ), which captures the assumed correlation between the error terms of the mediator and outcome models. Shaded regions around each line indicate 95 % confidence intervals. The horizontal axis represents the degree of potential unmeasured confounding, while the vertical axis shows the magnitude of the mediation effect. The relatively stable and positive ACME values for ρ up to approximately 0.1–0.2 indicate that the indirect effects of budget allocation remain robust to moderate violations of the ignorability assumption.

group that receives the differential transfer intensity (treatment) in period 3, I combine conventional pre-trend diagnostics with the [Callaway and Sant'Anna \(2021\)](#) multi-period DiD framework to strengthen identification and transparently report dynamic treatment effects. This approach mitigates well-known pitfalls of pooled two-way fixed effects in the presence of time-varying or group-specific heterogeneity by estimating group-time average treatment effects under a conditional parallel trends restriction and then aggregating them in a principled way.

As a first and complementary check to the formal pre-trend tests in [Section 5.1.1](#), I estimate pre-period regressions using only the 2009 and 2013 waves. Each outcome is regressed on a marginalized-county indicator, a year-2013 indicator, and their interaction, with controls included and standard errors clustered at the county level. The interaction coefficient captures any differential change between marginalized and privileged counties prior to the reform. Accordingly, the identifying assumption is that, absent the differential transfer intensity, consumption trends in marginalized and privileged counties would have evolved similarly under nationwide decentralization. Consistent with our identification strategy, these interaction terms are statistically insignificant for most outcomes (Table A4 of the [Supplementary](#)

[Information](#)), lending support to the parallel trends assumption. We do, however, observe significant pre-period differences in household bills and total consumption, and we carry these caveats forward when interpreting the corresponding DiD estimates.

To further probe the parallel trends assumption and to visualize dynamics around the reform, I implement an event-study analysis using the [Callaway and Sant'Anna \(2021\)](#) difference-in-differences estimator for multiple time periods. The approach compares treated counties, which received the marginalization-based reform in 2013, to control counties in the same periods, while accounting for pre-treatment differences in county-level characteristics. At the household level, I construct a composite socio-economic index based on standardized measures of financial literacy, education levels, land and home ownership, geographic region, and marital status. This index, along with household income, is aggregated to the county-year panel using the median, capturing the central tendency of socio-economic and income characteristics while reducing sensitivity to outliers. Additional baseline covariates include per-capita county budget.

Using this setup, I estimate the dynamic treatment effects for the seven consumption categories: mobile airtime, education, household bills, medical, rent, family transfers, and total expenditure. The event-

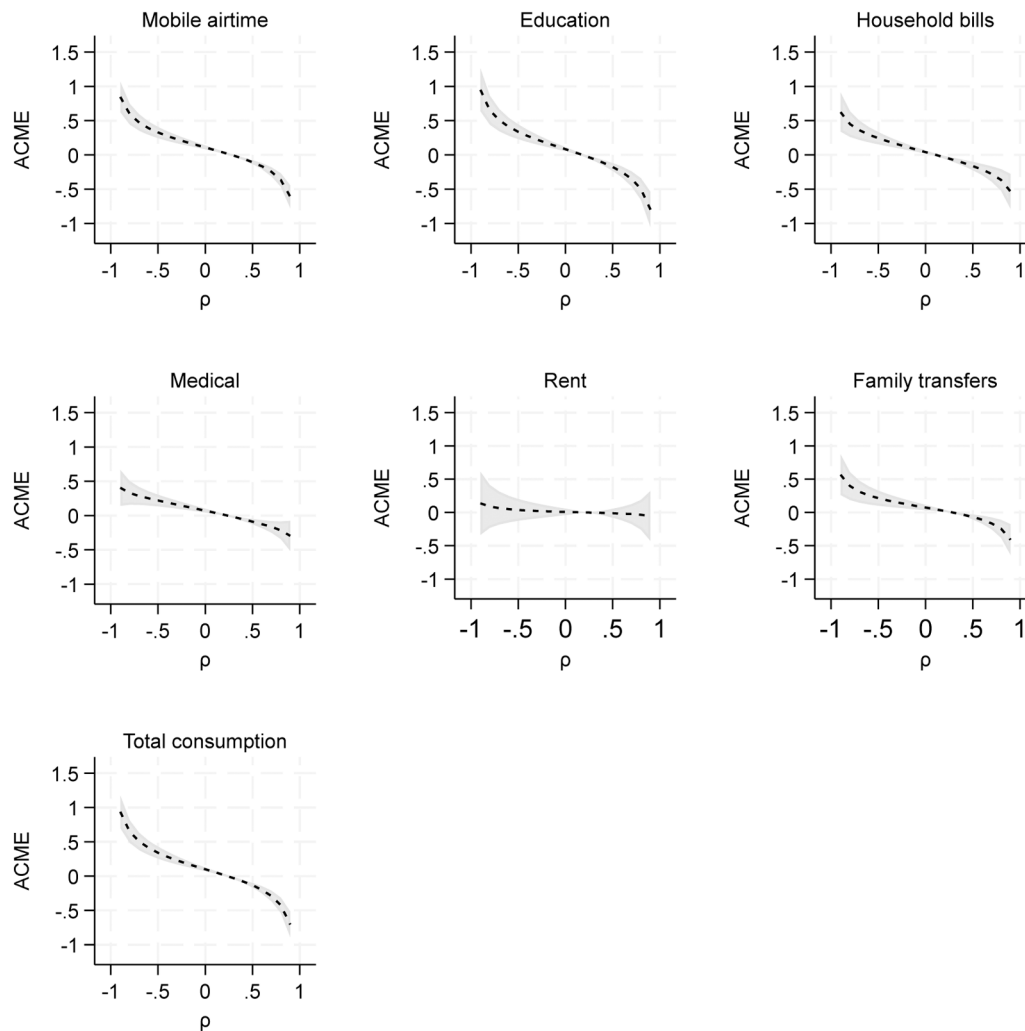


Fig. 8. Sensitivity of the Average Causal Mediation Effect (ACME) to unmeasured confounding for household income, Note: Each panel shows the estimated Average Causal Mediation Effect (ACME) of household income on different categories of household consumption as the assumed correlation (ρ) between the mediator and outcome disturbances varies. The shaded area represents 95 % confidence intervals. The ACME remains positive and statistically significant for most outcomes up to moderate values of ρ (approximately 0.1–0.2), indicating that the mediation effects of income are relatively robust to potential unmeasured confounding, though generally smaller in magnitude than those of county budget allocation.

study graphs in Fig A1 of the [Supplementary Information](#) depict the estimated average treatment effect on the treated at each time period relative to the baseline, providing a transparent view of pre-treatment trends and the evolution of post-reform impacts. Results show that the pre-treatment coefficients are close to zero and statistically insignificant for all outcomes, supporting the parallel trends assumption, while post-reform effects are positive and statistically significant for most expenditure categories. These findings are consistent with the static DiD estimates reported in the main analysis. Overall, these diagnostics confirm that pre-reform trends are parallel and that the estimated post-reform effects are attributable to the marginalization-based reform rather than pre-existing differences between counties.

5.2.2. Matching, alternative inferences, and placebo tests

This section evaluates the robustness of the baseline difference-in-differences estimates to key threats to identification, including selection on observables, confounding policy shocks, and inference with a limited number of clusters. I implement three complementary checks: covariate reweighting via entropy balancing, falsification tests based on random assignment and timing, and alternative inference procedures designed for few-cluster settings. Together, these exercises assess whether the main findings are driven by pre-existing differences,

unrelated contemporaneous reforms, or overly optimistic standard errors.

First, the substantial socioeconomic disparities between marginalized and privileged counties documented in Table A2 of the [Supplementary Information](#) raise concerns that unobservable structural differences may bias the baseline estimates reported in Table 2. To mitigate this risk, I employ entropy balancing ([Hainmueller, 2012](#)), a reweighting procedure that ensures the treatment and control groups are statistically equivalent across observed covariates. Specifically, the method reweights observations from privileged counties so that their covariate distributions match those of marginalized counties across key moments—mean, variance, and skewness—thereby improving balance and reducing potential selection bias ([Grossman et al., 2019](#); [Tao et al., 2024](#)). Unlike conventional matching approaches, entropy balancing automates the balancing process and avoids subjective specification choices. As shown in Table A5 of the [Supplementary Information](#), this procedure eliminates all pre-treatment covariate imbalances. The resulting entropy-balanced difference-in-differences estimates, reported in Panel A of Table A6, closely mirror the baseline results in Table 2. In fact, estimated treatment effects increase for all outcome variables except mobile airtime and family transfers, reinforcing the robustness of the main findings.

Table 4
Mediation and Sensitivity Analysis Results.

Metric	Mobile airtime	Education	Household bills	Medical	Rent	Family transfers	Total consumption
Panel A: Budget allocation							
ACME	0.249 (0.230, 0.268)	0.256 (0.229, 0.282)	-0.050 (-0.079, -0.023)	0.291 (0.261, 0.320)	0.231 (0.201, 0.262)	0.126 (0.102, 0.151)	0.285 (0.263, 0.309)
ADE	0.163 (0.115, 0.209)	0.130 (0.071, 0.191)	0.147 (0.086, 0.209)	0.270 (0.210, 0.329)	-0.070 (-0.150, 0.012)	-0.009 (-0.075, 0.054)	0.123 (0.076, 0.170)
Total Effect	0.412 (0.370, 0.457)	0.386 (0.330, 0.441)	0.096 (0.043, 0.150)	0.560 (0.507, 0.615)	0.161 (0.087, 0.240)	0.117 (0.060, 0.179)	0.408 (0.368, 0.452)
% Mediated	0.604 (0.544, 0.673)	0.663 (0.580, 0.775)	-0.522 (-1.176, -0.335)	0.519 (0.472, 0.573)	1.424 (0.964, 2.660)	1.075 (0.701, 2.090)	0.701 (0.632, 0.775)
Rho	0.205	0.140	-0.059	0.134	0.229	0.086	0.121
$R^2_{M \rightarrow R^2_Y}$	0.042	0.020	0.004	0.018	0.053	0.007	0.015
$R^2_{M \sim R^2_Y \sim}$	0.027	0.012	0.003	0.012	0.031	0.005	0.010
Panel B: Income							
ACME	0.106 (0.095, 0.118)	0.058 (0.048, 0.068)	0.041 (0.032, 0.050)	0.077 (0.065, 0.088)	0.041 (0.024, 0.059)	0.085 (0.067, 0.103)	0.102 (0.090, 0.114)
ADE	0.320 (0.275, 0.364)	0.330 (0.276, 0.387)	0.079 (0.024, 0.136)	0.494 (0.440, 0.548)	0.127 (0.051, 0.205)	0.056 (-0.001, 0.111)	0.295 (0.253, 0.338)
Total Effect	0.427 (0.382, 0.470)	0.388 (0.333, 0.446)	0.120 (0.064, 0.176)	0.571 (0.517, 0.626)	0.168 (0.089, 0.248)	0.141 (0.086, 0.200)	0.397 (0.352, 0.442)
% Mediated	0.249 (0.226, 0.278)	0.149 (0.130, 0.173)	0.340 (0.231, 0.636)	0.134 (0.123, 0.148)	0.246 (0.166, 0.464)	0.602 (0.427, 0.994)	0.257 (0.231, 0.289)
Rho	0.262	0.183	0.103	0.155	0.212	0.258	0.229
$R^2_{M \rightarrow R^2_Y}$	0.069	0.033	0.011	0.024	0.045	0.066	0.053
$R^2_{M \sim R^2_Y \sim}$	0.043	0.021	0.008	0.018	0.026	0.039	0.030

Note: Table reports mediation and sensitivity analysis. Panel A presents the results when Budget per Capita is the mediator, while Panel B reports results when Income is the mediator. For each category, the table shows the Average Causal Mediation Effect (ACME), representing the indirect effect of decentralization through the mediator; the Average Direct Effect (ADE), which captures the direct effect excluding mediation; and the Total Effect, combining both direct and indirect effects. The % Mediated indicates the proportion of the total effect explained by the mediator. Sensitivity metrics include Rho, the correlation between mediator and outcome residuals that would nullify ACME ($ACME=0$); $R^2_{MR^2_Y}$ and $R^2_{M \sim R^2_Y \sim}$, which quantify the strength of an unobserved confounder required to eliminate the mediation effect. Specifically, $R^2_{MR^2_Y}$ represents the proportion of residual variance in the mediator and outcome explained by the confounder, while $R^2_{M \sim R^2_Y \sim}$ reflects the proportion of total variance in the mediator and outcome explained by the confounder. Confidence intervals are reported in parentheses.

Second, to rule out the possibility that the estimated effects are driven by concurrent policy changes or broader macroeconomic trends rather than the decentralization reform itself, I conducted two falsification tests. During the study period, Kenya expanded several social protection programs, such as the Inua Jamii cash transfers,⁴ and introduced the 2016 interest rate cap under the Banking (Amendment) Act. Both policies could plausibly influence household consumption patterns. For example, unconditional cash transfers have been shown to increase consumption and welfare in rural Kenya (Haushofer and Shapiro, 2016), while the interest rate cap reduced access to credit for micro, small, and medium enterprises and disproportionately affected riskier borrowers in marginalized regions (Alper et al., 2020; Safavian and Zia, 2018). If the baseline results were driven by such unrelated reforms or general time trends, placebo treatment effects should be weak or contradictory. To test this, I first randomly assign placebo treatment status while preserving the original treatment group size, and second randomly reassign treatment timing while maintaining the number of treated observations. Results reported in Panels B and C of Table A6 of the [Supplementary Information](#) show that placebo effects are largely insignificant or negative, with the sole exception of household bills in one specification. Importantly, these patterns remain unchanged when entropy balancing weights are applied, lending further credibility to the causal interpretation of the main results.

Third, given the relatively small number of clusters—47 counties in this study—standard cluster-robust inference may underestimate sampling variability. To address this concern, I re-estimated standard errors

using two complementary approaches. First, I apply bias-corrected cluster-robust standard errors with the [Bell and McCaffrey \(2002\)](#) degrees-of-freedom adjustment, as recommended by [Imbens and Kolesár \(2016\)](#). This correction mitigates the downward bias in variance estimates commonly observed with few clusters. Second, I implement the wild cluster bootstrap procedure, which provides more reliable p-values and confidence intervals in settings with few clusters and weak distributional assumptions ([Imbens and Kolesár, 2016](#)). Results from these alternative inference methods, reported in Table A7 of the [Supplementary Information](#), are fully consistent with the baseline findings in Table 2. Coefficient magnitudes remain unchanged, while t-statistics are modestly smaller, reflecting the more conservative inference. Crucially, expenditures on education, medical, rent, and total consumption remain positive and statistically significant under both approaches, confirming that the main conclusions are not driven by underestimated standard errors.

Finally, to address the concern that contemporaneous household controls can induce post-treatment bias, I re-estimate the model using pre-reform (baseline) county-level covariates interacted with year dummies, rather than contemporaneous values. Because the data are repeated cross-sections, baselines are constructed at the county level from pre-reform waves and merged into all waves. To mitigate multicollinearity among compositional shares, I adopt a parsimonious baseline set of variables. This includes county shares of secondary and tertiary education, divorced marital status, financial literacy (“agree”), land ownership, log population, log of income, financial inclusion, and region, each interacted with year dummies. At the individual level, I retain only exogenous attributes, including gender, age-group dummies, and interview language (indicator for Swahili), to absorb observable heterogeneity without conditioning on post-treatment characteristics. Table A8 of the [Supplementary Information](#) reports the corresponding ATETs. Coefficients remain positive across all categories and are statistically significant for mobile airtime, education, medical, rent, and total consumption. Effects on household bills and family transfers are positive

⁴ Inua Jamii—a Swahili phrase meaning “uplift the community”—is Kenya’s flagship social protection program ([Gelb et al., 2023](#); [Porisky et al., 2023](#)). It provides regular, unconditional cash transfers to vulnerable populations, including older persons, orphans and vulnerable children, and persons with severe disabilities. Administered by the Ministry of Labour and Social Protection, the program aims to reduce poverty, enhance household resilience, and promote financial inclusion, particularly in marginalized regions.

but small and insignificant. Notably, the estimated effects for medical and total consumption are somewhat larger than in the main specification, while those for education and rent remain very close to the baseline estimates, suggesting that the central findings are unlikely to be driven by post-treatment conditioning or multicollinearity and appear robust to a more bias-averse specification.

5.2.3. Alternative county grouping and variable definitions

This section tests the robustness of the main findings to alternative definitions of treatment, outcomes, and the estimand. Given that decentralization involved both marginalization-based targeting and heterogeneous transfer increases across counties, these checks assess whether the results reflect county designation, measurement choices, or fiscal intensity.

First, I assess whether the results are robust to alternative definitions of treated and control counties. The baseline classification of “marginalized” and “privileged” counties follows the official designation of the CRA, which governs equitable resource distribution under Kenya’s decentralization framework. As described in [Section 2](#), the CRA formula combines population size, land area, poverty incidence, and income distance to capture multiple dimensions of structural disadvantage. To test the sensitivity of the results to this operationalization, I redefine treatment status using two alternative criteria: (a) county land surface area (top and bottom five counties) and (b) county population size (top and bottom five counties). These alternative groupings isolate key geographic and demographic components embedded in the CRA’s approach. As shown in Table A9 of the [Supplementary Information](#), the estimated treatment effects remain stable across specifications. In particular, large and sparsely populated counties exhibit positive and significant effects, while smaller and more densely populated counties show negative, weaker, or insignificant responses, consistent with the redistributive objectives of the reform. This consistency suggests that the baseline results are not an artifact of county classification but reflect genuine redistribution toward structurally disadvantaged regions.

Second, I examine the sensitivity of the results to alternative definitions of household consumption. Because welfare assessments can depend on how consumption is measured ([Deaton, 2003](#); [Grootaert, 1983](#)), I re-estimate all models using two alternative outcome specifications: (a) binary indicators equal to one if any expenditure is reported and zero otherwise, and (b) per capita expenditure, obtained by dividing total household spending by household size. The binary specification captures participation in consumption, reflecting access to goods and services, while the per capita measure adjusts for household composition and aligns more closely with Kenya’s population-based revenue-sharing rules. Results reported in Table A10 (Panel A) of the [Supplementary Information](#) confirm the robustness of the main findings. Per capita estimates closely mirror the baseline results in both magnitude and significance, while binary outcomes yield smaller but consistently positive effects across most categories. These findings indicate that decentralization increased both the likelihood of consumption and the intensity of spending, reinforcing the welfare interpretation of the results.

Third, I address concerns that the binary treatment indicator conflates decentralization with differences in fiscal magnitude by replacing the policy dummy with county-level per-capita transfers as a continuous treatment variable. This specification reframes the estimand as a dose–response relationship, capturing how household consumption responds to variation in fiscal resources rather than designation status alone. The results, reported in Table A10 (Panel B) of the [Supplementary Information](#), show that higher per-capita transfers are associated with statistically significant increases in education, medical, housing, and total household expenditure, closely mirroring the patterns observed in the baseline DiD estimates. These findings clarify that while the primary estimand of the paper is the effect of marginalization designation under decentralization, variation in transfer intensity explains meaningful heterogeneity in outcomes. Thus, this complementary analysis confirms

that the baseline results are not driven by a binary comparison of counties but reflect both institutional targeting and differential fiscal intensity within Kenya’s decentralized framework.

5.2.4. Spatial heterogeneity and spillover effects

[Fig. 1](#) reveals pronounced geographic clustering in treatment assignment: marginalized counties are concentrated in the northern, northeastern, and coastal regions, while privileged counties are predominantly located in central and western Kenya. This spatial pattern raises concerns that spillovers across county borders and regionally correlated shocks may compromise the quasi-experimental interpretation of the difference-in-differences design. In addition, county boundaries in Kenya largely reflect colonial administrative divisions rather than functioning as economically integrated or service-delivery units ([Greiner, 2013](#); [Smoke, 1993](#)), increasing the likelihood of cross-border interactions through migration, trade, and service usage. These features motivate a systematic assessment of whether treatment effects vary with geographic proximity or are contaminated by spatial spillovers. To address these concerns, I implement three complementary spatial robustness checks designed to assess heterogeneity in treatment effects and explicitly test for spillover mechanisms.

First, to probe adjacency-driven spillovers, I partition marginalized counties into those that directly border at least one privileged county (Samburu, West Pokot, Narok, Tana River, Taita Taveta, Isiolo, Kwale, and Kilifi) and those that are geographically isolated from privileged counties (Turkana, Wajir, Marsabit, Mandera, Lamu, and Garissa). Geographically isolated counties tend to score lower on the CRA’s development and fiscal capacity indices and therefore receive larger per-capita allocations from the equalization component of intergovernmental transfers, implying a stronger fiscal shock under decentralization. I then re-estimate the DiD specification separately for each subsample, clustering standard errors at the county level. The results, reported in Table A11 of the [Supplementary Information](#), show that for bordering treated counties, the estimated reform effects remain positive and statistically significant for mobile, education, rent, and total consumption, but are insignificant for household bills, medical, and family transfers. In contrast, for isolated marginalized counties, treatment effects are positive and statistically significant across education, household bills, medical, rent, family transfers, and total consumption, while effects on mobile airtime are positive but imprecisely estimated. Magnitudes are consistently larger than those observed in bordering counties. This pattern is consistent with a combination of cross-county spillovers attenuating measured impacts near treated–control borders and greater treatment intensity, through higher equalization transfers, in more remote marginalized counties.

Second, I examine spatial heterogeneity by interacting the treatment indicator with geographic distance to the nearest control county. Following established approaches in the literature ([Alonso-Villar and del Río, 2017](#); [Dharmasankar and Yoo, 2023](#); [Greenaway-McGrevy, 2025](#)), distances are computed using the Haversine formula based on county capital coordinates. For marginalized counties, distance is defined as the great-circle distance to the closest privileged county capital; for privileged counties, it is the minimum distance to any marginalized county capital. This specification directly tests whether treatment effects attenuate or amplify with proximity, thereby addressing concerns related to control group contamination via migration, trade, or service usage, as well as spatially concentrated shocks such as drought cycles or conflict dynamics. For interpretability, I report average treatment effects at distances of 0 km, 100 km, and 200 km from the reform boundary; results are robust to alternative transformations and winsorization of the distance measure. As shown in Table A12 of the [Supplementary Information](#), treatment effects vary systematically with distance for several expenditure categories. Education and total consumption effects remain positive and statistically significant even at 200 km, while medical expenditure effects increase with distance, suggesting that proximity plays an important role in shaping reform impacts.

Third, I explicitly model spatial spillovers by augmenting the baseline specification with a spatially lagged treatment variable capturing exposure to neighboring counties' marginalization status. Specifically, I construct an inverse-distance weighted measure of neighboring counties' eligibility, where weights are based on the great-circle distance between county centroids and are row-standardized to sum to one.⁵ This approach allows for continuous spillover effects arising from geographic proximity, rather than relying solely on discrete border or distance thresholds. The augmented specification includes both the county's own treatment status and the spatially lagged treatment term, while retaining county and year fixed effects. Results from this spatial lag model, summarized in Table A13 of the [Supplementary Information](#), indicate that the direct effect of decentralization eligibility remains positive and statistically significant for education, medical, and total household consumption expenditures, with magnitudes comparable to the baseline estimates. Spillover effects, as captured by the spatially lagged treatment variable, are limited and sector specific. Notably, spillovers are positive and marginally statistically significant for education expenditure.⁶

While basic and higher education are not formally devolved functions in Kenya, this pattern is consistent with indirect channels through which county-level fiscal expansions affect household education spending. These channels include improvements in complementary public services (such as health, roads, and local transport), county-supported bursaries and early childhood development programs, and income effects that relax household budget constraints. Because households may access schools across county boundaries or respond to improved local conditions by increasing educational investments, education-related expenditures are particularly likely to exhibit spatial externalities. In contrast, spillover effects are small and statistically insignificant for most other consumption categories, including total expenditure.

In sum, these three robustness tests provide consistent evidence that while treatment assignment is geographically clustered, the estimated effects of decentralization are not driven by spatial contamination of the control group or unmodeled regional shocks. Spillovers appear limited and concentrated in sector-based expenditures where cross-county interactions are theoretically expected, thereby strengthening the plausibility of the identifying assumptions underlying the DiD design and reinforcing the interpretation of the main results as causal.

6. Conclusion and discussion

This study examined how Kenya's constitutional decentralization reform affected household consumption across six expenditure categories, distinguishing between historically marginalized and privileged regions. While decentralization is often promoted as a means to improve service delivery and reduce spatial inequalities, evidence on its distributional impacts has been mixed. Using a difference-in-differences framework complemented by quantile treatment effects and causal mediation analysis, this paper contributes to the growing literature on the welfare implications of decentralization in developing economies ([Martinez-Vazquez et al., 2017](#); [Oates, 2005](#)).

⁵ The spatially lagged treatment variable is an inverse-distance weighted measure of neighboring counties' policy eligibility given by $W.Policy_{it} = \sum_{j \neq c} w_{cj} Policy_{jt}$, where $w_{cj} = \frac{1/d_{cj}}{\sum_{k \neq c} 1/d_{ck}}$ and d_{cj} is the great-circle distance between county centroids c (focal county) and j (neighbor). The weights are row-standardized so that they sum to one for each county.

⁶ Although responsibility for basic and higher education remains centralized in Kenya, county governments play an indirect role through complementary investments, early childhood development, vocational training, bursary schemes, and infrastructure provision. Household education expenditure therefore responds to county-level fiscal conditions even in the absence of formal devolution of schooling.

The results show that decentralization significantly increased household spending on education, medical, rent, and household bills, with limited effects on mobile airtime and family transfers. These gains were unevenly distributed: total consumption rose most among lower-income households, consistent with the reform's pro-poor intent, whereas upper-quantile households experienced greater increases in education and medical spending, likely reflecting stronger capacity to leverage improved infrastructure and local services ([Bold et al., 2013, 2015](#)). Meanwhile, the rise in household bills among poorer households indicates that expanded access to utilities also introduced new financial obligations, a pattern observed in other developing contexts ([Lenz et al., 2017](#); [Saing, 2018](#)). [Fig. 6](#) illustrates this dual pattern: decentralization expanded access to essential services but also reinforced stratification in consumption. These findings mirror cross-country evidence that public investments under devolution often disproportionately benefit better-off households ([Almanzar and Torero, 2017](#); [Charlery et al., 2016](#)). By highlighting heterogeneity in treatment effects, this study contributes to the broader literature calling for disaggregated analyses of decentralization outcomes ([Faguet, 2014](#); [Smoke, 2015](#)).

Causal mediation analysis further clarifies the mechanisms at work. County budget allocations and household income emerge as key mediators, jointly explaining a substantial share of the effect on education, medical, and total consumption. Roughly half of the medical spending effect, for instance, is mediated through higher county-level budgets, indicating that fiscal transfers are a primary channel through which devolution improves welfare. Income plays a complementary role, suggesting that economic capacity conditions households' ability to capitalize on decentralized services. Sensitivity analysis indicates moderate robustness of these pathways but also points to potential unobserved confounding, an inherent limitation of observational mediation models ([Hicks and Tingley, 2011](#); [Imai et al., 2010](#)).

Together, these findings demonstrate that Kenya's decentralization reform had a broadly positive but uneven impact on household welfare. Fiscal transfers effectively expanded access to public goods and stimulated consumption, but gains were stratified by distribution. These patterns reinforce the need for institutional designs that complement fiscal equity with administrative capacity and inclusive service delivery.

These findings carry important implications for regional development policy. First, they reinforce evidence that well-designed fiscal decentralization can promote spatial equity when supported by transparent, rules-based intergovernmental transfers ([Bellofatto and Besfamille, 2021](#); [Sepulveda and Martinez-Vazquez, 2011](#)). Second, the quantile and distributional analyses suggest that intra-county heterogeneity matters: lower-income households in relatively affluent counties may not fully benefit from decentralized transfers. Policymakers could address this by linking allocations to local-level socio-economic indicators or household vulnerability scores, thereby improving targeting precision and inclusivity ([Guo et al., 2022](#); [Tohari et al., 2019](#)). Third, our mediation analysis shows that county budget size mediates welfare gains, highlighting the potential value of performance-based or needs-sensitive allocation mechanisms ([Banful, 2011](#); [Pal and Wahhaj, 2017](#)). For example, a portion of transfers could be conditional on measurable outcomes such as school enrollment, health service utilization, or access to basic utilities, directly connecting funding to the outcomes documented in our study. Fourth, strengthening local administrative capacity and data systems remains critical to ensure that fiscal transfers translate into tangible welfare gains. Finally, and more broadly, the results underscore the potential of decentralization to contribute to progress toward Sustainable Development Goals related to poverty reduction (SDG 1), reduced inequalities (SDG 10), education (SDG 4), and health (SDG 3). The observed increases in education and medical expenditures—particularly among higher-quantile households—and the pro-poor gains in total consumption suggest improved access to human-capital-enhancing services, though the uneven distribution of these gains highlights the importance of complementary policies to address structural barriers to access ([Muinde and Prince, 2023](#);

Porisky et al., 2023).

While these lessons are broadly relevant, several contextual features shape their interpretation and transferability to other settings. Kenya's decentralization operates under a constitutionally mandated, technocratic revenue-sharing formula designed by the CRA, which limits political discretion in intergovernmental transfers. The reform emerged from a broad national consensus around the 2010 Constitution, reducing partisan contestation over resource allocation. Additionally, Kenya's highly developed mobile-money infrastructure enables rapid adjustments in household consumption following fiscal shocks, a dynamic less likely in settings with limited financial inclusion. Finally, the spatial concentration of pastoralist economies introduces unique patterns of regional inequality that influence the distributional effects of decentralization. These conditions suggest that the findings are most relevant to countries with strong institutional frameworks, high financial connectivity, and pronounced regional disparities, and less generalizable to contexts lacking these features.

By contrast, Uganda's far-reaching decentralization—covering a five-tier local government system, fiscal and political devolution, and regular local elections—has yielded mixed outcomes due to recentralized control, funding shortfalls, capacity constraints, corruption, and district proliferation, limiting service delivery gains (Green, 2015). Similarly, Ghana's constitutionally mandated District Assemblies' Common Fund demonstrates how transfer systems can entrench central control and dependence when formulas are opaque and tax-effort incentives are weak, thereby failing to stimulate local revenue mobilization or strengthen accountability (Ayee, 1995). Accordingly, similar reforms in SSA countries with different fiscal institutions, lower digital infrastructure, or more dispersed populations may yield qualitatively similar patterns but quantitatively different outcomes. Future research should adopt comparative, multi-country designs, using harmonized household surveys or multi-level modeling, to identify the institutional and fiscal conditions under which decentralization improves welfare outcomes (Rodden, 2004; Smoke, 2015).

This study has several limitations. While the quantile analysis reveals important distributional effects, it does not unpack the mechanisms driving these patterns. Future research could employ mixed-methods approaches to explore how households navigate access to decentralized services. Additionally, the use of repeated cross-sectional data limits the ability to control for unobserved, time-invariant household characteristics. Longitudinal data would allow for more precise identification of causal pathways and long-term effects.

In conclusion, Kenya's experience underscores both the promise and the limits of fiscal decentralization. When backed by adequate local capacity, transparent transfer systems, and inclusive financial infrastructure, decentralization can serve as a powerful tool for redistributive growth. Yet without attention to institutional quality, intra-regional inequality, and precise targeting mechanisms, its transformative potential remains only partially realized.

Declaration of Competing Interest

There is no conflict of interest on the paper.

1. The paper did not receive financial support from any organisation or individual in either the public or private sector.
2. I do not hold a paid or unpaid positions as officer, director, or board member of relevant non-profit organizations or profit-making entities other than my teaching and research position at the University of Stirling.
3. No other party had the right to review the paper prior to its circulation

Acknowledgements

I thank the two anonymous reviewers for their careful and constructive feedback, which substantially improved the clarity, analytical coherence, and presentation of the paper. Their comments strengthened the conceptual framing, identification strategy, robustness analysis, and overall exposition. I am grateful for the time and expertise they dedicated to the review process.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.pirs.2026.100140.

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