

The Cultural Foundations of State Legitimacy: Kinship Intensity and Institutional Mismatch

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Job Market Paper

NOVEMBER 2026

Abstract

Why do institutional transplants succeed or fail? I argue that formal institutions lose legitimacy when they conflict with local social structures. I examine whether Common Law better accommodates kinship norms by allowing judges greater flexibility. I compare communities within ethnic homelands partitioned by borders between Civil and Common Law. Civil Law lowers economic activity in high-kinship societies but not in low-kinship societies, implying 12.6% lower GDP per capita. Surveys link this mismatch to lower state trust and civic participation. To investigate the mechanism, I analyse a new corpus of colonial court documents and find more accommodating language toward customary norms under Common Law. In Cameroon, after Civil Law judges were appointed to Common Law courts in 2016, economic activity fell three times as much in high-kinship communities as in low-kinship communities. The results suggest that compatibility between culture and institutions affects state legitimacy and economic performance.

Keywords: Institutional mismatch, Kinship, Culture, State legitimacy, Legal origins

JEL Classification: Z13, O43, O17, K10

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1 Introduction

“Laws should be so appropriate to the people for whom they are made that it is very unlikely that the laws of one nation can suit another.”

– MONTESQUIEU

Large disparities in economic development across societies are often attributed to either weak formal institutions (North, 1990; Acemoglu et al., 2001) or persistent cultural norms (Guiso et al., 2006; Nunn and Wantchekon, 2011). In this paper, I examine empirically whether ‘institutional mismatch’ between formal legal institutions and kinship-based social arrangements also contributes to state weakness and underdevelopment. Kinship intensity captures the extent to which social organisation centres on extended families and clans, as reflected in marriage, descent, and residence practices (Bahrami-Rad et al., 2022; Schulz, 2022). Civil Law emphasises codified rules, while Common Law gives judicial precedent a larger role. I test whether these differences affect the accommodation of kin-based arrangements. Comparing communities within ethnic homelands divided by Civil Law–Common Law borders, I find lower economic activity on the Civil Law side among high-kinship communities, with nightlight estimates implying approximately 12.6% lower GDP per capita. I detect no corresponding mismatch penalty among low-kinship communities.

To understand the mechanics of institutional mismatch, consider a dispute over ancestral land. In a high-kinship society, land often belongs to the lineage, not the individual (Migot-Adholla et al., 1991; Platteau, 1996, 2015). Suppose a Civil Law court applies individualised property rules that require such land to be divided or liquidated to satisfy an heir’s exit rights. Such enforcement threatens the community’s economic base and identity and can transform the state from a neutral arbiter into an adversary, encouraging the clan to retreat further into informality. A Common Law court might instead recognise and enforce the lineage’s collective rights, giving the community a reason to rely on formal

adjudication (Gennaioli and Shleifer, 2007).¹

Anthropologists and legal scholars have long documented the functional utility of kinship in providing social safety nets and contract enforcement (Malinowski, 1926; Llewellyn and Hoebel, 1941; Gluckman, 1955; Posner, 1980). Economists have documented both these insurance and enforcement benefits and the costs of in-group favouritism and limited trust beyond the group (Fafchamps, 1992; Coate and Ravallion, 1993; Akbari et al., 2019; Schulz, 2022). I hypothesise that the economic costs of kinship depend partly on how transplanted formal institutions, which presuppose impersonal cooperation and trust in anonymous authorities (Enke, 2019; Schulz et al., 2019), interact with kinship-based cooperation. Where cooperation relies on clan networks, formal institutions that undermine these arrangements may alienate communities and discourage engagement with the state.

To test this hypothesis, I begin by documenting a robust pattern: the relationship between kinship intensity and economic development is not uniformly negative (cf. Bahrami-Rad et al., 2022). Using grid-cell-level nighttime light intensity as a proxy for economic activity across 1,131 ethnolinguistic regions (Henderson et al., 2012; Donaldson and Storeygard, 2016), I find that tight kinship norms (measured using the standardised index of Schulz et al. (2019)) are negatively correlated with development only under Civil Law legal origins. Country fixed effects absorb national characteristics, allowing me to compare within-country relationships between kinship and development across the two legal traditions. Under Common Law, I find no corresponding kinship penalty. These patterns motivate the hypothesis that the economic consequences of kinship intensity are conditional on the surrounding legal-institutional environment.

To strengthen the causal interpretation, I employ a spatial regression discontinuity design across national borders that separate Civil Law and Common Law jurisdictions. Focusing on ethnic homelands partitioned by these boundaries, I estimate the effect of

¹For example, Section 19 of the Gold Coast Supreme Court Ordinance No. 4 of 1876 contained a “Repugnancy Clause”, under which local customary law could be enforced provided it was not “repugnant to natural justice, equity, and good conscience.”

assignment to Civil Law versus Common Law by comparing communities located near opposite sides of colonial borders that were often drawn without regard to local social structures (Michalopoulos and Papaioannou, 2014, 2016). The results reveal a significant ‘mismatch penalty’: in high-kinship societies, being assigned to a Civil Law jurisdiction lowers nighttime light relative to co-ethnics just across the border, corresponding to an approximate GDP-per-capita gap of 12.6% under a back-of-the-envelope conversion from nightlights. In contrast, I find no corresponding Civil Law penalty in low-kinship societies. Accounting for the population share of high-kinship societies and translating the nightlight estimates into economic output, a back-of-the-envelope extrapolation implies population-weighted aggregate output losses of up to 4.7% across the Civil Law jurisdictions represented in the sample. Beyond its direct economic effects, this finding suggests that the relative disadvantage of Civil Law is conditional, as it emerges where the legal system conflicts with the underlying social structure of the population.

I next investigate the plausible mechanisms underlying this result. If the mismatch hypothesis is correct, the economic penalty could be driven by a withdrawal from state institutions in favour of kin-based arrangements for cooperation and enforcement. Using geocoded micro-data from the Afrobarometer, a series of surveys of political attitudes and behaviour in Africa, I find evidence of an erosion of state legitimacy. Respondents living in high-kinship areas under Civil Law exhibit significantly lower trust in courts and formal institutions than their Common Law counterparts living just across the border. No corresponding difference in trust appears among low-kinship respondents. This legitimacy gap is comparable to the difference in institutional trust between Kenya and Nigeria and is approximately 1.6 times the cross-country standard deviation in mean state trust across the 34 African countries surveyed.

Furthermore, I document lower civic engagement: these groups score significantly lower on a composite index of participation in the formal public sphere, being less likely to attend community meetings, join others to raise an issue, or take part in demonstrations (as in

Alesina and Giuliano, 2011). These survey estimates also account for respondents’ age, gender, and education. Lower trust and participation may contribute to the economic penalty, reflect lower incomes, or both. These patterns are consistent with citizens responding to friction between formal rules and local norms by ‘exiting’ the state (Hirschman, 1970; Mamdani, 1996), potentially weakening state capacity and shifting activity toward informal networks (Dixit, 2004; Besley and Persson, 2009).

To assess whether legal practice differs in its treatment of customary institutions, I analyse a new corpus of colonial court cases. I first train separate word-embedding models on decisions from the British Judicial Committee of the Privy Council (Common Law) and the *Revue Juridique du Congo Belge* (Civil Law). The word embeddings measure similarities in how terms are used in the judgments. In the Common Law corpus, terms referring to customary institutions are more closely associated with land and property than in the Civil Law corpus. This pattern is consistent with greater accommodation of customary property claims in the Common Law judgments.

I then use a multilingual text model to compare whether customary institutions are described in terms of legal recognition or rejection. On this measure, references to customary institutions are more strongly associated with language of repugnancy in the Civil Law corpus than in the Common Law corpus, suggesting a less accommodating stance toward indigenous norms. The gap on this textual measure is equivalent to approximately three decades of the secular shift toward greater recognition of customary rights observed over the twentieth century. Together, these analyses document a semantic divergence between the two legal traditions that is in line with the proposed institutional mismatch mechanism.

Because legal origin is bundled with other colonial institutions, I examine Cameroon, where Common Law and Civil Law coexist within a single sovereign state (Lee and Schultz, 2012; Guarnieri and Rainer, 2021). Substantial variation in kinship intensity within both legal regions allows me first to test whether the global mismatch pattern replicates within a common national setting. Prior to the 2016 crisis, high-kinship areas

in the Francophone Civil Law region exhibited significantly lower levels of development than comparable high-kinship areas in the Anglophone Common Law region, while no corresponding legal-system gap appeared among low-kinship societies. This peacetime comparison, however, still bundles legal origin with the distinct French and British colonial administrative legacies inherited by the two regions.

I therefore exploit the onset of the Anglophone Crisis as a modern increase in institutional mismatch within the historically Common Law region. In late 2016, the central government appointed Civil Law judges to Anglophone Common Law courts, introducing a source of legal incompatibility distinct from the region’s inherited British administrative legacy ([Konings and Nyamnjoh, 2003](#); [Pommerolle and Heungoup, 2017](#)). Resistance subsequently took the form of “Ghost Towns”: recurrent, community-enforced shutdowns designed to withdraw cooperation from and delegitimise the state. Following this regional judicial shock, I exploit the locally staggered onset of active conflict in a dynamic difference-in-differences design. I find that economic activity in high-kinship communities fell roughly three times as much as in low-kinship communities. The decline is concentrated in urban areas, where the Ghost Town strategy directly targets commercial activity, and does not appear to be driven by differential exposure to violence. The subsequent economic decline erased the pre-existing advantage of high-kinship Anglophone areas. To test whether kinship generally amplifies the economic costs of conflict, I examine the Boko Haram insurgency in Northern Cameroon and find no comparable high-kinship penalty. Dense kin networks therefore do not appear inherently detrimental during conflict.

These findings have broad implications for development policy. They suggest that the efficacy of institutional reforms depends partly on their compatibility with underlying social structures. Persistent challenges in state-building in the Global South may therefore stem from the imposition of one-size-fits-all structures that lack cultural legitimacy ([Rodrik, 2007](#); [Pritchett et al., 2013](#)). Institutional reform should weigh the costs of incompatibility against the potential costs of preserving customary arrangements.

Related Literature. This paper makes three contributions to the political economy of development. First, I contribute to a longstanding literature on the relationship between social organisation and institutional performance ([Banfield, 1958](#); [Putnam et al., 1993](#)). Research in economics has documented the roles of formal institutions ([North, 1990](#); [Acemoglu et al., 2001](#); [Rodrik et al., 2004](#); [Dell, 2010](#); [Iyer, 2010](#); [Lee and Schultz, 2012](#)) and persistent cultural norms ([Guiso et al., 2006](#); [Nunn and Wantchekon, 2011](#); [Alesina et al., 2013](#); [Michalopoulos and Papaioannou, 2020](#)), while a substantial literature examines their interaction ([Helmke and Levitsky, 2004](#); [Tabellini, 2008](#); [Bisin and Verdier, 2011](#); [Gorodnichenko and Roland, 2017](#); [Lowes et al., 2017](#); [Le Rossignol et al., 2024](#); [McGuirk and Nunn, 2024](#)). I advance this literature by identifying this interaction as a determinant of state legitimacy. My findings provide causal evidence in support of the theoretical insights of [Greif \(1994\)](#); [Greif and Tabellini \(2010\)](#), showing that the developmental effects of national institutions depend in part on their compatibility with local moral economies (cf. [Michalopoulos and Papaioannou, 2014](#); [Godefroy and Lewis, 2025](#)).

Second, I refine the Legal Origins hypothesis by introducing social structure as a scope condition. The classic Legal Origins literature, beginning with [La Porta et al. \(1998, 1999\)](#) and extended by [Glaeser and Shleifer \(2002\)](#), associates Common Law with stronger investor protections, greater judicial independence, and more favourable economic outcomes. I qualify the universality of this view. By documenting a mismatch penalty concentrated in high-kinship societies, I show that the relative performance of the two legal traditions depends on the social structure into which they are transplanted. The relative advantage of Common Law in these settings may lie partly in its adaptability ([Beck et al., 2003](#); [Gennaioli and Shleifer, 2007](#)) and capacity to accommodate customary institutions. This provides a potential explanation for why the relative performance of Civil Law varies across settings (see [Musacchio, 2008](#); [Acemoglu et al., 2011](#); [Bugge, 2016](#)): legal transplantation may be costly where relatively rigid formal rules encounter social structures organised around divergent systems of enforcement ([Djankov et al., 2003](#); [Berkowitz et al., 2003](#); [Platteau, 2009](#)).

Finally, this paper contributes to research on the economic consequences of kinship and its relationship with state capacity. A rapidly growing economic literature documents both the costs of kinship networks for impersonal exchange and state formation and their role in providing insurance and enforcement. Scholars have shown that strong kinship structures foster favouritism and narrow the scope of trust (Moscona et al., 2017; Akbari et al., 2019), concentrate political power and sustain dynastic persistence (Dal Bó et al., 2009; Querubin, 2016; Cruz et al., 2017), exacerbate conflict (Moscona et al., 2020), and limit cooperation beyond the group (Henrich et al., 2010; Enke, 2019; Schulz, 2022). Taken together, this literature suggests that strong kinship structures can crowd out the generalised trust required for impersonal institutions (Alesina and Giuliano, 2011; Schulz et al., 2019).

Henn (2023) shows that the constitutional integration of traditional authorities shapes whether they operate as complements or substitutes to the state. I examine how legal traditions shape the dual role of kinship, which can substitute for formal institutions by providing insurance and enforcement when these institutions fail (Fafchamps, 1992; Coate and Ravallion, 1993; Platteau, 1994; Scott, 2009), but may also complement them when the legal framework accommodates local forms of cooperation. This interpretation suggests that the kinship penalty depends on the institutional environment in which kinship networks operate (Ashraf et al., 2020). The question is when reshaping traditional structures (Aldashev et al., 2012; Bau, 2021) is preferable to designing institutions that can harness them as complements to state capacity (Baldwin, 2016; Bolt and Gardner, 2020; Bolt et al., 2022).

The remainder of the paper proceeds as follows: Section 2 outlines the conceptual framework and previews a simple model of institutional mismatch, which Appendix A formalises; Section 3 details the data construction; Sections 4 and 5 present the causal evidence using nightlights and survey data; Sections 6 and 7 provide complementary tests of the institutional mismatch hypothesis using colonial court cases and the Anglophone Crisis; and Section 8 concludes.

2 Conceptual Framework

This section outlines a theoretical framework for understanding the interaction between formal legal institutions and informal social structures. I suggest that state capacity is not only determined by the intrinsic quality of these institutions, but also by their compatibility. I model this interaction as a strategic choice. The adaptability of the legal code determines whether kinship networks function as a complement to the state, enhancing its legitimacy, or as a substitute, enabling citizens to exit the formal sphere and rely on the lineage for public goods.

2.1 Kinship as an Enforcement Technology

Historically, in environments characterised by high transaction costs and weak central authority, economic cooperation could not rely on third-party enforcement by an impartial state (Landa, 1981; Milgrom et al., 1990; Bernstein, 1992; Dixit, 2004). Instead, societies developed dense kinship networks as a primary technology to mitigate information asymmetries and solve commitment problems (Fafchamps, 1992; Coate and Ravallion, 1993; Greif, 2006; Enke, 2019). By leveraging the credible threat of social ostracism and collective liability, the lineage functions as a second-best institution that enforces contracts and creates reliable social safety nets (Greif, 1993; Fafchamps and Lund, 2003). In these contexts, the clan is a functional economic unit in which the lineage, not the household or the individual, is the primary agent of survival.

This enforcement technology primarily relies on a specific moral economy regarding the structure of property rights. Anthropologists emphasise that land in high-kinship societies is viewed as a communal asset held in trust for the lineage, as opposed to an alienable commodity. For instance, in the *Stool Land* systems of Ghana, land is held in perpetuity for the collective, preventing the emergence of individual property rights and resisting the state's efforts at formal land titling (Goldstein and Udry, 2008; McGuirk and Nunn,

2024; Le Rossignol et al., 2024). This communal structure maximises the group’s retention of assets, but creates friction with legal systems predicated on individual ownership and alienability.

This reliance on reputation-based enforcement creates a radius of particularised trust that coincides with the lineage but often excludes the anonymous state bureaucracy (Henrich et al., 2010; Nunn and Wantchekon, 2011), implying that kinship networks function as a competitor to the state’s monopoly on rule-making and public goods provision. Because the clan provides an effective substitute for state services, ranging from dispute resolution to insurance (see Grimard, 1997; Miguel and Gugerty, 2005), citizens in high-kinship societies possess a viable exit option that atomistic individuals in low-kinship societies lack (Hirschman, 1970).

Following the logic of Scott (2009), the existence of this informal infrastructure lowers the cost of withdrawing from the formal sector. When the state attempts to impose institutions that conflict with local norms (such as privatising communal land), the clan allows society to bypass the formal sphere entirely, retreating into an informal sector where the state’s fiscal and legal capacity cannot reach. A classic manifestation of this is the ‘uncaptured peasantry’ described by Hydén (1980) in Tanzania. When the post-colonial state imposed the *Ujamaa* villagisation policy (i.e., a rigid formal structure that contradicted local land-tenure customs), rural communities leveraged their ‘economy of affection’ (kinship networks) to revert to subsistence and informal exchange, neutralising the state’s attempt at social engineering.

2.2 Legal Origins and Institutional Mismatch

Formal legal systems face a trade-off between legal certainty (minimising judicial discretion through top-down rules) and adaptability (the capacity to fit law to local context). While the foundational Legal Origins literature has largely attributed the economic advantages of Common Law to stronger investor protections and judicial independence (La Porta et al., 1998; Glaeser and Shleifer, 2002), this framework highlights a different dimension:

institutional porosity, or the capacity of the legal system to absorb pre-existing informal norms.

The Civil Law tradition, particularly in its French colonial application, is an archetype of logical ‘formal rationality’: a constructed order designed to reconstruct society from the top down (Weber, 1922; Hayek, 1973). By asserting a strict monopoly on rule-making, the Code renders the negotiated, reputation-based norms of the clan illegible and legally void. The friction is most acute for lineage property, as illustrated by the *Affaire Dipoko* (1983) in Cameroon. In this dispute over ancestral land, the court strictly applied Article 815 of the Napoleonic Code,² which prioritises individual exit rights over collective ownership. Since the communal land could not be physically partitioned among the numerous heirs without destroying its economic viability, the court ordered the sale of the ancestral estate to outsiders.

By prioritising the atomistic property rights of the individual over the collective preservation of the lineage, the ruling dismantled the community’s economic base. In the language of Akerlof and Kranton (2000), Civil Law thus imposes an ‘identity externality’: compliance with the state requires the violation of deep-rooted moral norms and the destruction of the social self. In such an environment, the state transforms from a neutral arbiter into an adversary, potentially incentivising agents to reject the formal institution entirely.

In contrast, the Common Law tradition possesses specific doctrinal mechanisms that allow it to absorb the ‘substantive rationality’ of the clan. The reliance on case law allows for a dynamic evolution where judges can distinguish precedents to accommodate local realities (Beck et al., 2003; Gennaioli and Shleifer, 2007). This flexibility was formalised through Repugnancy Clauses, which mandated courts to enforce local customary law provided it was not “repugnant to natural justice, equity, and good conscience.”³ This legal technology acts

²Article 815 of the Code Civil (1804): “*Nul ne peut être contraint à demeurer dans l’indivision et le partage peut être toujours provoqué*” (No one can be forced to remain in co-ownership, and partition may always be initiated).

³See, e.g., Section 19 of the Supreme Court Ordinance No. 4 of 1876 (Nigeria). For a comprehensive

as a filter, allowing the kinship network to enter the formal canon. A seminal example is the Privy Council’s ruling against the colonial government’s claim to “unoccupied” land in *Amodu Tijani v. Secretary of Southern Nigeria* (1921). Explicitly rejecting the imposition of English *fee simple* rights, the court held that native title must be understood on its own communal terms. By validating the moral economy of the clan instead of erasing it, the Common Law lowers the cost of participation, allowing the lineage to view the state as a complement rather than a substitute.

It is useful at this point to distinguish this mechanism from the bifurcated state critique of Mamdani (1996), who argued that British Indirect Rule created a decentralised despotism by empowering unaccountable chiefs. Mamdani’s critique identifies a genuine democratic deficit, as Indirect Rule often entrenched patriarchal authority and shielded local elites from popular accountability. However, the relevant counterfactual in high-kinship societies is not liberal democracy but the exclusionary rigidity of the Civil Code. By offering a bridge, however narrow, between the citizen and the subject, Common Law’s legal porosity prevented the total alienation and dual legalism observed under the Code (see Platteau, 2009).

2.3 Strategic Exit and State Capacity

I model the interaction between the informal moral economy of kinship and the formal architecture of the state as a choice between ‘Voice’ and ‘Exit’ (Hirschman, 1970). The institutional mismatch hypothesis posits that state legitimacy is not intrinsic, but conditional on compatibility. When rigid institutions, epitomised by the Civil Law, are imposed upon societies organised around reputation-based kinship networks, the state transforms from a complement into a competitor. Moreover, by rendering the complex, opaque web of kinship obligations ‘legible’ and subordinate to central command (Scott, 1998), the Civil Law system may delegitimise indigenous conflict resolution mechanisms without effectively replacing them.

review of the judicial application of this doctrine, see Allott (1970) and Fullerton Joireman (2006).

This institutional clash can generate two distinct equilibrium outcomes. Under Common Law, the porosity of the Repugnancy Clause allows customary norms to be absorbed into the formal canon. This accommodation lowers the cost of compliance, sustaining a cooperative equilibrium where the state is viewed as a neutral arbiter. Conversely, the rigidity of the Civil Law marginalises high-kinship societies. Facing a state that criminalises their moral obligations (e.g., regarding lineage land), such high-kinship groups strategically withdraw from the formal sphere. Following [Scott \(2009\)](#), these citizens choose to bypass the state entirely, retreating into the ‘illegible’ informal economy where the clan provides public goods and protection.

This strategic withdrawal generates a self-reinforcing trap of low state capacity. By rendering their economic activities invisible to avoid a hostile legal system, high-kinship groups cap the state’s fiscal capacity to tax and its legal capacity to enforce contracts ([Besley and Persson, 2009](#)). When the state cannot observe or regulate the clan, it cannot extract the resources necessary to provide public goods ([Mayshar et al., 2017](#)), further diminishing its utility relative to the kinship network. As [Kranton \(1996\)](#) demonstrates, this dynamic is self-sustaining: the thinning of the formal sector increases the relative return to personalised exchange, locking the society into a low-level equilibrium. The ‘kinship penalty’ therefore reflects a structural failure of the state to establish the fiscal and legal monopoly required for development.

2.4 A Simple Model: Overview

I formalise this mechanism in a stylised contract-theory framework ([Hart, 1995](#)), presented in full in [Appendix A](#). The central idea is that rigid legal institutions can generate a mismatch when ancestral assets are embedded in lineage obligations that formal courts cannot fully process. Following the incomplete-contracts literature ([Grossman and Hart, 1986](#); [Hart and Moore, 1990](#)), I treat the legal regime as a technology for allocating control rights when customary claims are only imperfectly legible to formal courts.

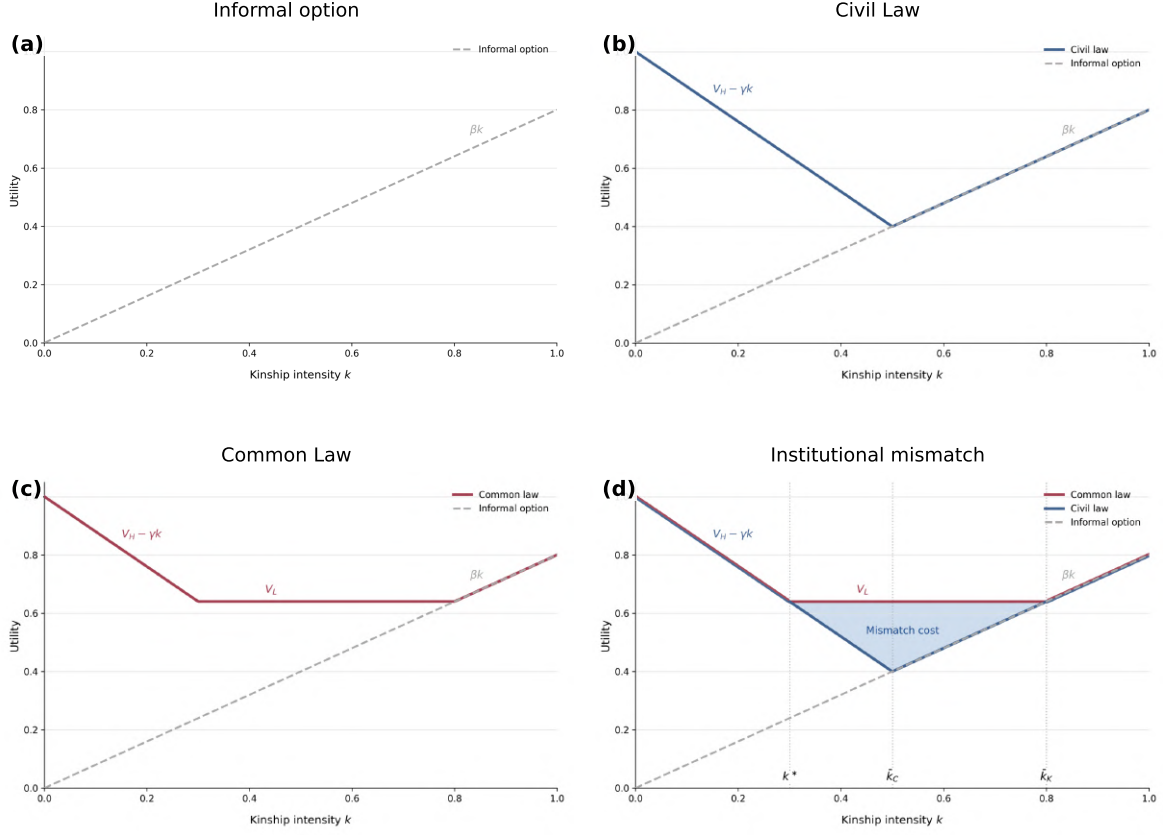


Figure 1: Building the Micro-Foundation

Notes. The figure builds the mechanism in four steps. Panel (a) introduces the informal outside option. Panel (b) adds the Civil-Law liquidation branch, under which formal utility declines with kinship because liquidation destroys lineage value. Panel (c) adds the Common-Law preservation branch, which keeps high-kinship agents in the formal sector at the preserved value V_L . Panel (d) combines these elements and shows the resulting institutional-mismatch wedge.

Figure 1 previews the model graphically. The outside option rises with kinship because lineage networks provide insurance and protection in the informal sector. Civil Law keeps the formal contract on the liquidation branch. Common Law instead preserves the asset once customary claims become sufficiently important, scaling with kinship intensity. Institutional mismatch emerges where rigid liquidation remains binding under Civil Law while Common Law preserves lineage assets and sustains formal participation until kinship intensity becomes very high. Appendix A first sets out the formal model, then its calibration to the empirical estimates of the paper, and ends with a general equilibrium extension.

3 Data and Measurement

To estimate the interaction between legal institutions and kinship norms, I combine spatial data from multiple primary sources: ethnographic maps, satellite imagery, as well as geocoded survey and conflict data. To harmonise these different data types, I construct a uniform global grid of $0.125^\circ \times 0.125^\circ$ cells (approximately $14 \text{ km} \times 14 \text{ km}$ at the equator), which serves as the unit of observation throughout the analysis. This spatial resolution is standard in the literature, and is selected to maximise the precision of the satellite nightlights data while maintaining computational tractability for the global sample. Finally, in order to provide further evidence for the historical existence of institutional mismatch, I construct a novel corpus of colonial court cases.

3.1 Kinship and Legal Origins

To test the hypothesis that cultural mismatch is driven by the incompatibility between formal rules and informal social structures, I require a measure of deep-rooted kinship norms that predates the imposition of modern legal codes. I follow the recent literature on the origins of psychological variation and institutional development by employing the Kinship Intensity Index (KII), a composite measure of social organisation originally formalised by [Schulz et al. \(2019\)](#) and extended by [Bahrami-Rad et al. \(2022\)](#), comparable in scope to [Enke \(2019\)](#). This measure captures the extent to which a society is organised around dense, interdependent networks as opposed to atomistic nuclear families. Specifically, the index synthesises five distinct pre-industrial traits rooted in ethnography, documented in the Ethnographic Atlas described below: the preference for cousin marriage, the presence of polygamy, the co-residence of adult children with parents, the existence of segmented lineage organisations, and the structure of local communities.

The primary source for this data is the Ethnographic Atlas (EA, [Murdock, 1967](#)), which provides pre-industrial ethnographic characteristics for 1,265 ethnic groups. To map

these historical traits to contemporary spatial data, I follow the procedure established by [Giuliano and Nunn \(2018\)](#), linking ethnic groups from the Atlas to language groups of the Ethnologue 16 database based on linguistic proximity. This procedure achieves good coverage: drawing on the KII values of 1,131 (out of 1,265) unique ethnic groups in the EA, I assign kinship scores to the ancestral homelands of 7,063 (out of 7,510) language groups mapped in Ethnologue 16. While this approach assigns traits based on ancestral territory, evidence confirms a high correlation between ancestral and contemporary inhabitants, validating the use of the Ethnographic Atlas for modern spatial analysis ([Bahrami-Rad et al., 2021](#)).

The Kinship Intensity Index is constructed as the first principal component of five key variables from the Ethnographic Atlas that capture the tightness of social organisation:

1. *Cousin Marriage Preferences*: Whether the society permits or prefers marriage between cousins, a practice that reinforces closed lineage networks.
2. *Polygamy*: The presence of polygynous marriage arrangements, which creates large, interdependent family units.
3. *Co-residence of Families*: Whether adult children reside with their parents (extended families) versus establishing independent households (nuclear families).
4. *Lineage Organisation*: The structure of inheritance, leading to the presence of localised clans or large lineage groups versus atomistic family structures.
5. *Community Organisation*: The structure of the local community or neighbourhood, specifically whether it is segmented into clan barrios.

These five components are highly correlated, reflecting a single underlying latent trait of social tightness. As reported in Appendix Table [B.3](#), a Principal Component Analysis (PCA) reveals that the first component alone explains 56% of the total variance, with all five variables loading positively (ranging from 0.33 to 0.54), confirming that these distinct practices reflect a coherent underlying dimension of social tightness. The internal consistency of the index is high as well, with a Cronbach’s alpha of 0.80.

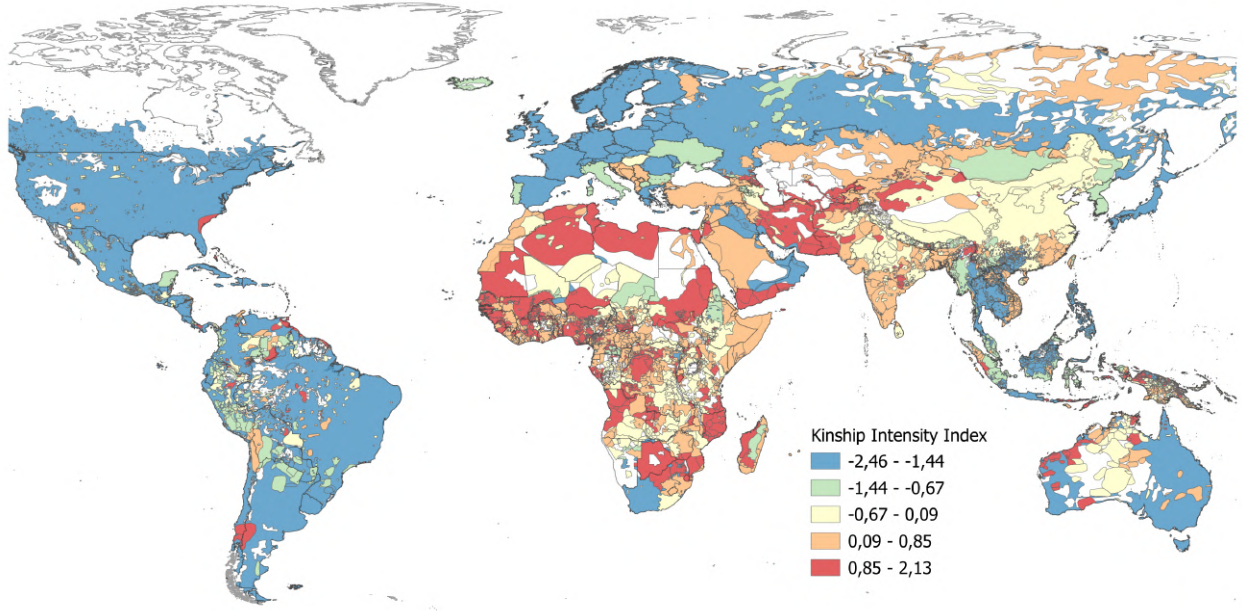


Figure 2: Global Distribution of Historical Kinship Intensity

Notes. The map displays the global variation in the Kinship Intensity Index (KII) across 1,131 ethnic groups. Darker red indicates tighter kinship norms (e.g., cousin marriage preferences, extended clans), while blue indicates loose kinship structures (nuclear families).

For the analysis, I follow [Schulz et al. \(2019\)](#) and first standardise all five subcomponents before calculating the KII, giving each dimension of kinship equal weight. I then standardise the index itself to have a mean of zero and a standard deviation of one across the global sample. Higher values of the KII denote tight kinship societies characterised by strong in-group loyalty and extended clan networks. Lower values denote loose kinship societies characterised by nuclear families and weaker extended ties.

Figure 2 illustrates the global distribution of kinship intensity. A visual inspection confirms that while kinship intensity is generally higher in the Global South, there is substantial variation even within regions sharing similar levels of economic development, providing the necessary variation to identify the interaction with legal origins.

3.2 Economic Activity

To measure economic activity at the sub-national level, where granular official income accounts are either non-existent or unreliable, I rely on satellite data of nighttime luminosity. This approach has become the standard in comparative development economics for measuring local economic activity and living standards ([Chen and Nordhaus, 2011](#); [Henderson et al., 2012](#); [Donaldson and Storeygard, 2016](#)).

I employ data from the Visible Infrared Imaging Radiometer Suite (VIIRS), available from 2012-2024. Unlike older DMSP-OLS series, which suffer from saturation in urban centres and bottom-coding in rural areas, VIIRS offers superior radiometric resolution and onboard calibration for detecting low-level economic activity in the rural Global South. I aggregate the raw satellite imagery to the standardised 0.125° grid-cell level to match the unit of observation and to reduce measurement error from high-frequency background noise.

To capture the full spectrum of economic development across the varying income levels of the Global South, I construct two distinct outcome variables. First, the Intensive Margin is calculated as the natural logarithm of the average light density in a grid cell, defined as $\ln(\text{light}_i + 0.00001)$, to account for cells with zero recorded light.⁴ This measure captures the intensity of economic agglomeration and infrastructure density ([Michalopoulos and Papaioannou, 2013](#)). Second, the Extensive Margin is constructed as a binary indicator equal to one if a grid cell emits any detectable light, and zero otherwise. As argued by [Keola et al. \(2015\)](#) and [Bruederle and Hodler \(2018\)](#), the extensive margin is often a more precise proxy for development in poorer, rural, or sparsely populated regions where variation in light intensity may be indistinguishable from background noise.

The use of nighttime lights as a proxy for development in this context is well-validated. [Michalopoulos and Papaioannou \(2014\)](#) and [Hodler and Raschky \(2014\)](#) demonstrate that

⁴I do not use the inverse hyperbolic sine transformation, $\text{asinh}(\text{light}_i)$, because a large share of positive luminosity observations lies between zero and one. Over this range, the inverse hyperbolic sine is approximately linear, $\text{asinh}(x) \approx x$, and therefore provides less compression of the right-skewed distribution than the logarithmic transformation.

luminosity correlates strongly with survey-based measures of wealth, public goods provision, and population density across African sub-national regions. By utilising both margins, I ensure that my results are not driven solely by urbanisation or the presence of extractive industries, and that they reflect broader patterns of economic activity.

3.3 Mechanism Data

To move beyond the economic consequences and investigate potential mechanisms, I employ two distinct types of micro-data that capture individual attitudes and local dynamics.

Afrobarometer. First, I use individual-level survey data from Round 8 of the Afrobarometer survey, covering 34 African countries between 2019 and 2021. I leverage the sub-nationally geocoded nature of the survey clusters, which allows for precise linkage between respondent attitudes and their specific KII and Legal Origin environment.

The outcomes capture two dimensions: state legitimacy, measured by an individual’s trust in formal state institutions (e.g., courts, tax office, police, ...), and civic engagement, measured by participation in formal processes such as demonstrations and willingness to contact local government. By relating these outcomes to the respondents’ historical KII and their Legal Origin environment, I can test if the negative effect of kinship on institutional trust and civic engagement is unique to Civil Law regions, thereby supporting the institutional mismatch hypothesis.

Conflict. Second, to validate the theoretical mechanism of strategic withdrawal, and to deal with colonial strategy as an alternative explanation for my previous findings, I construct a high-resolution panel of conflict dynamics using data from the Armed Conflict Location & Event Data Project (ACLED). This panel spans the period 2013–2024.⁵ For this specific validation exercise, I refine the unit of observation to a $0.05^\circ \times 0.05^\circ$ grid (approximately 5.5 km $\times$ 5.5 km in Cameroon). This finer resolution is adopted to better capture hyper-local

⁵Although the VIIRS series begins in mid-2012, I exclude the first, partial year of coverage.

variation in strike enforcement within towns and villages. I assign each grid cell to either the Anglophone (Common Law) or Francophone (Civil Law) region based on the 1961 Federal Republic of Cameroon administrative boundaries, creating a precise spatial identifier for the institutional treatment.

To disentangle the specific Ghost Town mechanism from general instability, I parse the daily ACLED event logs to construct distinct conflict indicators aggregated to the grid-cell-year level. First, I isolate events specifically related to the Anglophone Crisis by filtering for known separatist actors (e.g., “Ambazonia Defence Forces,” “Red Dragons”) and contrast these with events involving Boko Haram and Islamic State insurgents in the Northern region. The full list of actors classified as “Anglophone Crisis” or “Boko Haram” is provided in Appendix [B.3](#).

Finally, I leverage the specific event categories to distinguish between “Violence against Civilians” and “Battles/Explosions.” This classification allows for a precise falsification test: if the mechanism is indeed strategic withdrawal enforced by kinship networks, I should observe economic collapse specifically following violence against civilians (a proxy for coercion and strike enforcement) in the Anglophone Crisis, but not necessarily following general kinetic warfare or in the Boko Haram conflict.

3.4 Sample Construction

To construct the analysis dataset, I assign variable values to the universal grid defined above using a spatial join procedure based on cell centroids. Specifically, each cell is assigned the KII value of the ethnic homeland and the legal tradition (Civil vs. Common Law) of the contemporary nation-state that contains its centroid, while topographic and climatic variables are first averaged at the grid cell level before being assigned to its centroid.

To ensure that the dataset reflects viable economic activity, I restrict the universal grid to populated land masses. Specifically, I only include grid cells that overlap with the Ethnologue’s ethnolinguistic regions, and I exclude cells with zero recorded population in

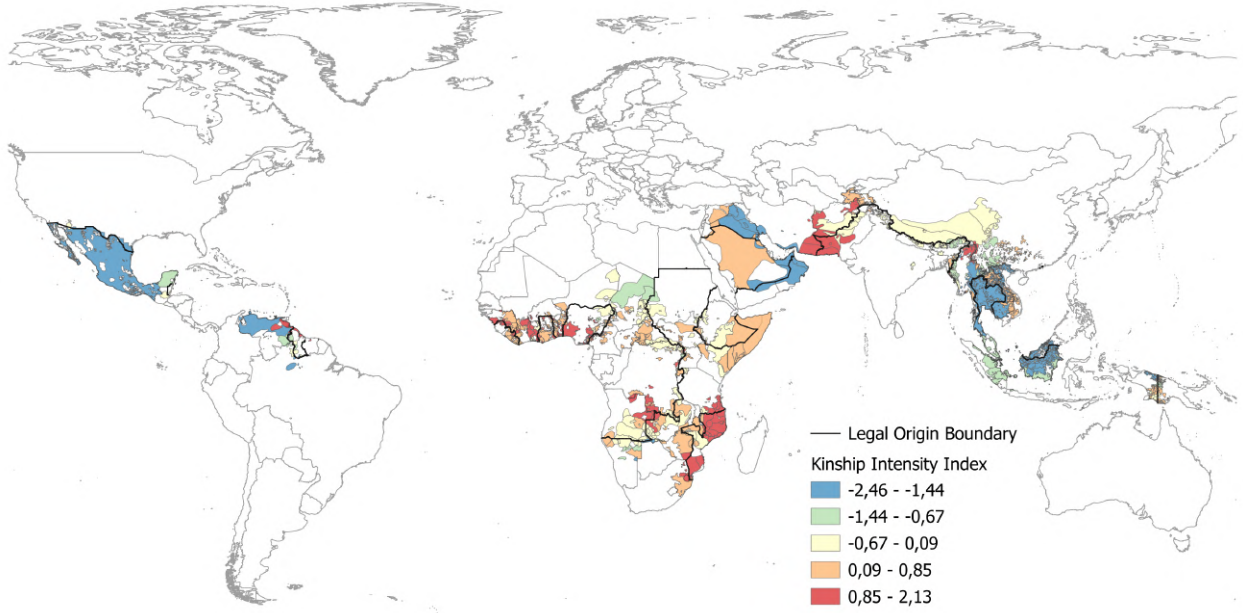


Figure 3: Global Ethnic Groups Partitioned by Legal Origin

Notes. The map illustrates the spatial Regression Discontinuity Design (RDD). The sample is strictly limited to partitioned ethnic homelands that exist on both sides of national borders separating Civil Law and Common Law jurisdictions (within a 200 km bandwidth). Black lines indicate the legal boundaries.

the LandScan database. This cleaned global grid ($N = 722,926$) is the basis for the global OLS benchmark. The estimation sample is slightly smaller ($N = 701,235$) once cells with missing kinship or control values are dropped. Summary statistics are provided in Table 1.

I further impose three restrictions to construct the primary spatial regression discontinuity design sample. First, I restrict the sample to partitioned ethnicities: groups whose ancestral homeland is bisected by a contemporary national border. Second, I retain only those partitions where the border separates a Civil Law jurisdiction from a Common Law jurisdiction, excluding borders between identical legal systems (e.g., Civil-Civil). Third, I restrict the sample to grid cells located within 200 km of the boundary to improve geographic comparability. The resulting dataset consists of 78,266 grid cells covering 1,956 partitioned linguistic homelands, of which 1,206 enter the final cross-border estimation sample, creating a set of spatially adjacent counterfactuals subject to different legal regimes.

Table 1: Global Summary Statistics by Legal Origin

Variable	(1) Full Sample Mean/SD	(2) Civil Law Mean/SD	(3) Common Law Mean/SD	(4) Difference (2)-(3)
Panel A: Economic Outcomes				
Probability of Light	0.60 (0.49)	0.58 (0.49)	0.65 (0.48)	-0.044 [0.072]
Log Nighttime Luminosity	-7.25 (5.77)	-7.51 (5.74)	-6.60 (5.81)	-0.684 [0.885]
Panel B: Institutional Inputs				
Kinship Intensity Index	-0.45 (0.77)	-0.43 (0.76)	-0.51 (0.78)	0.088 [0.238]
Jurisdictional Hierarchy	3.63 (1.30)	3.78 (1.31)	3.25 (1.21)	0.532** [0.229]
Panel C: Geographic Controls				
Absolute Latitude	33.18 (19.08)	34.19 (20.32)	30.59 (15.18)	4.308 [6.375]
Elevation (m)	633.5 (776.0)	644.6 (837.2)	605.0 (590.1)	43.9 [137.1]
Ruggedness Index	92.6 (136.8)	96.2 (137.1)	83.4 (135.7)	15.4 [14.8]
Distance to Coast (km)	679.45 (613.40)	725.13 (654.31)	562.65 (474.01)	159.052 [104.163]
Distance to River (km)	154.4 (253.6)	155.5 (256.4)	151.6 (246.1)	-7.3 [33.8]
Temperature (°C)	13.61 (11.75)	12.44 (12.32)	16.62 (9.50)	-4.721 [4.215]
Precipitation (mm)	779.5 (702.9)	780.4 (730.9)	777.3 (625.5)	24.8 [129.8]
Agricultural Suitability	0.96 (0.08)	0.96 (0.09)	0.97 (0.04)	-0.012*** [0.004]
Malaria Suitability	2.46 (5.83)	2.40 (5.59)	2.60 (6.40)	-0.263 [1.419]
Observations (Grid Cells)	722,926	519,681	203,245	722,926

Notes. The table reports summary statistics for the global grid-cell sample used in the OLS estimation. Columns 1-3 report means with standard deviations in parentheses. Column 4 reports the difference in means between Civil Law and Common Law jurisdictions, estimated via OLS with standard errors clustered two-way at the country and ethnolinguistic family level reported in brackets. For Kinship and Hierarchy, clustered SEs are at the country level due to collinearity with ethnic clusters. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3 presents the universe of partitioned ethnic homelands before applying the 200 km bandwidth restriction. Homelands extending deep into the interior (visible, for instance, in Mexico or China) are subsequently trimmed to the 200 km border region.

Data Limitations and Validity. I acknowledge three limitations inherent to this spatial data construction. First, the Ethnographic Atlas represents historical norms that may have evolved. The verification of this source in Bahrami-Rad et al. (2021) has recently mitigated this concern. Moreover, the persistence of these traits is part of the mechanism under investigation. Second, while I assign traits based on ancestral homelands, substantial migration could dilute these effects. As shown by Bau (2021), this likely introduces classical measurement error that biases the estimates downward, suggesting that my results are conservative. Third, nighttime lights are an imperfect proxy for informal economic activity. However, as noted by Storeygard (2016), they remain the only viable sub-national measure for a global sample, and the use of the *Probability of Light* (extensive margin) mitigates concerns about detecting low-level rural activity.

3.5 Colonial Jurisprudence

To quantify the institutional stance of the colonial state, I construct a novel text corpus representing the highest level of judicial review in both legal traditions. Constructing this dataset required overcoming an archival challenge: while Common Law relies on centralised case repositories, Civil Law usually relies on diffuse administrative doctrine. The Belgian Congo’s annual volumes on colonial jurisprudence provide a rare exception. By restricting the analysis to the highest legal authority available in each colony, I maximise institutional comparability.

For Common Law, I digitise the complete records of the *Judicial Committee of the Privy Council* (JCPC) restricted to Sub-Saharan Africa (1890–1974). This amounts to $N = 167$ decisions at the apex of appellate review. As the final court of appeal for the British Empire,

the JCPC established the binding precedents regarding native title that lower courts were legally obliged to follow. For Civil Law, I digitise the *Revue Juridique du Congo Belge* (RJCB, 1924–1963). Unlike the British system, the Belgian colonial administration did not rely solely on judicial precedent but on a hybrid of jurisprudence (court rulings) and doctrine (authoritative interpretations by legal scholars and administrators). The RJCB served as the primary vehicle for this doctrine, making it the functional equivalent of the JCPC in terms of defining the interpretation of the *Code Civil*.

I use optical character recognition (OCR) on the raw archival material comprising over 25,000 pages of scanned text. I normalise both sources structurally: I reconstruct the British corpus, originally digitised as separate documents and pages within cases (e.g., appellant case, respondent case, record of proceedings, judgment), into unified case files. I segment the Belgian corpus, preserved as large agglomerated annual volumes, into $N = 1,448$ cases using a regex-based extraction of French legal headers (e.g., *Arrêt*, *Tribunal*, *Jugement*) to isolate individual court cases. This yields a final unified dataset of $N = 1,615$ standardised legal cases, tagged by year and legal origin. As shown in Appendix Figure B.1, the two corpora overlap in time from 1924 to 1963. See Appendix B.2 for the complete corpus data cleaning details.

4 Global Evidence: The Mismatch Penalty

4.1 Cross-Sectional Evidence

In Section 2, I hypothesised that the rigidity of Civil Law systems may alienate customary norms. I now test whether such institutional mismatch can generate real economic costs. If the theory holds, I should observe a development penalty specifically where these rigid laws intersect with high-kinship societies. I begin by documenting a stylised fact: the negative relationship between kinship intensity and economic development indeed appears conditional on the legal institutional environment. While prior literature establishes that tight kinship

networks generally impede development by fostering particularised trust (Enke, 2019; Schulz, 2022), I examine whether this penalty persists when these ethnic groups are governed by legal systems capable of accommodating such norms.

I estimate the following OLS specification across a global sample of 701,235 grid cells:

$$y_{i,j,c} = \alpha + \beta_1 \text{Kinship}_i + \beta_2 (\text{Kinship}_i \times \text{Common Law}_c) + \mathbf{X}'_{i,j,c} \gamma + \delta_c + \varepsilon_{i,j,c} \quad (1)$$

where $y_{i,j,c}$ denotes the presence of light for ethnolinguistic homeland i in grid cell j in country c , and Kinship_i is the standardised Kinship Intensity Index (KII). The vector $\mathbf{X}_{i,j,c}$ comprises the set of geographic and historical controls detailed in Table 2, including population density and pre-colonial centralisation.

The OLS estimation includes country fixed effects (δ_c) in all specifications. These absorb all time-invariant national characteristics, including the direct effect of legal origin and national institutional quality. Consequently, β_2 identifies the differential association between kinship and economic activity under Common Law relative to Civil Law, exploiting only within-country variation in social structure.

Table 2 reports the OLS estimates. Column 1 (Panel A) confirms the standard negative correlation found in the literature. In the aggregate sample, higher Kinship Intensity is associated with significantly lower economic activity. This replicates the baseline finding in Bahrami-Rad et al. (2022). Panel B documents significant heterogeneity by legal origin. The negative association seems to be driven entirely by Civil Law jurisdictions. In contrast, the interaction term for Common Law is positive throughout and statistically significant once geographic and population controls are included (Columns 2 and 3), offsetting the negative baseline effect.

The marginal effects reported at the bottom of Table 2 quantify the net relationship. Under Common Law, the slope on kinship is statistically indistinguishable from zero in the baseline, is essentially zero under the full set of controls (-0.003 , Column 2), and turns

Table 2: Kinship Intensity and Economic Activity by Legal Origin

<i>Dependent Variable: Probability of Light</i>			
	(1) Baseline	(2) Controls	(3) Hierarchy
Panel A: Aggregate Relationship			
Kinship Intensity	-0.084** (0.033)	-0.031*** (0.008)	-0.021* (0.011)
Panel B: Split by Legal Origin			
Kinship Intensity	-0.100*** (0.033)	-0.041*** (0.007)	-0.032*** (0.010)
Common Law $\times$ Kinship	0.064 (0.043)	0.037*** (0.014)	0.042*** (0.012)
Jurisdictional Hierarchy			0.017*** (0.004)
<i>Marginal Effects of Kinship:</i>			
Slope: Civil Law	-0.100	-0.041	-0.032
Slope: Common Law	-0.036	-0.003	0.010
Observations	701,235	684,926	659,246
Clusters	119	113	103
Adj. R-Squared	0.288	0.540	0.540
Country FEs	✓	✓	✓
Geographic Controls		✓	✓
Population Controls		✓	✓

Notes. The table reports OLS estimates of the relationship between Kinship Intensity and Probability of Light. Panel A reports the average effect; Panel B allows the effect to vary by legal origin. The unit of observation is a $0.125^\circ \times 0.125^\circ$ grid cell. All models include country fixed effects (which absorb the time-invariant main effect of Legal Origin). Controls include agricultural suitability, malaria, distance to coast/rivers, temperature, rainfall, ruggedness, elevation, absolute latitude, and population density. Column 3 adds Jurisdictional Hierarchy, a measure of pre-colonial centralisation from the Ethnographic Atlas (Murdock, 1967). Kinship Intensity is an index composed of cousin marriage preferences, polygamy, co-residence of families, lineage organisation, and community organisation (See Bahrami-Rad et al., 2022). Standard errors are two-way clustered at the ethnolinguistic family and country levels and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

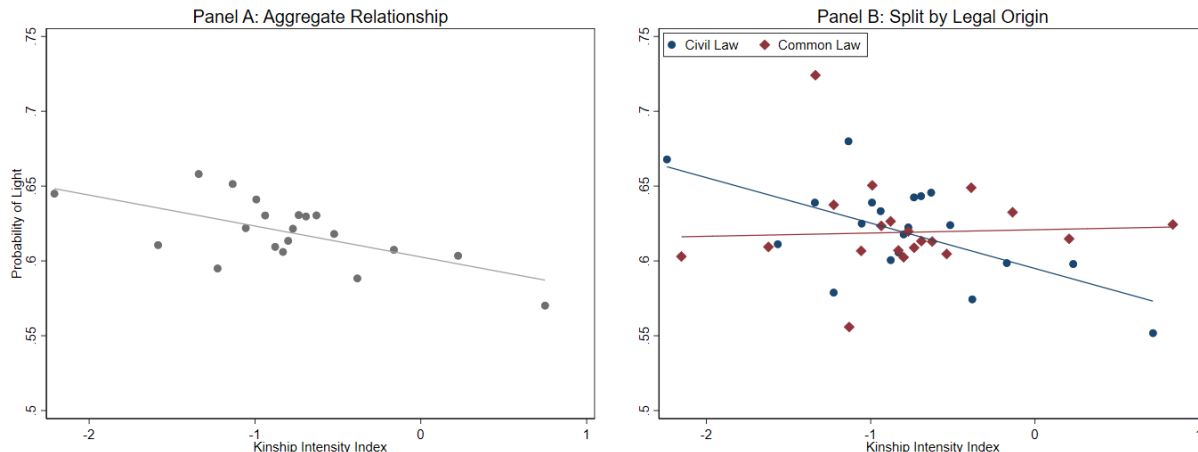


Figure 4: Relationship between Kinship Intensity and Economic Activity by Legal Origin

Notes. The figure presents binned scatterplots of the residualised Probability of Light against the Kinship Intensity Index after absorbing country fixed effects. Panel A plots the relationship for the full global sample ($N = 1,131$ ethnic homelands). Panel B plots the relationship split by legal origin: Civil Law jurisdictions (blue circles) and Common Law jurisdictions (red diamonds). Linear fit lines are displayed for each subsample. All specifications include country fixed effects.

positive when conditioning on jurisdictional hierarchy (0.010, Column 3). The latter control ensures that the estimated interaction is distinct from the documented channel of pre-colonial state centralisation ([Michalopoulos and Papaioannou, 2013](#)). Moreover, the negative gradient in Civil Law jurisdictions remains robust and statistically significant across all specifications, consistent with an institutional mismatch specific to the legal architecture.

Figure 4 visualises the residualised conditional correlations from equation 1. Panel A displays the aggregate within-country relationship. Panel B decomposes this slope by legal origin, plotting the partial correlation between kinship and nightlights after absorbing country fixed effects. The slope for Civil Law (blue circles) is negative, while the slope for Common Law (red diamonds) is flat. These results indicate that the kinship penalty depends on the institutional environment.

While suggestive, these estimates rely on cross-sectional variation that may be confounded by unobservable historical or geographic factors correlated with kinship and

legal origins. To isolate the causal effect of the institutional interaction, I proceed with a spatial regression discontinuity design.

4.2 Identification Strategy

To isolate the causal impact of institutional mismatch, I employ a spatial Regression Discontinuity Design (RDD) across national borders. This strategy addresses the concern that legal origins and kinship norms may correlate with unobservable historical or geographic confounders in the cross-sectional OLS estimation.

Central to this empirical strategy is the premise that the intersection of legal and cultural spheres is largely accidental. I exploit legal origins imposed by colonial powers, often without regard for local social structures (La Porta et al., 2008), while treating kinship intensity as a deep-rooted, slowly evolving trait (Enke, 2019). To isolate the causal impact of this mismatch, I focus on partitioned ethnicities (Michalopoulos and Papaioannou, 2013, 2014): ethnolinguistic homelands bisected by national borders that separate a Civil Law jurisdiction (Treatment) from a Common Law jurisdiction (Control), thereby holding cultural factors constant while varying the institutional environment.

Following Dell (2010) and Keele and Titiunik (2015), I use geodesic distance to the legal border as the running variable. The core identification assumption is that treatment assignment is locally randomised, conditional on distance to the border. While institutions change discontinuously at the boundary, pre-determined factors such as geography, climate, and pre-colonial history should vary smoothly. I restrict the full sample to grid cells within 200 km of the boundary to ensure broader geographic comparability.

Sample Selection and Balance. The validity of the spatial RDD relies on the assumption that potential outcomes vary smoothly at the border in the absence of treatment. I first verify the continuity of the key interaction variable. Figure 5 demonstrates that Kinship Intensity varies smoothly across the legal boundary. This supports the view that the design isolates an

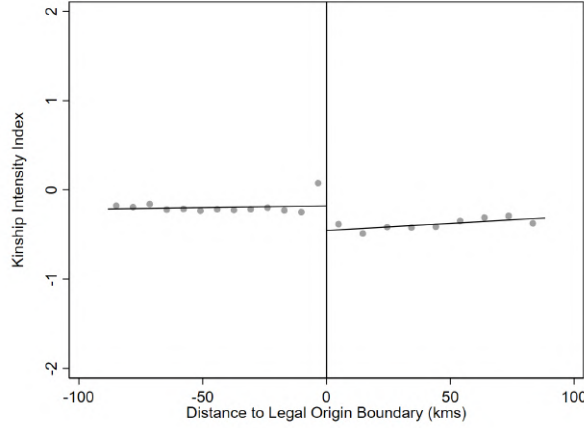


Figure 5: Kinship Intensity across the Legal Origin Boundary

Notes. The figure displays a binned scatterplot of the standardised Kinship Intensity Index against the distance to the border separating Civil Law and Common Law jurisdictions. The sample includes partitioned ethnic homelands within 200 km of the boundary. The solid lines represent local linear polynomial fits estimated separately on each side of the cutoff. The estimated discontinuity at the border is statistically indistinguishable from zero ($p = 0.47$).

institutional shock while holding social structure locally constant, and addresses the concern that legal borders proxy for cultural discontinuities.

A second threat to identification is the coincidence of legal borders with major geographic barriers. Table 3 reports balance tests for geographic covariates. For the High Kinship sample (Columns 2 and 4), the legal boundary separates populations in statistically indistinguishable geographic environments. For the population of interest, the border therefore appears to randomise institutional assignment conditional on location.

In contrast, the unrestricted Low Kinship sample exhibits large discontinuities (Column 1). The Civil Law side is significantly higher in elevation (+1,640 metres) and more rugged than the Common Law side. This imbalance is largely driven by the China-India border, which traces the Tibetan Plateau. Because this first-order topographic discontinuity violates the smoothness assumption, I restrict the baseline sample to exclude this border. As reported in Column 3, this restriction improves balance on the first-order topographic dimensions: the difference in elevation drops to a statistically insignificant 440 metres, and the difference in ruggedness vanishes. Differences in river distance and temperature remain, which the

Table 3: Balance of Covariates across the Legal Boundary

Dependent Variable	Full Sample		Restricted Sample	
	(1) Low Kinship	(2) High Kinship	(3) Low Kinship	(4) High Kinship
Panel A: Fundamental Geography				
Elevation	−1640.895 ^{***} (610.899)	63.198 (131.594)	−440.650 (276.899)	41.057 (137.452)
Ruggedness	−144.521 ^{**} (70.324)	24.243 (23.546)	−2.256 (29.875)	22.992 (25.317)
Distance to Coast	−307.447 ^{***} (109.890)	20.777 (90.071)	−164.838 (127.277)	18.302 (88.113)
Distance to River	346.066 ^{***} (127.107)	41.790 (35.880)	284.346 ^{**} (131.505)	47.365 (38.561)
Panel B: Ecology & Climate				
Temperature	11.567 ^{***} (3.901)	−0.723 (0.976)	3.787 ^{**} (1.804)	−0.587 (1.007)
Precipitation	−59.708 (210.610)	−187.301 (205.828)	−220.830 (224.907)	−186.907 (221.158)
Soil Suitability	−0.010 ^{**} (0.005)	−0.002 (0.004)	−0.008 (0.005)	−0.002 (0.004)
Malaria Suitability	0.385 (1.258)	−1.723 (2.369)	−1.372 (1.413)	−1.722 (2.537)
Avg. Observations	17,909	17,871	13,911	16,337
Avg. Optimal Bandwidth	65.58	80.59	55.40	81.51
Country Pair FEs	✓	✓	✓	✓

Notes. The table reports spatial RD estimates of the difference in geographic characteristics at the legal boundary. The running variable is distance (km) to the Civil–Common Law legal border, with negative values denoting the Civil Law side. Columns 1 and 2 report results for the Full Sample. Columns 3 and 4 report results for the Restricted Sample, which excludes China to improve geographic comparability. Each set is split by the median Kinship Intensity Index. Each cell reports the bias-corrected robust RD estimate. Standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

estimation addresses by conditioning on the full geographic control set. The main analysis proceeds with this restricted sample. Results remain unchanged for the full sample (see Appendix Table C.2).

I estimate the causal effect of legal origins using a local linear regression discontinuity design. To isolate the mechanism, I stratify the analysis by social structure using a median split, estimating the model separately for High Kinship and Low Kinship societies:

$$y_{i,b,c} = \alpha_k + \beta_k \text{Common Law}_c + f_k(\text{distance}_{i,b}) + \mathbf{X}'_i \gamma_k + \delta_b + \varepsilon_{i,b,c} \quad (2)$$

where i indexes grid cells, b country pairs, and c countries; $y_{i,b,c}$ denotes the log of nighttime luminosity and Common Law_c is the treatment indicator. The term $f_k(\text{distance}_{i,b})$ represents a side-specific local linear polynomial of the running variable (geodesic distance to the border), estimated separately on the Civil Law and Common Law sides of the cutoff. Subscript $k \in \{\text{Low}, \text{High}\}$ denotes the subsample defined by the median Kinship Intensity Index. The coefficient of interest, β_k , measures the jump in economic activity at the border for that specific group.

All specifications include the set of geographic and historical controls, country-pair fixed effects (δ_b), and employ a triangular kernel with MSE-optimal bandwidths (Calónico et al., 2014). Standard errors are clustered at the country-pair level. For the baseline estimation, I use the restricted sample (excluding China) to ensure balanced geographic covariates. In Appendix Table C.2, I find that including China does not alter the qualitative results.

4.3 Main Results

Table 4 presents the spatial regression discontinuity estimates. Panel A reports the results for the High Kinship sample ($KII > \text{Median}$), which constitutes the primary test of the institutional mismatch hypothesis. Across all specifications, the causal effect of Common Law institutions (relative to Civil Law) is positive, large, and statistically significant. The baseline

point estimate using the MSE-optimal bandwidth (Column 1) implies that for high-kinship societies, crossing the border from a Civil Law to a Common Law jurisdiction generates a substantial increase in economic activity.

To gauge the economic magnitude of this ‘mismatch penalty,’ I convert the estimated nightlight differences at the intensive margin into implied GDP per capita. Following the elasticity estimates established by [Henderson et al. \(2012\)](#), who find that a 1% increase in nightlights corresponds to roughly a 0.3% increase in GDP, the baseline RD estimate of 1.90 log units (Column 1) suggests a substantial divergence in real economic output. Applying this elasticity implies that high-kinship ethnic homelands under Common Law enjoy approximately 77% higher economic activity than their counterparts under Civil Law ($1.90 \times 0.3 \approx 0.57$ log units of GDP, $e^{0.57} \approx 1.77$). This intensive margin figure should be treated as an upper bound, as it conditions on the existence of light and applies a cross-country elasticity to a within-border comparison. Nevertheless, to provide a sense of scale, this gap is comparable to the persistent income disparity observed between Ghana (\$3,190 per capita) and Cameroon (\$2,030 per capita) or Kenya vs. Uganda, spanning the threshold from “low-income” to “lower-middle-income” status.

Turning to the extensive margin (the probability of detecting any light at all) provides a more conservative assessment that is better suited to the African context, where low electrification rates make the binary distinction between ‘dark’ and ‘lit’ a primary indicator of development. Column 1 of Table 5 reports that Common Law governance increases the probability of light by 19.5 percentage points relative to a Civil Law baseline of 46%, a 42% increase in the spatial extent of economic activity. Translating this extensive-margin estimate into GDP requires an additional approximation, since the elasticity applies directly to luminosity rather than to the probability of observing any light. As a back-of-the-envelope calibration, I treat the 42% expansion in the probability of light as proportional to an expansion in aggregate nightlight activity and then apply the 0.3 elasticity ([Henderson et al., 2012](#)). Under this assumption, the estimate implies an approximate 12.6% differential in

Table 4: The Causal Effect of Institutional Mismatch: RD Estimates (Log)

	<i>Dependent Variable: Log Nighttime Luminosity</i>			
	(1)	(2)	(3)	(4)
Specification:	CCT	2.0× CCT	Fixed BW	Quadratic
Bandwidth:	Optimal	Wide	200 km	Optimal
Panel A: High Kinship				
RD Estimate	1.908** (0.822)	1.995** (0.786)	1.955** (0.818)	1.925** (0.911)
Control Mean	-9.60	-9.57	-9.89	-9.61
Observations	8,950	16,598	31,819	11,908
Clusters	48	50	50	49
Bandwidth (km)	43.8	87.5	200.0	59.8
Panel B: Low Kinship				
RD Estimate	-0.567 (0.392)	-0.283 (0.379)	0.284 (0.457)	-1.102* (0.575)
Control Mean	-8.92	-9.44	-9.51	-9.18
Observations	8,246	15,279	34,502	10,893
Clusters	48	49	49	48
Bandwidth (km)	31.5	62.9	200.0	42.8
Polynomial Order	1	1	1	2
Kernel	Tri	Tri	Uni	Tri
Country Pair FEs	✓	✓	✓	✓
Full Controls	✓	✓	✓	✓

Notes. The table reports spatial RD estimates of the effect of Common Law relative to Civil Law, dropping China from the global sample to achieve balance among Low Kinship groups. Treatment = assignment to Common Law jurisdiction; Control = Civil Law jurisdiction. Control Mean is the mean outcome on the Civil Law side within the optimal bandwidth. The unit of observation is a $0.125^\circ \times 0.125^\circ$ grid cell. Panel A restricts the sample to High Kinship groups; Panel B restricts to Low Kinship groups. Column 1 uses the MSE-optimal bandwidth (Calonico et al., 2014). Column 2 doubles this bandwidth. Column 3 uses a fixed 200 km bandwidth with a uniform kernel to grant equal weight to all observations in the broader border region. Column 4 fits a local quadratic polynomial. Coefficients are bias-corrected RD estimates; robust standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

GDP per capita.

For the remainder of the paper, I rely primarily on the extensive margin estimates. This measure is more conservative and avoids the low signal-to-noise ratio of low-luminosity readings. In rural and peri-urban contexts, variation in light intensity is often distorted by sensor blooming and unstable voltage, whereas the presence of stable nightlights serves as a far more reliable proxy for the existence of permanent infrastructure and electrified settlements (Chen and Nordhaus, 2011; Storeygard, 2016). Given that the subsequent analysis focuses on Sub-Saharan Africa and Cameroon, relying on this extensive margin reinforces that I am capturing genuine variation in economic capacity as opposed to artefacts of measurement error.

The estimated effects for both the extensive and intensive margins are robust to bandwidth selection and functional form. As reported in Columns 2 through 4, doubling the bandwidth, employing a local quadratic polynomial, or using a uniform kernel yields estimates that are statistically indistinguishable from the baseline magnitude. These results indicate that the estimated development penalty is robust to the examined bandwidths and functional forms. Appendix Tables C.4 and C.5 further support robustness with coefficients for placebo boundaries and Donut-hole RDDs (excluding observations near the boundary), and Figure C.2 shows randomisation-inference results in the form of a permutation test.

Falsification: Low Kinship Societies. Panel B reports the estimates for the Low Kinship sample ($KII < \text{Median}$), restricted to exclude the China-India border to ensure balance. In contrast to the high-kinship results, the RD estimates for low-kinship societies are small in magnitude and statistically indistinguishable from zero across all specifications. The lone exception is the quadratic polynomial specification (Column 4), which yields a negative coefficient.⁶ This null result is generally supportive of the institutional mismatch hypothesis. It indicates that the development gap is not driven by a universal inefficiency

⁶This likely reflects the well-known instability of higher-order polynomials in RD settings (Gelman and Imbens, 2019), particularly when the underlying relationship is flat.

Table 5: The Causal Effect of Institutional Mismatch: RD Estimates

	<i>Dependent Variable: Probability of Light</i>			
	(1)	(2)	(3)	(4)
Specification:	CCT	2.0× CCT	Fixed BW	Quadratic
Bandwidth:	Optimal	Wide	200 km	Optimal
Panel A: High Kinship				
RD Estimate	0.195** (0.079)	0.199** (0.078)	0.176*** (0.067)	0.200** (0.090)
Control Mean	0.46	0.45	0.43	0.45
Observations	7,628	13,987	31,822	10,873
Clusters	48	50	50	49
Bandwidth (km)	36.0	72.0	200.0	53.8
Panel B: Low Kinship				
RD Estimate	-0.078 (0.049)	-0.057 (0.048)	0.030 (0.038)	-0.099* (0.059)
Control Mean	0.50	0.45	0.44	0.47
Observations	7,620	14,118	34,502	11,153
Clusters	48	49	49	48
Bandwidth (km)	28.8	57.6	200.0	44.3
Polynomial Order	1	1	1	2
Kernel	Tri	Tri	Uni	Tri
Country Pair FEs	✓	✓	✓	✓
Full Controls	✓	✓	✓	✓

Notes. The table reports spatial RD estimates of the effect of Common Law relative to Civil Law, dropping China from the global sample to achieve balance among Low Kinship groups. Treatment = assignment to Common Law jurisdiction; Control = Civil Law jurisdiction. Control Mean is the mean outcome on the Civil Law side within the optimal bandwidth. The unit of observation is a $0.125^\circ \times 0.125^\circ$ grid cell. Panel A restricts the sample to High Kinship groups; Panel B restricts to Low Kinship groups. Column 1 uses the MSE-optimal bandwidth (Calónico et al., 2014). Column 2 doubles this bandwidth. Column 3 uses a fixed 200 km bandwidth with a uniform kernel to grant equal weight to all observations in the broader border region. Column 4 fits a local quadratic polynomial. Coefficients are bias-corrected RD estimates; robust standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

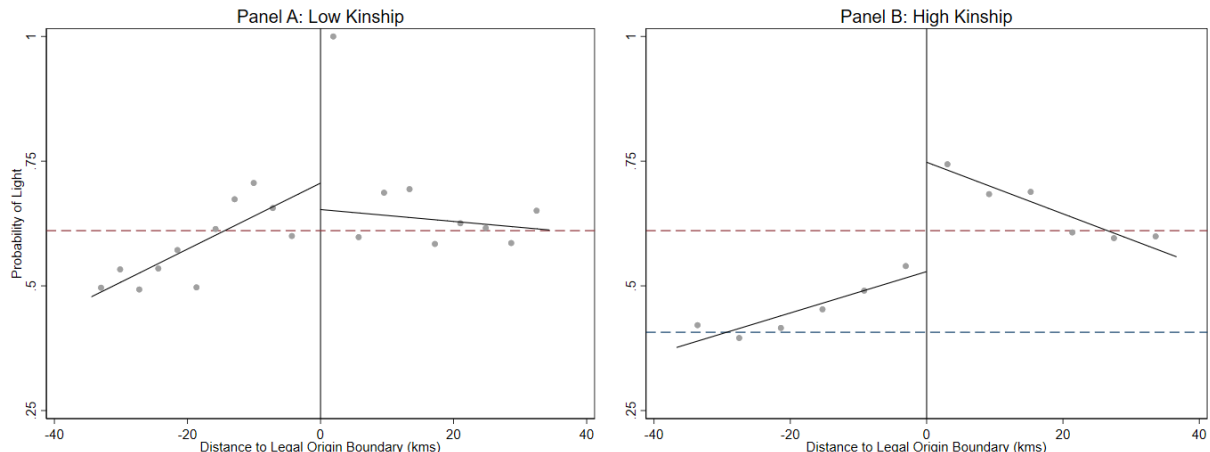


Figure 6: Spatial Regression Discontinuity Plots by Kinship Intensity

Notes. The figure plots the probability of light against the geodesic distance to the border separating Civil Law (negative values) and Common Law (positive values) jurisdictions. Panel A displays the Low Kinship sample ($KII < \text{Median}$), restricted to exclude the China-India border to ensure geographic balance. Panel B displays the High Kinship sample ($KII > \text{Median}$). Dots represent binned sample means. Solid lines represent local linear polynomial fits estimated separately on each side of the cutoff. The red horizontal dashed line represents the mean outcome in the Low Kinship sample. The blue dashed line represents the mean outcome in the High Kinship Civil Law sample.

of tight kinship norms. Rather, the penalty seems to arise specifically from the interaction between rigid legal institutions and high-kinship groups.

Figure 6 visualises these contrasting effects. Panel B (High Kinship) displays a discontinuous drop in economic activity at the border as one moves from Common Law to Civil Law. Conversely, Panel A (Low Kinship) shows no such discontinuity, with economic activity varying more smoothly across the legal boundary.

Finally, it is helpful to provide a back-of-the-envelope calculation to assess the aggregate cost of this institutional mismatch penalty. The extensive margin estimates suggest that for a high-kinship region, the mismatch penalty of Civil Law lowers economic density by approximately 12.6%. High-kinship regions account for roughly 37% of the population in the Civil Law jurisdictions represented in the sample. A purely mechanical, population-weighted extrapolation therefore implies that resolving this mismatch (e.g., through the integration

of customary norms as in Botswana’s *Kgotla* system) could raise aggregate output across these sampled jurisdictions by up to 4.7% (0.126×0.37). This figure is an upper bound that assumes the estimated 12.6% penalty applies throughout the exposed population and does not account for differences in baseline output per capita across regions. While these extrapolations require caution, they suggest that the kinship penalty is economically relevant and responds to institutional design. Appendix A.2 further reports the RD coefficient profile by kinship quintile and calibrates the theoretical model to these estimates.

5 Mechanism: Crisis of Representation

Having established the negative economic consequences of institutional mismatch, I now investigate a potential mechanism. I propose that when formal legal institutions contradict the moral imperatives of kinship (e.g., by enforcing individual property rights over communal claims), high-kinship groups may view the state as illegitimate.

5.1 Data and Identification Strategy

To test this mechanism, I leverage geocoded micro-data from the Afrobarometer, a pan-African series of nationally representative surveys on political attitudes and behaviour. I use Round 8, fielded between 2019 and 2021, which surveys 48,084 citizens across 34 African countries and records the GPS coordinates of each enumeration area ([Afrobarometer, 2024](#)). The survey provides information about the priorities, preferences, experiences, and opinions of these respondents.

To assign kinship values to individuals, I spatially match each respondent to their ancestral ethnic homeland from the Ethnographic Atlas ([Murdock, 1967](#)), based on the GPS coordinates of the enumeration area. This allows me to link individual political attitudes to the pre-colonial Kinship Intensity Index (KII) directly. Similarly to the economic analysis in Section 4, I restrict the sample to respondents located within partitioned ethnic homelands

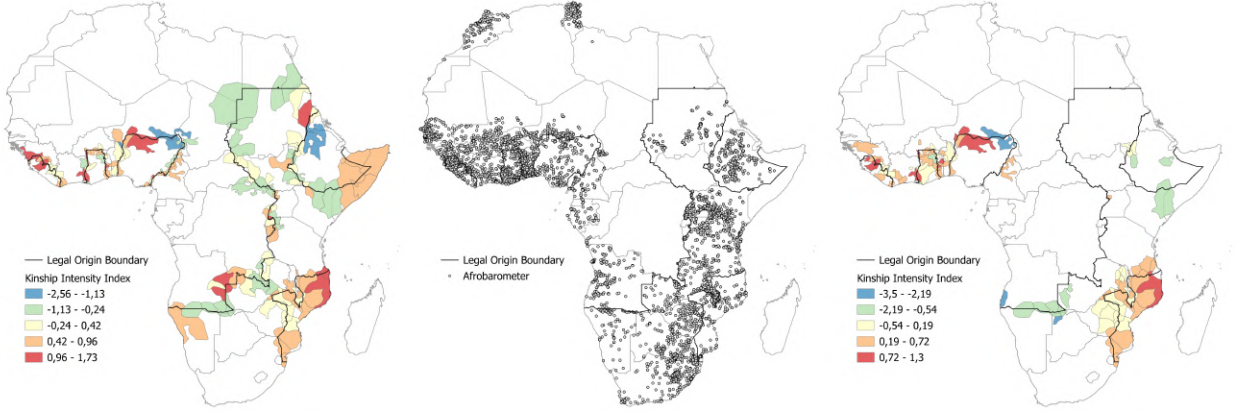


Figure 7: Afrobarometer Sampling and Partitioned Ethnic Homelands

Notes. The figure illustrates the spatial overlap between the Regression Discontinuity sample and Afrobarometer survey coverage. Left Panel: All partitioned ethnic homelands in sub-Saharan Africa. Middle Panel: The universe of Afrobarometer respondents (geocoded clusters). Right Panel: The final estimation sample restricted to partitioned groups intersected by the legal boundary. Black lines indicate national legal borders.

spanning a Civil Law-Common Law border. The final estimation sample comprises 15,770 respondents across 43 partitioned ethnic homelands.

The identification strategy mirrors the spatial regression discontinuity design employed in Section 4, but repeated at the individual level. I compare the attitudes and behaviours of individuals belonging to the same ethnic group and residing in the same geographic environment, yet subject to different legal systems.

The core identifying assumption remains that, conditional on distance to the border, the assignment of individuals to legal origins is locally randomised. Table 6 supports this assumption for the Afrobarometer sample. Across geography, climate, and colonial infrastructure (railways and mission stations), no covariate differs significantly at the legal boundary for either kinship group. The inclusion of the same country-pair fixed effects ensures that comparisons are made strictly between neighbours. A further advantage of the micro-data is the ability to control for individual-level covariates (such as education, age, and gender), which improve precision and account for potential demographic imbalances.

I estimate the following local linear regression separately for High Kinship and Low

Table 6: Afrobarometer Balance Check: Covariates by Kinship Intensity

Dependent Variable	(1) Low Kinship	(2) High Kinship
Panel A: Fundamental Geography		
Elevation	−146.792 (222.793)	−41.642 (215.431)
Ruggedness	−32.400 (76.956)	87.407 (63.288)
Distance to Coast	−238.319 (202.627)	33.154 (177.676)
Distance to River	11.423 (10.469)	−5.724 (17.640)
Panel B: Ecology & Climate		
Temperature	1.138 (1.732)	−0.529 (1.479)
Precipitation	526.662 (393.054)	−175.553 (373.672)
Soil Suitability	0.000 (0.004)	−0.003 (0.007)
Malaria Suitability	3.849 (5.255)	4.596 (3.519)
Panel C: Colonial Infrastructure		
Railways	63.614 (94.759)	3.440 (51.751)
Mission Stations	−34.649 (59.909)	22.014 (21.371)
Avg. Observations	2,120	2,977
Avg. Bandwidth (km)	52.75	49.60
Country Pair FEs	✓	✓

Notes. The table reports spatial RD estimates of the difference in geographic characteristics at the legal boundary, for the Afrobarometer sample. The running variable is distance (km) to the Civil–Common Law legal border, with negative values denoting the Civil Law side. Columns 1 and 2 split the sample by the median Kinship Intensity Index. Each cell reports the bias-corrected robust RD estimate for the indicated dependent variable. Standard errors (in parentheses) are derived from the 95% confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Kinship individuals (median split):

$$y_{ihbc} = \alpha_k + \beta_k \text{Common Law}_c + f_k(\text{dist}_h) + \mathbf{X}_i' \gamma + \mathbf{Z}_i' \Gamma + \delta_b + \varepsilon_{ihbc} \quad (3)$$

where y_{ihbc} is the outcome of interest (e.g., Trust in Courts) for individual i in ethnic homeland h , country pair b , and country c . Common Law_c is the treatment indicator. The term $f_k(\text{dist}_h)$ is a side-specific local linear polynomial of the geodesic distance from the respondent's enumeration area to the border, estimated separately on the Civil Law and Common Law sides of the cutoff. The vector $\mathbf{X}_i$ again comprises the set of geographic and historical controls.

The vector $\mathbf{Z}_i$ includes individual controls: age, a gender indicator, and educational attainment. δ_b denotes country-pair fixed effects. Standard errors are clustered at the country-pair level to account for spatial correlation in errors within border segments.

5.2 State Legitimacy and Institutional Trust

To capture the extent of institutional alienation, I construct a summary State Trust Index as a measure of institutional legitimacy. This index aggregates responses to Afrobarometer questions regarding trust in all (ten) formal state institutions, including the courts of law, police, and parliament. These institutions represent the primary interface between the citizen and the formal legal state. A lack of trust in these specific bodies suggests a rejection of the state's enforcement and legislative monopoly.

Following standard practice to facilitate interpretation, I first standardise each component variable to have a mean of zero and a standard deviation of one, and then take the unweighted average of these components (Kling et al., 2007). Because the components are positively correlated but not perfectly so, the resulting index has a standard deviation of 0.70. Coefficients are therefore reported in index units, and I convert them to standard deviations of the index where that aids interpretation. The full list of components and the

construction details are provided in Appendix B.4.

Legitimacy Gap. Panel A of Table 7 presents the results on state legitimacy. Column (2) reports the estimates for High Kinship individuals. The results reveal a trust deficit in Civil Law jurisdictions. The aggregate State Trust Index is 0.415 index units higher under Common Law, equivalent to 0.60 standard deviations of the index, indicating that high-kinship groups view the Civil Law state as significantly less legitimate.

Examining the individual trust items alongside related measures of representation reveals a broad legitimacy gap across state institutions. The gap is largest for the Ruling Party (0.676), the Courts of Law (0.624), the Tax Office (0.551), the Electoral Commission (0.548), and Parliament (0.536). High-kinship respondents under Civil Law are also significantly less likely to report that their Member of Parliament listens to them (0.512). The effects are smaller but remain positive for the Army (0.454), the Opposition (0.314), and the Police (0.296). The gap therefore appears to extend across legal, fiscal, representative, and coercive institutions.

Column (1) shows that this legitimacy crisis is absent in Low Kinship societies. For individuals with looser kinship ties, the State Trust Index is statistically identical across the legal boundary, and the estimates for specific institutions are overwhelmingly small and insignificant, except for a reversal of trust in police forces mirroring patterns observed elsewhere.⁷ This null result suggests that the delegitimisation of the state is not an inherent feature of Civil Law, but arises specifically from its incompatibility with tight kinship norms.

In terms of magnitude, the 0.415 index-unit gap in state legitimacy corresponds to 0.60 standard deviations of the index, or 0.45 points on the four-point (0–3) trust scale. To benchmark this effect, I compare it to cross-national differences in state trust across the 34 countries surveyed in Round 8.⁸ The ‘mismatch penalty’ on trust is roughly equivalent to the

⁷While Common Law jurisdictions have historically enjoyed high police legitimacy, recent data indicate a significant erosion of this trust in the UK and US (low-kinship societies), falling below the levels observed in low-kinship Civil Law contexts (Hough et al., 2013; Roché and Fleming, 2022; Rogers et al., 2024).

⁸National means are computed on the full Round 8 sample using Afrobarometer’s within-country sampling weights, and are therefore nationally representative. The benchmark index is the unweighted

Table 7: Legal Origins, State Legitimacy, and Civic Engagement by Kinship Intensity

Dependent Variable	(1) Low Kinship	(2) High Kinship
Panel A: State Legitimacy & National Representation		
State Trust Index	−0.023	0.415 ^{***}
Trust in Courts	0.028	0.624 ^{***}
Trust in Police	−0.331 ^{***}	0.296 [*]
Trust in Army	−0.100	0.454 ^{***}
Trust in Tax Office	−0.029	0.551 ^{***}
Trust in Electoral Comm.	0.099	0.548 ^{***}
Trust in Parliament	−0.253	0.536 ^{***}
Trust in Ruling Party	−0.002	0.676 ^{***}
Trust in Opposition	−0.172	0.314 ^{**}
MP Listens	0.123	0.512 ^{***}
MP Performance	−0.189	0.296 ^{**}
Panel B: Civic Engagement		
Political Participation Index	−0.205 ^{***}	0.207 ^{**}
Panel C: Local Intermediaries		
Trad. Leader Influence	0.228 ^{***}	0.327 ^{***}
Trad. Leader Corruption	−0.291 ^{**}	−0.264 ^{**}
Trust in Trad. Leaders	0.056	0.195 ^{**}
Trust in Local Council	0.076	0.424 ^{***}
Local Councillor Listens	−0.053	0.390 ^{**}
Local Councillor Performance	0.109	0.435 ^{***}
Country Pair FEs	✓	✓
Full Controls	✓	✓

Notes. The table reports RD estimates of the effect of legal origin (Common vs. Civil Law). The unit of observation is an individual, geocoded at the village or town level. The sample is split by the median Kinship Intensity Index to contrast the effect in low-kinship societies (Column 1) vs. high-kinship societies (Column 2). Panel A aggregates measures of state legitimacy and national representation. Panel B reports the index of active civic engagement. Panel C reports the integration and responsiveness of local intermediaries. Positive coefficients indicate higher trust/participation under Common Law (except for Corruption, where negative is better). Standard errors are clustered at the country-pair level. ^{*} $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$

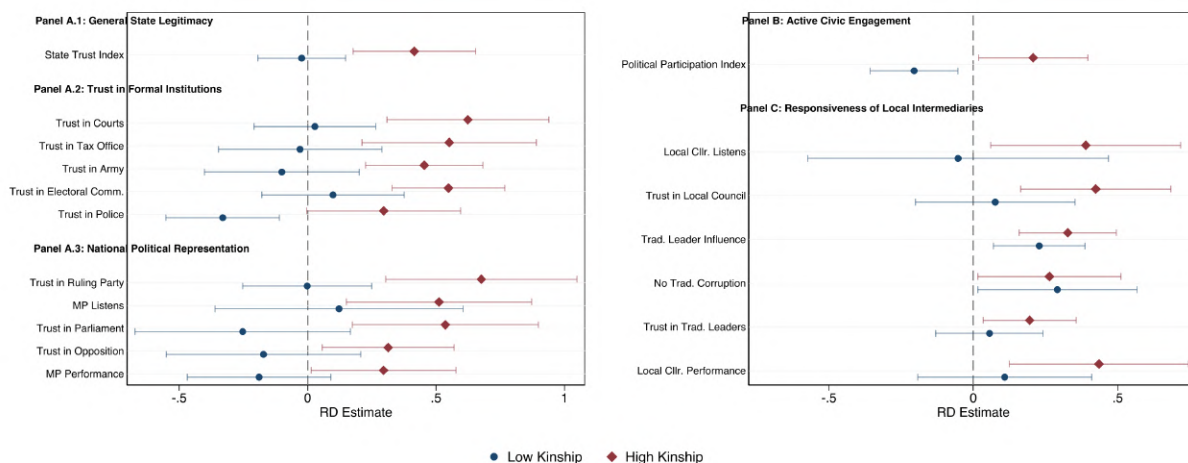


Figure 8: Heterogeneous Effects of Legal Origin on State Legitimacy and Participation

Notes. The figure displays regression discontinuity estimates of the effect of Common Law institutions (relative to Civil Law) on individual attitudes and behaviours. The sample is stratified by the median Kinship Intensity Index: High Kinship (red diamonds) and Low Kinship (blue circles). Coefficients represent standardised effect sizes (standard deviations). A positive value indicates higher trust, participation, or perceived quality of governance under Common Law. The variable “Traditional Leader Corruption” is inverted so that a positive coefficient indicates lower perceived corruption (i.e., better outcomes). Panel B reports effects on the Political Participation Index, an unweighted average of standardised measures of joining others to raise an issue, meeting attendance, and protest participation. See Figure D.1 for the decomposition. All specifications include country-pair fixed effects, individual demographic controls (age, gender, education), and the full vector of geographic covariates. Bars represent 95% confidence intervals clustered at the country-pair level.

gap in state trust between Kenya and Nigeria (0.44 points), two Common Law jurisdictions at broadly similar income levels. Measured against the dispersion of national means, the effect is 1.6 times the standard deviation of average state trust across these 34 countries. In other words, the within-homeland penalty of legal mismatch exceeds the typical difference between two African countries. In distributional terms, the imposition of Civil Law shifts the median high-kinship respondent from the 50th percentile of the Common Law trust distribution down to approximately the 21st percentile.

average of the same ten trust items on their original 0–3 scale. The cross-country standard deviation assigns equal weight to each of the 34 country-level means (Afrobarometer, 2024).

5.3 Political Participation and Exit

Exit Option. If high-kinship groups view the state as illegitimate, how do they respond? I hypothesise that this alienation leads to political exit (i.e., a withdrawal from the formal democratic process (Hirschman, 1970)). Panel B of Figure 8 tests this by estimating the effect on the Political Participation Index, a composite measure of joining others to raise an issue, attendance at community meetings, and participation in demonstrations.

The results indicate that the crisis of representation extends to behavioural disengagement. For high-kinship individuals, civic participation is significantly lower under Civil Law than among their Common Law counterparts ($\beta = 0.207$ index units, equivalent to approximately 0.29 standard deviations of the index). This suggests that institutional mismatch lowers participation as well as trust, as high-kinship communities facing a legal system that does not reflect their norms opt out of the public sphere. This pattern reverses for low-kinship societies, where participation is instead higher under Civil Law ($\beta = -0.205$ index units, equivalent to approximately 0.28 standard deviations of the index). This indicates that the rigid structure of the Civil Code may be better aligned with the social topology of the nuclear family, which would further validate that state legitimacy depends on the match between formal rules and informal norms rather than legal origin alone.

Failure of Local Integration. Panel C of Figure 8 investigates a plausible source of this alienation: the integration of local intermediaries. Historically, British colonial administrations often relied on Indirect Rule, incorporating chiefs and local councils into the state apparatus. In contrast, the Civil Law tradition favoured Direct Rule, often bypassing or stripping power from customary authorities.

The estimates suggest that this historical divergence persists. Under Common Law, traditional leaders retain significantly greater influence and are viewed as less corrupt. Furthermore, this gap in representation seems to extend to local democracy. High-kinship citizens under Civil Law are significantly less likely to believe that their Local Councillors

listen to them, and rate their performance accordingly. This suggests that the Common Law advantage could stem from its historical ability to co-opt local brokers who bridged the gap between the state and the kin network, whereas the centralised Civil Law structure may have marginalised them, leaving high-kinship citizens with no effective interface to the state.

5.4 Alternative Explanations

One challenge in identifying the causal effect of legal origins is the historical bundling of colonial institutions. Common Law jurisdictions frequently coincided with the administrative practice of Indirect Rule, whereas Civil Law regimes often employed Direct Rule. Consequently, a sceptical interpretation of the mismatch penalty is that it may not stem from the rigidity of the legal code, but simply from the administrative dismantling of local political structures under the French colonial model. While this subsection provides preliminary micro-evidence addressing this concern, the subsequent two sections tackle it more comprehensively. Section 6 uses natural language processing on colonial court records to document that the mismatch is encoded in the deep associative structure of the legal traditions themselves. Section 7 provides a contemporary test within Cameroon’s Anglophone region, where historical colonial strategy is held constant.

If the Common Law advantage is an artefact of Indirect Rule, I might observe a substitution effect where heightened traditional legitimacy comes at the expense of formal state capacity (see Iyer, 2010). Table 8 addresses this hypothesis by restricting the analysis to centralised societies (Jurisdictional Hierarchy ≥ 2). This is a specific subset of high-kinship groups where colonial Indirect Rule was, on average, most active. The results are inconsistent with the weak state hypothesis. While Panel A confirms that Common Law significantly strengthens traditional authority, Panel B reveals that the effect on formal state institutions is substantially larger. For these centralised groups, the Common Law premium on Trust in Courts remains 0.602 standard deviations.

Table 8: Legal Origins, Traditional Authority, and State Legitimacy

Dependent Variable	Centralised Societies (High Kinship)
Panel A: The “Indirect Rule” Baseline (Traditional Authority)	
Trust in Trad. Leaders	0.261 ^{***}
Trad. Leader Influence	0.350 ^{***}
Corruption: Trad. Leaders	−0.302 ^{**}
Panel B: The State Legitimacy Premium (Formal Authority)	
Trust in Courts	0.602 ^{***}
Trust in Parliament	0.493 ^{**}
Trust in Local Council	0.322 ^{**}
Corruption: Civil Servants	−0.266 ^{**}
Panel C: Relative Legitimacy (State vs. Custom)	
Relative Trust (Courts - Chiefs)	0.275 ^{**}
Avg. Observations	1,562
Avg. Bandwidth (km)	29.5
Country Pair FEs	✓
Full Controls	✓

Notes. The table reports RD estimates of the effect of Common Law relative to Civil Law, restricted to the sample of High Kinship Centralised Societies (Jurisdictional Hierarchy ≥ 2). Panel A includes trust in traditional leaders; Panel B includes trust in formal state institutions. Panel C reports the effect on Relative Trust, defined as standardised Trust in Courts minus standardised Trust in Traditional Leaders, and separately standardised. Corruption variables are coded such that negative values indicate lower reported corruption (better outcomes). Standard errors are clustered at the country-pair level. ^{*} $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$

Panel C reports the effect on ‘Relative Trust’ (Standardised Trust in Courts minus Standardised Trust in Traditional Leaders). The positive coefficient ($\beta = 0.275$) indicates that under Common Law, citizens place relatively greater trust in the modern legal system than in traditional authorities, when compared to their Civil Law counterparts. This pattern points to complementarity. By integrating traditional norms, the Common Law system seems to draw on local legitimacy to raise trust in the modern state instead of ceding authority to custom.

6 Institutional Mismatch: Evidence from Legal Semantics

I validate the mismatch hypothesis using multi-stage computational text analysis. I do this to ensure that the contrasting case histories presented in Section 2 represent structural features of the two colonial legal regimes, and not just isolated anecdotes. I employ two complementary approaches: Word2Vec embeddings to map the internal semantic structure of each tradition, and a cross-lingual Transformer to quantify the normative divergence on a shared legitimacy axis. The first method shows the associative structure within each legal tradition separately, while the second permits direct comparison across traditions in a shared semantic space. Together, these approaches allow me to observe the associative links between words and texts, a proxy for the semantic structure through which each legal tradition processes indigenous concepts.

6.1 Customary Language and Kinship Intensity

As a preliminary check, I test whether places with stronger kinship structures are also the places where colonial courts more frequently discuss customary law and informal norms. This exercise is purely descriptive, verifying whether the court corpora contain the conceptual material that the theory predicts should be most salient where kinship networks are stronger.

Figure 9 shows a positive association in both corpora, using colony-level KII for British Sub-Saharan Africa and province-level KII for the Belgian Congo. Court locations are extracted from the legal documents and serve as proxies for the locations of the disputes themselves. This pattern motivates the subsequent semantic exercise: if customary language is concentrated in high-kinship legal environments, the relevant question becomes how each legal tradition represents and legitimises these customs.

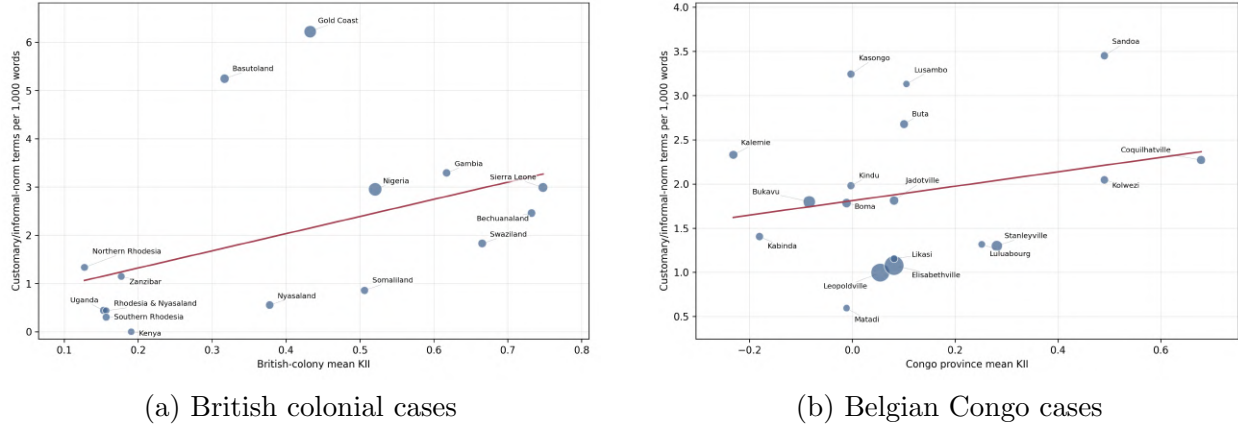


Figure 9: Kinship Intensity and Customary-Law Language in Colonial Court Cases

Notes. The figure plots customary/informal-norm term mentions per 1,000 words against kinship intensity. Panel (a) reports British Sub-Saharan African colonies with matched Judicial Committee of the Privy Council cases. Panel (b) reports Belgian Congo court locations with assigned province-level KII, excluding the outlier of Irumu. KII is computed from grid-cell values aggregated to the relevant geography. Bubble size reflects the number of cases.

6.2 The Semantic Topology of Property Rights

The results in Figure 10 reveal a clear divergence in semantic structure (see Appendix B.2 for full topological maps and further detail). In the Common Law vector space, customary terms (e.g., *stool land*) exhibit high cosine similarity with concepts of land and ownership, consistent with the system’s capacity to absorb local norms. In contrast, the Civil Law model has a significantly lower cosine similarity between custom and land-related terms. This suggests that in the Common Law corpus, the concepts related to ‘Custom’ lie statistically closer to ‘Property’. In the Civil Law corpus, the two concepts are much further apart.

6.3 The Legitimacy Gap

To quantify the intensity of this divergence, I move from static word embeddings to a Cross-Lingual Transformer methodology. Whereas the Word2Vec exercise identifies the internal semantic topology of each corpus separately, the cross-lingual Transformer allows me to compare English and French legal texts directly in a common vector space. I employ the

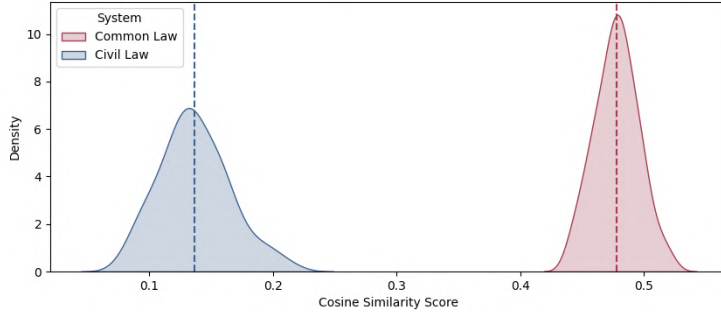


Figure 10: The Semantic Divergence of Colonial Law between Land and Custom

Notes. The figure displays the bootstrap distribution of cosine similarity scores between ‘Custom’ and ‘Land/Property’ vectors ($N = 100$ resamples). There is virtually no overlap between the Common Law (red) and Civil Law (blue) corpora, showing that Common Law associates customary terms with land and property concepts more than Civil Law.

paraphrase-multilingual-mpnet-base-v2 architecture to map English and French text segments into a shared high-dimensional vector space (Reimers and Gurevych, 2019). I partition each case into 300-word context windows and project them onto a constructed Legitimacy Axis.

I operationalise this axis as a continuous geometric dimension constructed from the vector difference between two bilingual sets of anchor terms. Let $\vec{\mu}_{pos}$ and $\vec{\mu}_{neg}$ represent the centroids (mean vectors) of terms associated with Legitimacy (e.g., *valid*, *coutume*, *recognised*, *droit indigène*) and Repugnancy (e.g., *repugnant*, *ordre public*, *void*, *sauvage*), respectively. I construct the normative axis $\vec{v}_{axis}$ by normalising the difference vector to unit length:

$$\vec{v}_{axis} = \frac{\vec{\mu}_{pos} - \vec{\mu}_{neg}}{\|\vec{\mu}_{pos} - \vec{\mu}_{neg}\|} \quad (4)$$

The normative stance of each legal text segment $\vec{e}_i$ is then calculated as its scalar projection onto this axis via the dot product, $S_i = \vec{e}_i \cdot \vec{v}_{axis}$, such that higher scores indicate greater semantic proximity to the concept of legitimacy.

The unit of analysis in both the distributional figure and the baseline regression is the legal case. Each 300-word segment receives a legitimacy score, and these segment-level projections

are averaged to the case level. The regressions then compare case-level scores across legal traditions while controlling for time and topic composition. The results, visualised in Figure B.3, indicate that while Common Law rulings exhibit a higher standard deviation, they treat customary law as more legitimate on average.⁹ In contrast, Civil Law rulings cluster closer toward the repugnancy pole and they display a lower variance. These patterns corroborate the idea that Civil Law judges have less discretion in their rulings compared to Common Law judges, which lies at the basis of the institutional mismatch hypothesis.

To investigate whether this result is driven by confounding variables, such as the Belgian courts hearing more criminal cases or operating in different decades, I estimate the following OLS specification:

$$Legitimacy_{i,c,t} = \alpha + \beta \text{Civil Law}_c + \delta_d + \epsilon_{i,c,t} \quad (5)$$

To isolate Land and Property disputes, I employ automated topic classification. This classifier utilises a bilingual dictionary of domain-specific keywords (e.g., *land*, *title*, *ownership*, *stool* for Common Law; *propriété*, *terrain*, *terre* for Civil Law) to identify disputes over customary tenure, excluding unrelated criminal or procedural matters. I include Decade Fixed Effects (δ_d) to control for secular trends in colonial governance; in alternative specifications (Columns 3 and 6 of Table 9), I replace the decade fixed effects with a linear time trend in decades, whose coefficient is denoted by λ .

As reported in Table 9, the results document a systematic divergence between the two colonial legal corpora. Across the full sample (Columns 1–3), the Civil Law coefficient is consistently negative, indicating that Belgian judgments are systematically projected closer to the “Repugnancy” pole than their British counterparts. Columns 4–6 restrict the analysis to the cases of land and customary tenure. Despite reduced statistical power, the coefficient

⁹As a simple measure of validation, the Privy Council’s ruling against the colonial government’s claim to “unoccupied” land in *Amodu Tijani v. Secretary of Southern Nigeria* (1921), mentioned in Section 2, received the second highest legitimacy score among all colonial court cases in the corpus.

Table 9: Legal Origins and Legitimacy Vector Projection

	<i>Dependent Variable: Judicial Legitimacy Score</i>					
	Full Sample			Land & Custom Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Civil Law System	-0.031*** (0.010)	-0.026** (0.012)	-0.037*** (0.010)	-0.027** (0.011)	-0.021 (0.014)	-0.029*** (0.011)
Linear Time Trend			0.012*** (0.002)			0.006 (0.003)
Observations	1,615	1,615	1,615	611	611	611
Decade FEs		✓			✓	
Linear Time Trend			✓			✓

Notes. The table reports OLS estimates of the relationship between Legal Origin (Civil Law = 1) and the Judicial Legitimacy Score (Vector Projection). Columns (1-3) use the full sample of 1,615 cases; Columns (4-6) restrict the sample to Land, Property, and Customary Law, excluding neutral commercial or criminal disputes (N=611). The dependent variable is the scalar projection onto the unit-length Legitimacy Axis, $S_i = \vec{e}_i \cdot \vec{v}_{axis}$ (Valid vs. Repugnant). Column 1 is the bivariate baseline. Columns 2 and 5 add Decade Fixed Effects. Columns 3 and 6 control for a continuous linear time trend in decades (centred on 1940). Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of Civil Law remains negative and relatively stable.¹⁰

The magnitude of this gap is historically substantial. The positive coefficient on the time trend in decades (Column 3, $\lambda = 0.012$) captures a secular liberalisation of colonial jurisprudence over the 20th century. Comparing this rate of change to the legal origin coefficient ($\beta = -0.037$) suggests that the Civil Law effect is equivalent to approximately three decades of secular progress. This interpretation comes from dividing the legal-origin coefficient by the estimated decade trend, so it should be read as a fitted trend-comparison. In other words, the estimated Civil Law gap corresponds to a setback of more than thirty years in the recognition of customary rights relative to the Common Law trend. On average, a Civil Law court in 1960 treated customary rights with the same degree of scepticism as a Common Law court did in 1930.

¹⁰Because legal origin varies only at the corpus level, clustering standard errors on the legal system itself is infeasible. As an alternative to the heteroskedasticity-robust standard errors reported in Table 9, clustering by year of decision (70 clusters) leaves the significance of the full-sample estimates unchanged. The corpus-level bootstrap in Figure 10 provides complementary non-parametric evidence at the 1% level.

7 Real-time Validation: The Anglophone Crisis

The cross-sectional results in Sections 4 and 5 established that institutional mismatch is associated with lower development and political alienation. However, a standard empirical challenge in this literature is disentangling the effect of the legal regime from broader colonial strategies. In cross-border analyses, Common Law is historically bundled with British Indirect Rule, while Civil Law is bundled with (French) Direct Rule. This raises a potential confounding alternative explanation: that divergent development trajectories stem from these administrative legacies rather than the friction of the legal codes themselves.

To address this confounder and validate the causal mechanism, I exploit a natural experiment in Cameroon: the sudden, modern imposition of Civil Law institutions upon a Common Law society. By focusing on a strictly judicial shock within a single sovereign state, I can isolate the effect of the legal framework from historical administrative strategies. Furthermore, if the mechanism driving the kinship penalty is indeed ‘Strategic Exit’ (as suggested by the Afrobarometer results), I should observe this withdrawal dynamically when institutional mismatch spikes. The 2016 Anglophone Crisis offers a precise test of this.

7.1 The 2016 Judicial Shock

Cameroon is uniquely divided between a Francophone majority (Civil Law) and an Anglophone minority (Common Law), a legacy of the partition of German Kamerun between France and Britain after WWI. Following the 1961 plebiscite, the territories united as the *Federal Republic of Cameroon*, a constitutional arrangement designed to preserve the institutional autonomy of the Anglophone minority. However, this federal structure was unilaterally dismantled by the central government in 1972 in favour of a unitary state, a move widely perceived by Anglophones as a forced annexation. This structural marginalisation, referred to in the literature as the Anglophone Problem, entrenched a deep-seated grievance regarding the preservation of Anglophone identity (Konings and

Nyamnjoh, 2003).

Over the subsequent decades, the centralised state progressively eroded Common Law protections, though the Anglophone regions long retained their distinct legal and educational systems. A flashpoint arrived in late 2016, when the Francophone central government attempted to fully harmonise the judicial system by appointing Civil Law judges (who often did not understand English or Common Law precedents) to courts in the Anglophone region (Pommerolle and Heungoup, 2017; Beseng et al., 2023).

The appointment of Civil Law judges in late 2016 constitutes a regional institutional shock. In other words, an increase in institutional mismatch. This imposition triggered immediate mass protests by Anglophone lawyers and teachers, citing the incompatibility of the legal traditions (Ekah, 2020). The state’s repressive response escalated the conflict into a separatist insurgency.¹¹ The initial institutional shock occurred simultaneously across the Anglophone region, but the onset of active resistance varied locally. Therefore, the staggered event-study design below uses this locally varying onset of conflict as the empirical treatment, not the judicial appointments themselves.

For this study, the primary tool of resistance and civil disobedience was the “Ghost Town” (*ville morte*): a series of general strikes enforced by local communities. Every Monday, and often for weeks at a time, all economic activity was banned to delegitimise the state. I hypothesise that Kinship Intensity served as the enforcement technology for these strikes. Sustaining a general strike requires solving a collective action problem: preventing individual merchants from defecting to open their shops for profit. In high-kinship communities, dense social networks could punish defectors, thereby sustaining the economic shutdown. In low-kinship areas, collective action could be harder to sustain or might fail altogether, allowing economic activity to continue.

Figure 11 illustrates the setting and identification strategy. The right panel highlights

¹¹On 1 October 2017, the Southern Cameroon Ambazonia Consortium United Front (SCACUF) marked the anniversary of the independence of the Republic of Ambazonia from British rule by unilaterally declaring the independence of Ambazonia (Nshom, 2025).

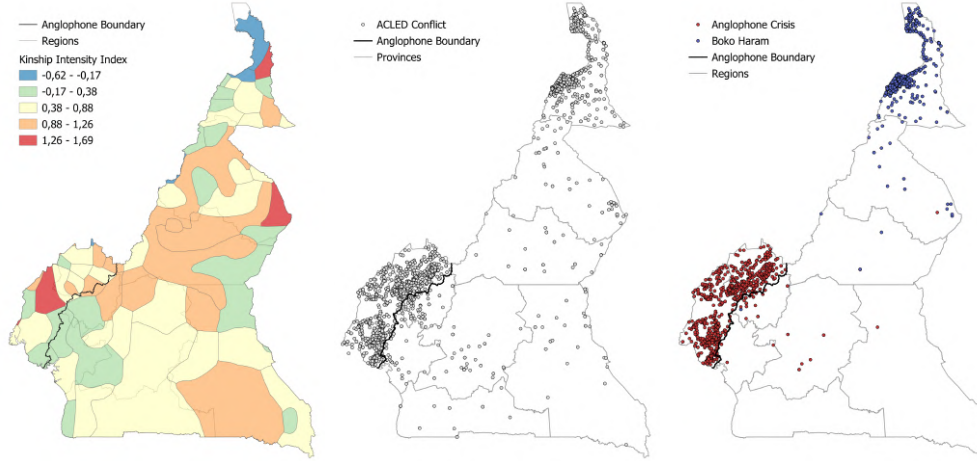


Figure 11: Kinship Structure and Conflict Clusters in Cameroon

Notes. The figure maps the spatial variation in social structure and conflict exposure. The left panel displays the Kinship Intensity Index (KII) across the ethnic groups of Cameroon, where darker red indicates tighter kinship norms. The centre panel displays the location of violent events recorded by the Armed Conflict Location & Event Data (ACLED) database (1997–2024). Two distinct conflict clusters are visible in the right panel: the Anglophone Crisis in the South-West (Common Law zone, initiated late 2016) and the Boko Haram insurgency in the Far North (Civil Law zone, initiated 2014).

the Anglophone conflict zone in the South-West, and the Boko Haram insurgency in the Far North. The left panel reveals substantial variation in Kinship Intensity within this zone, providing the necessary variation to test whether social structure modulated the economic impact of the institutional shock.

7.2 Identification Strategy

To isolate the causal role of social structure in mediating the institutional shock, I observe economic activity in grid cells across Cameroon from 2013 to 2024. The identification exploits variation in three dimensions: (i) spatial exposure to the institutional shock (Anglophone vs. Francophone regions), (ii) temporal exposure (pre- vs. post- local conflict onset), and (iii) cross-sectional variation in the pre-colonial Kinship Intensity Index.

My estimation strategy proceeds in two steps. First, to verify the parallel trends assumption and trace the temporal evolution of resistance, I exploit the variation in the *de*

facto implementation of the boycott. While the *de jure* institutional shock (the 2016 policy) was regional and simultaneous, the onset of active resistance (Ghost Towns) was locally staggered. I employ the estimator proposed by [Callaway and Sant’Anna \(2021\)](#), which allows for treatment effect heterogeneity and staggered adoption. I estimate group-time average treatment effects, $ATT(g, t)$, measuring the impact of conflict onset for units treated at time g in period t , using not-yet-treated and never-treated units as controls:

$$ATT(g, t) = \mathbb{E}[Y_t - Y_{g-1} | G_g = 1] - \mathbb{E}[Y_t - Y_{g-1} | C = 1] \quad (6)$$

To examine the mediating role of social structure, I estimate these parameters separately for the High and Low Kinship subsamples (median split). I then aggregate these parameters by event-time e (where $e = t - g$) to recover the dynamic parameters $\theta_{es}(e)$:

$$\theta_{es}(e) = \sum_g \sum_t \mathbb{I}[t - g = e] w_{g,t} \widehat{ATT}(g, t) \quad (7)$$

This dynamic specification allows me to explicitly test for pre-trends (where $e < 0$) and observe whether the economic divergence coincides precisely with the locally heterogeneous onset of the crisis ($e \geq 0$).

A potential confounder is that high-kinship areas might simply have experienced more violence (collateral damage) rather than strategic economic shutdowns. To address this, I estimate a mechanism specification that explicitly controls for conflict intensity. I split the treatment by specific conflict types derived from ACLED data: (i) *Violence Against Civilians* (a proxy for enforcement) and (ii) *Battles/Explosions* (a proxy for physical destruction). I then test whether the high-kinship interaction is specific to violence against civilians instead of battles and explosions, where such a pattern would be consistent with strike enforcement as opposed to physical destruction.

7.3 The Cost of Imposition

I begin by inspecting the raw trends in economic activity. Figure 12 plots the probability of nighttime light separately for the Low Kinship (Panel A) and the High Kinship (Panel B) samples. In Low Kinship areas (Panel A), trends in the Anglophone (Treatment) and Francophone (Control) regions track each other relatively closely throughout the entire period. There seems to be no structural break following the 2016 crisis, suggesting that without the enforcement capacity of kinship networks the institutional shock did not trigger a sustained economic shutdown.

Panel B, however, reveals two notable patterns. First, the baseline levels prior to 2016 mirror the global mismatch penalty documented in Section 4: Francophone High Kinship groups (blue line) exhibit significantly lower economic activity, while Anglophone High Kinship groups (red line) perform on par with the Low Kinship benchmark. This suggests that the Civil Law mismatch penalty was already present in the static equilibrium. Second, immediately following the judicial imposition in 2016, Anglophone economic activity collapses, erasing its historical advantage and converging downward toward the Francophone level. This divergence suggests that the economic costs of the crisis were concentrated in communities with the social structure capable of enforcing collective resistance.

Figure 13 presents the timing of the response using the dynamic difference-in-differences estimator of Callaway and Sant’Anna (2021). Panel A plots the estimated impact on economic activity (probability of nightlight). The coefficients in the pre-period ($e < 0$) are small and display no sustained divergence for either group.¹² This broadly flat pre-period supports the identification assumption that high-kinship and low-kinship areas were on similar economic trajectories prior to the institutional shock.

Following the onset of the crisis ($e = 0$), the trajectories diverge. In the Low Kinship sample (blue circles), economic activity remains resilient, with coefficients hovering near zero

¹²As reported in Appendix Table E.3, two isolated coefficients ($e = -3$ for the High Kinship group and $e = -2$ for the Low Kinship group) reach statistical significance, but neither forms part of a systematic trend.

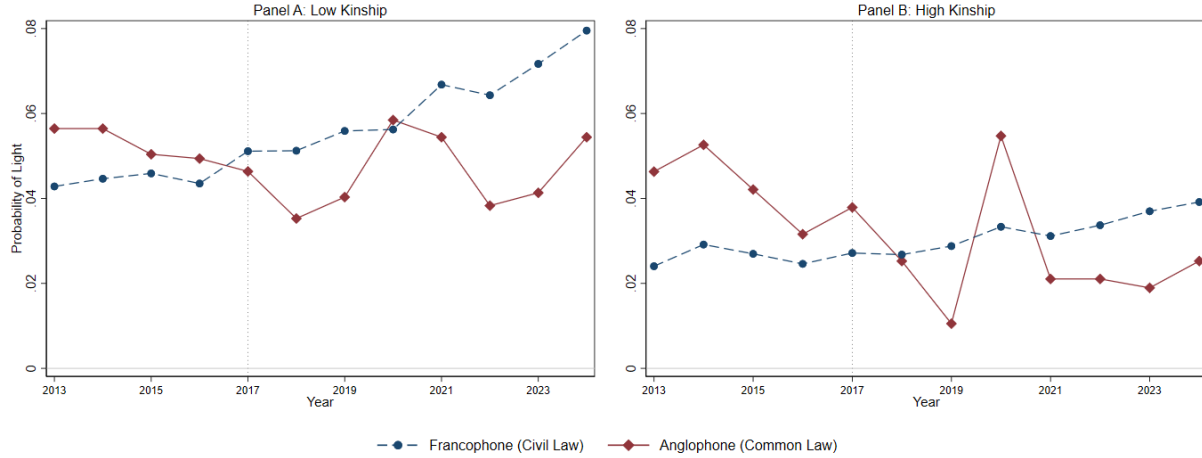


Figure 12: Trends in Economic Activity around the Anglophone Crisis by Kinship Intensity

Notes. The figure plots raw trends in the probability of detectable nighttime light at the grid cell level ($0.05^\circ \times 0.05^\circ$) in Cameroon (2013–2024). The sample is stratified by the median Kinship Intensity Index. Panel A displays trends for Low Kinship ethnic groups; Panel B displays trends for High Kinship ethnic groups. The red line represents the Anglophone (Common Law) region, and the blue line represents the Francophone (Civil Law) region. The vertical dashed line marks the onset of the judicial crisis in late 2016.

throughout the post-conflict period. By contrast, the High Kinship sample (red diamonds) exhibits a statistically significant and persistent decline. The timing of the decline, which begins at local conflict onset, suggests a break linked to the imposition of the Civil Law judiciary. Panel B reports the trajectory of violence against civilians. The overlapping confidence intervals in the early post-period suggest that differential exposure to violence alone does not explain the economic divergence in Panel A, which points instead to a differential response of the social network to that environment.

Table 10 formalises the visual evidence using the Doubly Robust Inverse Probability Weighting (DR-IPW) estimator (Callaway and Sant’Anna, 2021). Columns 1-2 report the aggregate Average Treatment Effect on the Treated (ATT) for the Anglophone Crisis. The estimates show that the economic collapse was concentrated in high-kinship communities. In Low Kinship areas (Column 1), the impact of the crisis is modest ($\beta = -0.027$) and only marginally significant. In contrast, High Kinship areas (Column 2) experienced a contraction

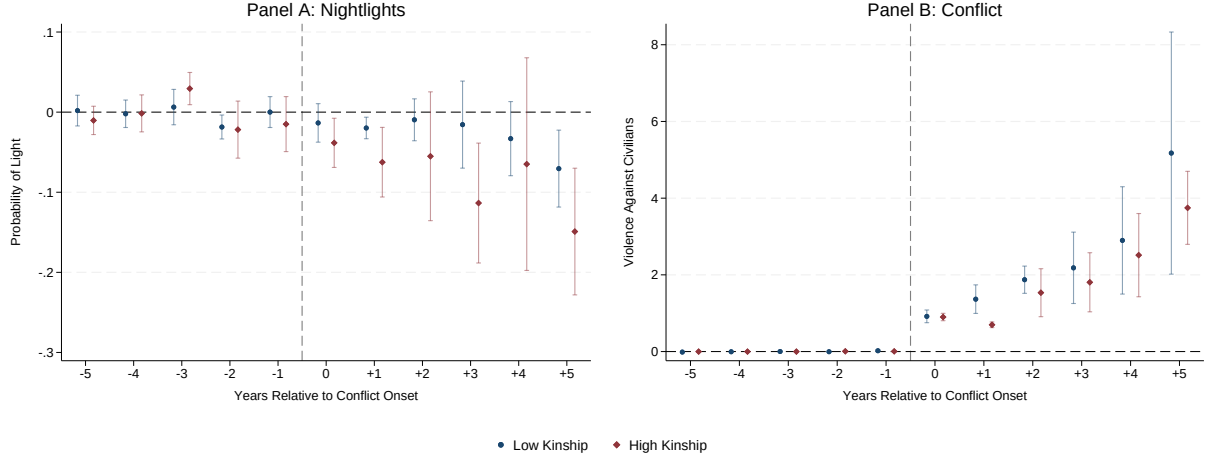


Figure 13: Dynamic Effect of the Anglophone Crisis on Economic Activity and Violence Against Civilians by Kinship Intensity

Notes. The figure reports dynamic Average Treatment Effects on the Treated (ATT), estimated using the doubly robust difference-in-differences estimator of Callaway and Sant'Anna (2021). The sample is stratified by the median Kinship Intensity Index (KII). Panel A reports estimates for the Probability of Nighttime Light (Extensive Margin). Panel B reports estimates for the number of events classified as Violence Against Civilians in the ACLED database. The x-axis represents years relative to the first recorded Anglophone conflict event ($e = 0$) in the grid cell. Blue circles represent estimates for the Low Kinship subsample; red diamonds represent estimates for the High Kinship subsample. Bars indicate 95% confidence intervals clustered at the department level.

roughly three times as large ($\beta = -0.081^{**}$). Appendix E.1 shows that this pattern is robust to an alternative stacked difference-in-differences design that estimates cohort-specific event windows around local crisis onset. The stacked estimates preserve the same qualitative result when restricting the comparison to the two Anglophone regions only.

To interpret the magnitude, consider that the pre-treatment mean probability of light in high-kinship areas was 0.069. A coefficient of -0.081 implies that the Ghost Town strikes all but extinguished detectable market activity in these communities (shops remained shuttered for weeks) relative to the counterfactual. The coefficient can exceed the pre-treatment mean because the comparison is against the counterfactual trajectory (i.e., control cells became more likely to be lit over the period, while treated high-kinship cells did not). This near-total shutdown matches qualitative accounts of the crisis, where local enforcement committees

Table 10: Heterogeneous Impact of Conflict by Kinship Intensity

<i>Dependent Variable: Probability of Light</i>				
	Anglophone Crisis		Boko Haram	
	(1)	(2)	(3)	(4)
	Low Kinship	High Kinship	Low Kinship	High Kinship
Estimated Impact				
Post-Treatment Avg.	-0.027* (0.014)	-0.081** (0.032)	0.018 (0.020)	0.043 (0.047)
Pre-Treatment Characteristics				
Pre-Treatment Avg.	-0.002 (0.003)	-0.004 (0.005)	0.002 (0.018)	-0.002** (0.001)
Pre-Treatment Mean	0.083	0.069	0.193	0.194
Observations	103,416	58,512	105,600	65,868
Clusters	54	33	45	26
DR-IPW Estimator	✓	✓	✓	✓

Notes. The table reports dynamic event study coefficients estimated using the Doubly Robust Inverse Probability Weighting (DR-IPW) method (Callaway and Sant’Anna, 2021). The unit of observation is a $0.05^\circ \times 0.05^\circ$ grid cell. The dependent variable is the probability of nighttime light. The estimation panel covers 2013–2024. Post-Treatment Avg. reports the average of the event-time coefficients over the post-conflict period ($e = 0$ to $e = 5$); Pre-Treatment Avg. reports the corresponding average over the pre-conflict period ($e = -5$ to $e = -1$ for the Anglophone Crisis and $e = -2$ to $e = -1$ for the Boko Haram Conflict, reflecting the earlier onset of the insurgency). “Pre-Treatment Mean” reports the average outcome in the pre-conflict period. Standard errors are clustered at the department level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

strictly prohibited all commerce to delegitimise the state. The result was a complete collapse of tax revenue from \$800,000 annually to just \$1,000 in 2019.¹³

Table 11 disaggregates the impact by an urban-rural divide. The logic of the “Ghost Town” (a general strike aimed at paralysing the commercial economy) implies that the effect should be concentrated in urban centres where trade occurs, not in rural subsistence economies. The results support this interpretation. The aggregate collapse in high-kinship areas is driven almost entirely by urban grid cells (Column 5), where the post-treatment average indicates a large contraction ($\beta = -0.087^{**}$). By $e = 5$, the probability of light in these high-kinship cities has plummeted by 15.6 percentage points relative to

¹³See VOA News, “Cameroon Says Conflict Destroying Anglophone Regions’ Economies,” September 2, 2020. www.voanews.com/cameroon-says-conflict-destroying-anglophone-regions-economies.

Table 11: Dynamic Impact of Anglophone Crisis by Kinship and Urban-Rural Divide

	<i>Dependent Variable: Probability of Light</i>					
	Low Kinship			High Kinship		
	(1) Full	(2) Urban	(3) Rural	(4) Full	(5) Urban	(6) Rural
Pre-Treat Avg.	−0.002 (0.003)	−0.005 (0.003)	0.001 (0.001)	−0.004 (0.005)	−0.005 (0.006)	−0.000 (0.000)
Post-Treat Avg.	−0.027* (0.014)	−0.037** (0.015)	0.031*** (0.010)	−0.081** (0.032)	−0.087** (0.036)	−0.000 (0.000)
DR-IPW Estimator	✓	✓	✓	✓	✓	✓

Notes. The table reports dynamic event study coefficients estimated using the Doubly Robust Inverse Probability Weighting (DR-IPW) method (Callaway and Sant’Anna, 2021) for the Anglophone Crisis. Columns 1 and 4 report estimates for the Full Sample. Columns 2 and 5 restrict the sample to Urban grid cells (above-median population density). Columns 3 and 6 restrict the sample to Rural grid cells (below-median population density). The dependent variable is the probability of nighttime light, observed over the 2013–2024 panel. Pre-Treat Avg. and Post-Treat Avg. report the averages of the event-time coefficients over the pre-period ($e = -5$ to $e = -1$) and post-period ($e = 0$ to $e = 5$), respectively. Standard errors are clustered at the department level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the counterfactual. In contrast, high-kinship rural areas (Column 6) show no detectable effect. Comparing the urban samples again implies a role for social structure: while low-kinship cities (Column 2) do experience a milder decline ($\beta = -0.037^{**}$), the collapse in high-kinship cities is more than twice as large. The same qualitative pattern holds for the Triple-Difference (DDD) specification in Table E.2 of Appendix E.2. These findings suggest that the economic penalty mostly stems from the capacity of dense urban networks to enforce non-cooperation, rather than from urban exposure to conflict per se.

Placebo: Boko Haram. To validate that the estimated effect is driven by legal incompatibility rather than a general propensity for high-kinship groups to disengage during conflict, I conduct a placebo test using the Boko Haram insurgency in Northern Cameroon. This region is strictly governed by Civil Law and faces a religious insurgency, not institutional mismatch. If high kinship universally exacerbates the economic costs of war, I should observe a similar penalty in the North. However, if the effect is specific to the Common Law vs. Civil Law mismatch, the interaction term in the North should be null.

Columns (3) and (4) of Table 10 report the results of the same dynamic event study applied to the Boko Haram insurgency. In the absence of an institutional grievance, high kinship does not lead to economic collapse. If anything, the positive sign suggests a solidarity effect in which kin networks provide safety nets during conventional conflict. This falsification test indicates that kinship does not by itself raise the economic cost of crises. In the Anglophone case, where the state imposed incompatible institutions, the same networks sustained a defensive mobilisation that contracted the formal economy.

8 Conclusion

This paper proposes that the kinship penalty often observed in development is not an immutable cultural feature, but an endogenous outcome of institutional mismatch. The collision between rigid Civil Law codes and the reputation-based moral economy of the clan appears to generate a crisis of representation, driving citizens to strategically withdraw from the formal sphere. This finding nuances the view that traditional social structures are inherently antithetical to modern state-building. They appear to become liabilities only when the state attempts to displace them with incompatible formal rules. Survey responses and the enforced boycott of economic activity during the Anglophone Crisis suggest that this friction is a conflict over state legitimacy.

These results indicate that the path to state capacity in high-kinship societies may not lie in suppressing traditional networks, but in co-opting them. Uniform legal rules risk undermining legitimacy when they displace institutions on which communities continue to rely. By contrast, the relative success of Common Law suggests that formal institutions may function better when they leave greater scope for customary arrangements to coexist with the state. The integration of the *Kgotla* system in Botswana provides one example of how hybrid institutions can bridge local forms of governance and the formal state. Ultimately, institutions endure when they reflect the moral psychology of the people they govern.

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Appendix

A A Simple Model of Institutional Mismatch

I develop a stylised contract-theory framework (Hart, 1995) linking legal origins and kinship norms to strategic exit from the formal sector. The central idea is that rigid legal institutions can generate a mismatch when ancestral assets are embedded in lineage obligations that formal courts cannot fully process. Following the incomplete-contracts literature (Grossman and Hart, 1986; Hart and Moore, 1990), I treat the legal regime as a technology for allocating control rights when customary claims are only imperfectly legible to formal courts. In that framework, institutional mismatch arises when rigid liquidation destroys lineage value and induces strategic exit from the formal sector. Appendix A.1 sets out the baseline model, and Appendix A.2 maps it into the coefficient profile recovered from the global RD estimates. Appendix A.3 develops a local low-kinship extension, while Appendix A.4 extends the same micro-foundation to a general equilibrium model of state capacity and fiscal feedback.

A.1 Micro-Foundation: Control Rights under Contractual Incompleteness

A.1.1 The Setup

Consider a developing economy with a continuum of risk-neutral agents (entrepreneurs), indexed by their kinship intensity $k \in [0, 1]$.

The Transaction. An agent wishes to commercialise an ancestral asset (e.g., land) through the formal legal system to undertake a productive project.

- **Monetary Payoffs:** Liquidation yields V_H , while preservation yields V_L , where $0 < V_L < V_H$.
- **The Conflict:** The asset is held in trust for the agent's lineage (kinship group).

Contractual Incompleteness. The formal legal arrangement is incomplete. The customary validity of the land transfer and every future contingency regarding the lineage's needs cannot be specified ex ante. If a dispute arises (e.g., a liquidity shock or inheritance dispute), the Court determines how the asset is treated.

A.1.2 The Ex-Post Decision and Payoffs

In the event of a dispute, the Court must make a binary allocation decision $d \in \{L, P\}$.

Liquidation ($d = L$). The Court treats the asset as alienable individual property and orders its sale or partition to realise its market value (e.g., strictly applying Article 815 of the Napoleonic Code).

- **Monetary Payoff:** V_H .
- **Mismatch Cost:** The agent incurs $\mu(k)$. Since liquidation violates the moral economy of the lineage, the agent faces social sanctions (ostracism). This cost is increasing in kinship intensity:

$$\mu(k) = \gamma k, \quad \text{where } \gamma > 0. \quad (\text{A.1})$$

Preservation ($d = P$). The Court recognises the customary title (usufruct rights) and blocks liquidation, preserving the asset for the lineage (e.g., applying the *Amodu Tijani* precedent).

- **Monetary Payoff:** V_L .
- **Mismatch Cost:** 0 (no social sanction).

Efficiency Condition. Let social efficiency be defined by the aggregate surplus.

- Liquidation is efficient if $V_H - \gamma k > V_L$ (assuming the identity cost γk represents a real destruction of social capital as in [Akerlof and Kranton \(2000\)](#)).
- Preservation is efficient if $V_H - \gamma k < V_L$.

For high-kinship agents (high k), *Preservation* is the efficient outcome because the social cost of destroying the lineage's asset outweighs the marginal market gain.

A.1.3 Legal Regimes as Information Technologies

I model the legal regime $R \in \{Ci, Co\}$ as the technology the Court uses to verify the state of the world and allocate control rights.

Civil Law ($R = Ci$). The Civil Law court operates under formal rationality. It relies on bright-line rules (the Code) and treats the opaque kinship network as legally illegible.

- **Assumption:** The Civil Law court cannot (or will not) verify k . It strictly enforces formal title.
- **Decision Rule:** The Court always chooses Liquidation ($d = L$) to maximise formal certainty and market alienability.
- **Outcome:** This creates a mismatch for high-kinship agents. The Court enforces a contract that destroys social value ($-\gamma k$).

Common Law ($R = Co$). The Common Law court operates under substantive rationality. It possesses institutional porosity via mechanisms such as Repugnancy Clauses, enabling it to admit customary evidence.

- **Assumption:** The Common Law court observes kinship intensity k .
- **Decision Rule:** The Court applies an adaptable rule:

$$d(k) = \begin{cases} L & \text{if } k < k^* \text{ (Low Kinship)} \\ P & \text{if } k \geq k^* \text{ (High Kinship)} \end{cases} \quad (\text{A.2})$$

- **Outcome:** The Court effectively completes the contract by incorporating the informal norm. It prevents liquidation when social costs are high.

A.1.4 The Participation Constraint (Strategic Exit)

The agent anticipates the Court's ruling *ex ante*. The agent will only enter the formal contract if the expected utility of the formal deal exceeds the outside option (the informal sector). Let $U_{\text{Formal}}^R(k)$ be the agent's utility under legal regime R . Let $\bar{U}_{\text{Informal}}(k) = \beta k$ be the utility in the informal sector, where the clan provides insurance and protection.

Case 1: Under Civil Law (Ci). The agent anticipates that if a dispute arises, the Court will force Liquidation ($d = L$).

$$U_{\text{Formal}}^{Ci}(k) = V_H - \gamma k \tag{A.3}$$

The agent participates if and only if:

$$\begin{aligned} V_H - \gamma k &\geq \beta k \\ V_H &\geq (\beta + \gamma)k \end{aligned}$$

This defines a Civil Law participation threshold $\bar{k}_{Ci}$:

$$k \leq \frac{V_H}{\beta + \gamma} \equiv \bar{k}_{Ci} \tag{A.4}$$

Case 2: Under Common Law (Co). For high-kinship agents ($k > k^*$), the agent anticipates the Court will rule for Preservation ($d = P$).

$$U_{\text{Formal}}^{Co}(k) = V_L \tag{A.5}$$

Thus, no mismatch cost γk is incurred. The agent participates if and only if:

$$V_L \geq \beta k$$

This defines a Common Law participation threshold $\bar{k}_{Co}$:

$$k \leq \frac{V_L}{\beta} \equiv \bar{k}_{Co} \quad (\text{A.6})$$

A.1.5 The Mismatch Penalty

Assumption A.1 (Threshold Ordering). *The Common Law recognition threshold lies below the Civil Law participation threshold, $k^* \leq \bar{k}_{Ci}$, and preservation sustains participation at the recognition threshold, $V_L \geq \beta k^*$.*

Assumption A.1 ensures that the relevant margin for the comparison below is the preservation branch: agents just above $\bar{k}_{Ci}$ anticipate Preservation, not Liquidation, under Common Law. If instead $k^* > \bar{k}_{Ci}$, agents with $k \in (\bar{k}_{Ci}, k^*)$ would anticipate liquidation under both regimes and exit both formal sectors identically, so the comparison would reduce to the baseline. The second condition guarantees that the Common Law participation region is non-empty at the switch point.

Proposition A.1 (Micro-Level Mismatch). *Under Assumption A.1, if $\beta(V_H - V_L) < \gamma V_L$, the formal sector is smaller under Civil Law than under Common Law for high-kinship societies.*

Proof. Strategic exit is more severe under Civil Law if $\bar{k}_{Ci} < \bar{k}_{Co}$:

$$\frac{V_H}{\beta + \gamma} < \frac{V_L}{\beta}$$

Rearranging:

$$\beta(V_H - V_L) < \gamma V_L \quad (\text{A.7})$$

The LHS represents the market premium of Civil Law (the efficiency gain from strict liquidation, $V_H - V_L$). The RHS represents the mismatch penalty (the social cost γ scaled by the preserved value V_L). When $\beta(V_H - V_L) < \gamma V_L$, the social cost of Civil Law rigidity

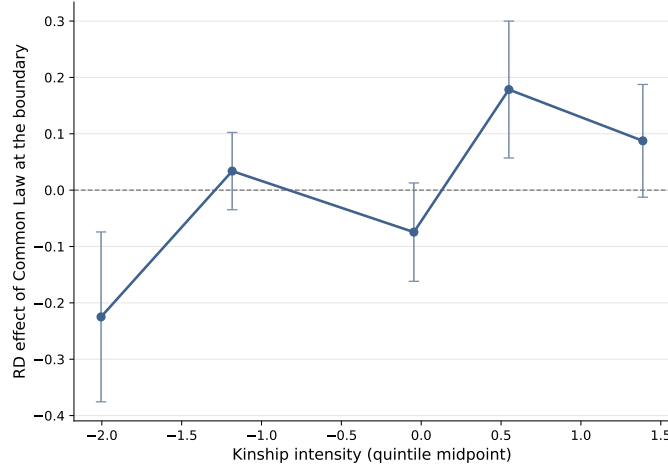


Figure A.1: Coefficient Profile by Quintile of Kinship Intensity

Notes. The figure reports spatial RD estimates for the probability of light separately by quintiles of the kinship distribution in the 200 km border sample. Points denote the estimated Common-Law advantage relative to Civil Law; vertical bars denote confidence intervals. The profile is negative at the extreme low end, approximately flat through the middle, and positive in the upper kinship region.

overwhelms its market efficiency advantages. Agents with $k \in (\bar{k}_{Ci}, \bar{k}_{Co}]$ exit the formal sector under Civil Law but, by Assumption A.1, remain integrated under Common Law. $\square$

A.2 From the Model to the RD Coefficient Profile

The median-split design remains the paper’s baseline because it preserves power and transparency. The model, however, also implies a richer coefficient profile across kinship intensity. Figure A.1 reports the quintile partition of the extensive-margin RD estimates. The pattern is negative at the extreme low end of kinship, close to zero through the middle of the distribution, and positive in the upper kinship region. This is the shape implied by the model’s mismatch logic: little difference where kinship claims are weak, a widening Common-Law advantage where rigid liquidation becomes costly, and a reversal at the extreme low end.

Figure A.2 maps those empirical magnitudes back into the model. I anchor the switching point at the empirical threshold $k^* \approx 0.29$ and scale the mismatch wedge so that the average

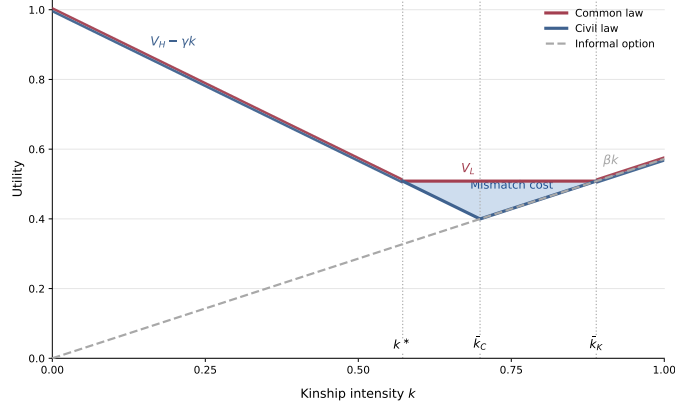


Figure A.2: Calibrated Utility Profiles in the Model

Notes. The figure anchors the switching point at the empirical calibration $k^* \approx 0.29$ and rescales the mismatch wedge so that the average Common-Law advantage over the empirically exposed region matches the paper’s preferred extensive-margin estimate. The vertical axis is renormalised to the unit interval for presentation.

Common-Law advantage over the empirically exposed region matches the paper’s preferred extensive-margin estimate. The figure should therefore be read as a stylised calibration of the model to the empirical magnitudes of the global RD coefficient profile.

Appendix A.3 extends this exercise in two directions. It develops a local low-kinship extension, under which Civil Law enjoys a small certainty premium at the extreme low end, rationalising the left-tail reversal in Figure A.1. It then places the stylised coefficient profile next to the more detailed, fixed-width 0.5-bin kinship intensity estimates.

A.3 Low-Kinship Extension and Coefficient Profiles

This subsection develops two refinements motivated by the coefficient profile in Appendix A.2. First, I introduce a local low-kinship extension under which Civil Law can enjoy a small certainty premium near the origin without altering the paper’s core comparative statics. Second, I show how the baseline mismatch model maps into the coefficient profiles recovered from finer empirical partitions of kinship intensity.

A.3.1 A Local Certainty Premium at the Extreme Low End

The baseline model in Appendix A.1 assumes that Civil Law and Common Law coincide on the liquidation branch when kinship intensity is low. The coefficient profiles in Appendix A.2 suggest a modest refinement. At the *extreme* low end of kinship, Civil Law may perform slightly better because bright-line formalism economises on verification, bargaining, and evidentiary costs precisely when customary claims are negligible.

I capture this with a local certainty premium $\pi(k)$ that is positive only near the origin:

$$\pi(k) = \delta \left(1 - \frac{k}{\underline{k}} \right) \mathbf{1}\{k \leq \underline{k}\}, \quad \delta > 0. \quad (\text{A.8})$$

Hence $\pi(0) = \delta$, the premium declines linearly, and it vanishes once kinship reaches $\underline{k}$.

Civil Law still liquidates, but now receives a small local premium:

$$U_{\text{Formal}}^{Ci}(k) = V_H - \gamma k + \pi(k). \quad (\text{A.9})$$

For $k > \underline{k}$, the premium is zero and the model returns to the baseline branch $V_H - \gamma k$.

Common Law retains the original logic:

$$U_{\text{Formal}}^{Co}(k) = \begin{cases} V_H - \gamma k & \text{if } k < k^*, \\ V_L & \text{if } k \geq k^*. \end{cases} \quad (\text{A.10})$$

The certainty premium is intentionally local. It does not overturn the main comparative statics of the model. Instead, it rationalises a small interval $[0, \underline{k}]$ on which Civil Law can weakly outperform Common Law, while preserving the central result that Common Law dominates once kinship claims become socially important. If $\underline{k}$ is small, the main participation thresholds remain governed by the baseline branches:

$$\bar{k}_{Ci} = \frac{V_H}{\beta + \gamma}, \quad \bar{k}_{Co} = \frac{V_L}{\beta}. \quad (\text{A.11})$$

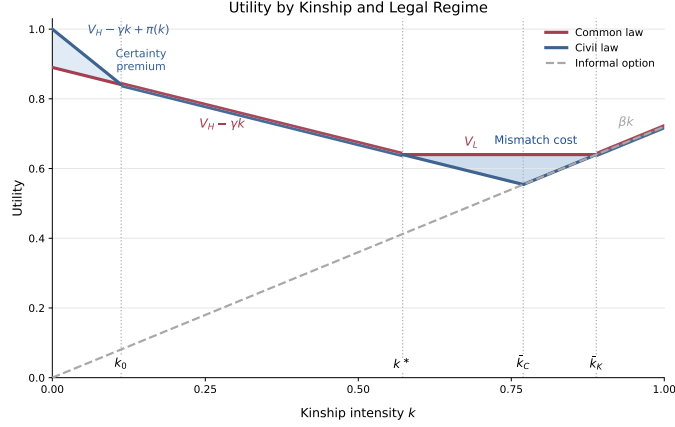
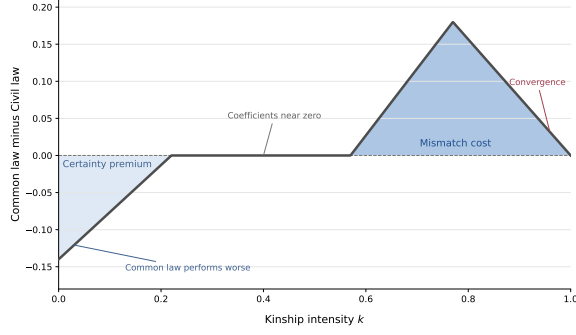


Figure A.3: Low-Kinship Certainty-Premium Extension

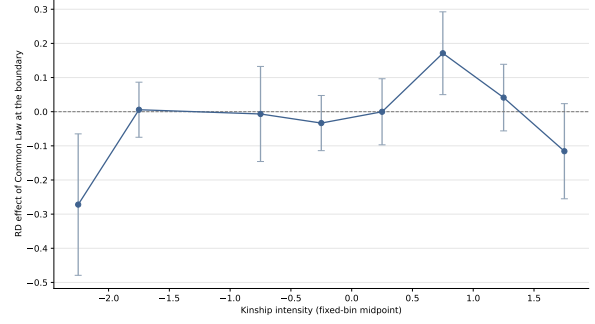
Notes. The premium shifts the Civil-Law branch slightly upward near the origin, then vanishes quickly, after which the model reverts to the baseline logic. The switching point k^* is shown at the empirical calibration used in Appendix A.2. The extension is local: it rationalises a slight Civil-Law advantage at the extreme low end of kinship without altering the model’s core mismatch region.

A.3.2 From Utility Wedges to Coefficient Profiles

The model’s empirical object is not the level of utility itself but the difference between Common Law and Civil Law across kinship intensity. Figure A.4 places the stylised coefficient profile next to the fixed-width 0.5-bin estimates. The left tail corresponds to the small certainty-premium region, the middle region is close to zero, the positive wedge corresponds to institutional mismatch, and the far right converges back toward zero as both regimes ultimately meet the informal option. The main text estimates the mismatch effect with a median split to preserve power and transparency, and Figure A.1 reports the preferred quintile partition. The right panel of Figure A.4 reports the corresponding fixed-width 0.5-bin profile as a robustness check. This specification is noisier because support is uneven across the tails, but it preserves the same broad shape: a negative low-kinship region, a flatter middle, and a positive upper-middle mismatch region.



(a) Stylised coefficient profile



(b) Fixed-width RD profile

Figure A.4: From the Model's Coefficient Profile to the Fixed-Width Bin Estimates

Notes. Panel (a) plots the stylised reduced-form difference between Common Law and Civil Law implied by the model. A negative left tail corresponds to a small Civil-Law certainty premium at the extreme low end of kinship. The positive region corresponds to the mismatch wedge in the model, and the right tail converges back toward zero as both regimes eventually meet the informal option. Panel (b) reports the corresponding fixed-width 0.5-bin RD profile for the probability of light in the 200 km border sample.

A.4 General Equilibrium

This subsection extends the contract-theory model from Appendix A.1 into general equilibrium. The micro-foundations established that legal rigidity generates a mismatch penalty when formal institutions cannot accommodate kinship-embedded claims. I now show how that same mismatch scales up into lower formal participation, a narrower tax base, and weaker state capacity.

A.4.1 The Macro Environment

Consider a continuum of risk-neutral agents indexed by kinship intensity k , distributed uniformly on $[0, 1]$. Each agent possesses an endowment y , corresponding to the expected market return V_H from Appendix A.1, and chooses between the **formal sector** (F) and the **informal sector** (I).

The Formal Sector (The State). Participation grants access to public goods G and formal legal enforcement, but agents must pay a statutory tax rate $\tau \in (0, 1)$ and bear the

mismatch cost $\theta_R k$:

$$U_F(k, R) = (1 - \tau)y + G - \theta_R k \quad (\text{A.12})$$

The Informal Sector (The Clan). Agents evade taxes but lose access to public goods and formal contract enforcement, incurring a productivity loss $\phi \in (0, 1)$. They rely on the lineage for protection and dispute resolution, with efficacy βk :

$$U_I(k) = (1 - \phi)y + \beta k \quad (\text{A.13})$$

A.4.2 Parametric Assumptions

1. **Formal Efficiency Condition:** $\phi > \tau$. In the absence of mismatch ($k = 0$), the formal sector is strictly preferred: $(1 - \tau)y > (1 - \phi)y$. Exit is driven by institutional friction and kinship dynamics, not by tax avoidance alone.
2. **Linear Functional Forms:** The kinship benefit $\beta(k) = \beta k$ ($\beta > 0$) and the mismatch cost $\mu(k, R) = \theta_R k$ are linear in kinship intensity, consistent with the micro-foundations of Appendix A.1. Civil Law imposes a strictly higher mismatch ($\theta_{Ci} \gg \theta_{Co} \geq 0$).

The small certainty premium introduced in Appendix A.3 is omitted here. Because it is local to the extreme low end of kinship, it does not alter the aggregate comparative statics that drive the general equilibrium results.

A.4.3 The Exit Threshold

An agent exits the formal sector if $U_I(k) > U_F(k, R)$. The indifference condition defines a unique threshold κ (distinct from the court's recognition threshold k^* in Appendix A.1):

$$(1 - \phi)y + \beta \kappa = (1 - \tau)y + G - \theta_R \kappa \quad (\text{A.14})$$

Rearranging, the agent exits when the total return to kinship exceeds the net premium of the state:

$$\underbrace{(\beta + \theta_R)\kappa}_{\text{Incentive to Exit}} = \underbrace{G + (\phi - \tau)y}_{\text{State Premium}}$$

Solving for the threshold:

$$\kappa = \frac{G + (\phi - \tau)y}{\beta + \theta_R} \quad (\text{A.15})$$

Since the incentive to exit is strictly increasing in k , all agents with $k > \kappa$ strategically withdraw to the informal sector.

A.4.4 The Fiscal Feedback Loop

In the partial equilibrium above, public goods G were exogenous. I now endogenise state capacity by assuming a balanced budget: public goods provision equals total tax revenue. Given the uniform distribution on $[0, 1]$, the mass of the formal sector is κ (since density equals 1), so:

$$G(\kappa) = \tau y \kappa \quad (\text{A.16})$$

Substituting (A.16) into the indifference condition (A.14) introduces a feedback loop: exit reduces the tax base, which reduces G , which prompts further exit.

$$(1 - \phi)y + \beta\kappa = (1 - \tau)y + \tau y \kappa - \theta_R \kappa$$

Solving for the equilibrium threshold under endogenous state capacity:

$$\kappa = \frac{(\phi - \tau)y}{\beta + \theta_R - \tau y} \quad (\text{A.17})$$

Assumption A.2 (Interior Solution). *An interior equilibrium with $\kappa \in (0, 1)$ satisfies:*

$$\beta + \theta_R > \phi y \quad (\text{A.18})$$

Derivation. Let $\Delta U(k, R) = U_F(k, R) - U_I(k)$. Substituting the endogenous budget constraint:

$$\Delta U(k, R) = (\phi - \tau)y - (\beta + \theta_R - \tau y)k$$

The intercept $(\phi - \tau)y > 0$ by the formal efficiency assumption. An interior threshold also requires $\Delta U(1, R) < 0$:

$$\Delta U(1, R) = \phi y - (\beta + \theta_R) < 0$$

Hence, $\beta + \theta_R > \phi y$. Because $\phi > \tau$, this condition also ensures that the denominator in Equation (A.17) is positive. If it fails, no interior exit threshold lies in $[0, 1]$. $\square$

A.4.5 Extension: The Leviathan State (Endogenous Taxation)

I extend the model to allow the state to set the tax rate strategically. A Leviathan state maximises revenue:

$$\max_{\tau} R(\tau) = \tau y \cdot \kappa(\tau) = \tau y \left[\frac{(\phi - \tau)y}{\beta + \theta_R - \tau y} \right] \quad (\text{A.19})$$

Let $S = \beta + \theta_R$ denote the total social barrier to formalisation. The first-order condition yields the revenue-maximising tax rate:

$$\tau^* = \frac{S - \sqrt{S(S - \phi y)}}{y} \quad (\text{A.20})$$

The Institutional Laffer Curve. The optimal tax rate τ^* and maximum revenue $R(\tau^*)$ are endogenous to the legal mismatch θ_R . As mismatch increases ($S \uparrow$), the elasticity of the tax base with respect to taxation rises. This shifts the Laffer curve inward, forcing the state to accept a strictly lower revenue ceiling regardless of its fiscal policy. State capacity is therefore structurally constrained by institutional mismatch (θ_R), rather than by administrative investment or policy choices (τ) alone.

Remark: The Benevolent Planner. The core insight holds for a welfare-maximising planner. Since public goods must be funded via taxation, and taxation triggers the participation constraint, any planner faces the same structural limit: high mismatch θ_R tightens the feasible budget set, reducing equilibrium public goods regardless of the state's objective.

Interior Tax Solution. Under Assumption A.2, Equation A.20 gives an interior revenue-maximising tax rate. When $S \leq \phi y$, the interior expression does not apply; over the maintained domain $\tau < \phi$, revenue is increasing up to the boundary.

A.4.6 Theoretical Propositions

Proposition A.2 (The Mismatch Penalty). *The aggregate size of the formal sector κ is strictly decreasing in the legal mismatch parameter θ_R .*

Proof. Differentiating Equation (A.17) with respect to θ_R :

$$\frac{\partial \kappa}{\partial \theta_R} = -\frac{\kappa}{\beta + \theta_R - \tau y} < 0$$

The denominator is positive by the interior-solution condition (Assumption A.2). Since $\theta_{Ci} > \theta_{Co}$, it follows that $\kappa_{Ci} < \kappa_{Co}$: high-kinship societies under Civil Law exhibit lower formal economic activity than identical societies under Common Law. $\square$

Proposition A.3 (The Fiscal Multiplier). *The negative impact of institutional mismatch is amplified by the fiscal feedback loop. The decline in the formal sector is strictly larger when public goods are endogenous than when they are exogenous.*

Proof. Let κ_{PE} denote the threshold under exogenous public goods (from Equation A.15) and κ_{GE} the threshold under endogenous public goods (from Equation A.17). The responses

to a mismatch shock are:

$$\frac{\partial \kappa_{PE}}{\partial \theta_R} = -\frac{\kappa_{PE}}{\beta + \theta_R} \quad (\text{A.21})$$

$$\frac{\partial \kappa_{GE}}{\partial \theta_R} = -\frac{\kappa_{GE}}{\beta + \theta_R - \tau y} \quad (\text{A.22})$$

Evaluating both at the same initial equilibrium ($\kappa_{PE} = \kappa_{GE} = \kappa$), the fiscal multiplier is:

$$M = \frac{\partial \kappa_{GE} / \partial \theta_R}{\partial \kappa_{PE} / \partial \theta_R} = \frac{\beta + \theta_R}{\beta + \theta_R - \tau y} = \frac{1}{1 - \frac{\tau y}{\beta + \theta_R}} \quad (\text{A.23})$$

Since $\tau y > 0$ and Assumption A.2 implies $\beta + \theta_R > \tau y$, the fraction $\frac{\tau y}{\beta + \theta_R} \in (0, 1)$, so $M > 1$. The impact of a mismatch shock is magnified: the state contraction is multiplicative, driven by the revenue-loss feedback. $\square$

Proposition A.4 (The Legitimacy Gap). *Trust in state institutions, defined as the net utility premium $T(k, R) = U_F(k, R) - U_I(k)$, is strictly decreasing in mismatch θ_R .*

Proof.

$$T(k, R) = \underbrace{[G - (\tau - \phi)y]}_{\text{Base Premium}} - \underbrace{(\beta + \theta_R)k}_{\text{Kinship/Mismatch Penalty}}$$

Differentiating: $\frac{\partial T}{\partial \theta_R} = -k < 0$. Under Civil Law, the penalty for interacting with the state rises strictly with kinship intensity, explaining the lower trust in courts observed in the Afrobarometer results (Section 5.2). $\square$

Proposition A.5 (The Capacity Trap). *Total equilibrium state revenue R^* is strictly decreasing in mismatch cost θ_R . If the state faces a fixed operating cost F , high mismatch can render the state unviable.*

Proof. Part 1: Revenue Decay. Substituting the equilibrium κ from Equation (A.17) into the revenue definition:

$$R(\tau, \theta_R) = \frac{\tau y^2(\phi - \tau)}{\beta + \theta_R - \tau y} \quad (\text{A.24})$$

Let τ^* be the optimal tax rate from Equation (A.20). By the Envelope Theorem:

$$\frac{dR^*}{d\theta_R} = \left. \frac{\partial R}{\partial \theta_R} \right|_{\tau^*} = -\frac{\tau y^2(\phi - \tau)}{(\beta + \theta_R - \tau y)^2} < 0$$

since $\phi > \tau$ and $\tau > 0$.

Part 2: The Viability Threshold. Assume the state requires a fixed cost $F > 0$ to operate. Since $R^*(\theta_R)$ is continuous and strictly decreasing with $R^*(0) > F$ and $\lim_{\theta_R \rightarrow \infty} R^* = 0$, the Intermediate Value Theorem guarantees a unique $\bar{\theta}$ such that $R^*(\bar{\theta}) = F$. If $\theta_R > \bar{\theta}$, the state cannot cover fixed costs and formal institutions collapse ($G \rightarrow 0$). $\square$

Corollary A.1 (The Interaction Effect). *The marginal impact of kinship intensity on formal sector activity is strictly more negative under Civil Law than under Common Law.*

Proof. Abstracting from constants and FEs, the empirical interaction specification can be written as

$$y_{ic} = \pi_1 \text{Kinship}_i + \pi_2 (\text{Kinship}_i \times \text{CommonLaw}_c) + \varepsilon_{ic},$$

where π_1 and π_2 denote reduced-form coefficients. The net utility premium of formal participation is:

$$\Delta U(k, R) = (\phi - \tau)y - (\beta + \theta_R - \tau y)k \quad (\text{A.25})$$

The slope with respect to k is $-(\beta + \theta_R - \tau y)$, and:

$$\frac{\partial}{\partial \theta_R} \left(\frac{\partial \Delta U(k, R)}{\partial k} \right) = -1$$

Under Civil Law ($\theta_{Ci} \gg 0$), the slope is strongly negative; under Common Law ($\theta_{Co} \approx 0$), the slope is less negative. The interaction effect is:

$$\text{Slope}_{Co} - \text{Slope}_{Ci} = \theta_{Ci} - \theta_{Co} > 0$$

This matches the positive interaction coefficient ($\pi_2 > 0$) observed in the data. $\square$

B Data Construction

B.1 Variable Definitions and Data Sources

Table B.1: Variable Definitions and Data Sources

Variable		Description	Source
<i>Panel A: Economic Outcomes</i>			
Log Luminosity	Nighttime	Natural logarithm of average visible light density in the grid cell, calculated as $\ln(\text{light} + 0.00001)$ (Intensive Margin).	Elvidge et al. (2021)
Probability of Light		Binary indicator equal to 1 if the grid cell emits any detectable nighttime light (Extensive Margin).	Elvidge et al. (2021)
<i>Panel B: Institutional and Cultural Variables</i>			
Kinship Index (KII)	Intensity	Standardised first principal component of five traits: cousin marriage, polygamy, extended family co-residence, lineage organisation, and community segmentation.	Murdock (1967); Schulz et al. (2019)
Legal Origin		Indicator for whether the country's legal system is based on Common Law or Civil Law.	La Porta et al. (1999)

Continued on next page

Table B.1 – continued from previous page

Variable	Description	Source
Jurisdictional Hierarchy	Measure of pre-colonial political centralisation (levels of jurisdictional hierarchy beyond the local community), ranging from 0 to 4.	Murdock (1967)
<i>Panel C: Survey Mechanisms (Afrobarometer)</i>		
State Trust Index	Standardised average of trust in 10 formal institutions (see Appendix B.4).	Afrobarometer (2024)
Political Participation Index	Composite index of civic engagement: attending community meetings, joining others to raise an issue, and attending demonstrations (see Appendix B.5).	Afrobarometer (2024)
Trust in Traditional Leaders	Respondent’s reported trust in traditional chiefs/leaders (Standardised).	Afrobarometer (2024)
Relative Trust	Difference between standardised Trust in Courts and standardised Trust in Traditional Leaders.	Afrobarometer (2024)
<i>Panel D: Conflict (Cameroon)</i>		
Anglophone Crisis Events	Conflict events involving separatist actors in Anglophone regions (see Appendix B.3).	ACLED (2024)
Continued on next page		

Table B.1 – continued from previous page

Variable	Description	Source
Boko Haram Events	Conflict events involving Islamist insurgents in the Far North region (see Appendix B.3).	ACLED (2024)
Violence Against Civilians	Subset of conflict events classified as direct violence against non-combatants (used as a proxy for strike enforcement).	ACLED (2024)
War	Subset of conflict events classified as battles and as explosions (used as a proxy for collateral damage).	ACLED (2024)
<i>Panel E: Geographic Controls</i>		
Elevation	Average elevation of the grid cell in metres.	WorldClim
Rainfall	Annual average rainfall in the grid cell in millimetres.	WorldClim
Temperature	Annual average temperature in the grid cell in degrees Celsius.	WorldClim
Ruggedness	Terrain Ruggedness Index (TRI) calculated for the grid cell.	Nunn and Puga (2012)
Distance to Coast	Geodesic distance from the grid cell centroid to the nearest coastline (km).	QGIS
Continued on next page		

Table B.1 – continued from previous page

Variable	Description	Source
Distance to Rivers/Lakes	Geodesic distance from the grid cell centroid to the nearest river or lake (km).	QGIS
Agricultural Suitability	Index of land suitability for agriculture based on soil and climate constraints.	Atlas of the Biosphere
Malaria Suitability	Index of climatic suitability for malaria transmission.	Kiszewski et al. (2004)
Population Density	Population count per grid cell used for per-capita adjustments.	LandScan
<i>Panel F: Textual Data (NLP Analysis)</i>		
Common Law Corpus (JCPC)	Full text of 167 judgments from the Judicial Committee of the Privy Council (1890–1974), restricted to Sub-Saharan African appeals.	(British and Irish Legal Information Institute, 2025)
Civil Law Corpus (RJCB)	Full text of the <i>Revue Juridique du Congo Belge</i> (1924–1963), comprising 38 volumes of colonial jurisprudence and doctrine.	(Droit Congolais, 2025)

Notes. This table summarises the primary variables used in the empirical analysis. The unit of observation for economic and geographic variables is a $0.125^\circ \times 0.125^\circ$ grid cell for the global analysis and a $0.05^\circ \times 0.05^\circ$ grid cell for the Cameroon analysis. Survey variables are defined at the individual respondent level and grouped in geocoded clusters.

B.2 Computational Analysis of Colonial Legal Semantics

This appendix details the corpus construction, text cleaning pipeline, and two complementary embedding architectures underlying the results in Section 6. The first approach (Word2Vec) maps the semantic topology of each legal tradition separately, producing the cosine similarity results in Section 6.2. The second approach (Cross-Lingual Transformer) projects English and French text into a shared vector space, producing the Legitimacy Axis scores and regressions in Section 6.3.

B.2.1 Corpus Construction

The analysis relies on two corpora representing the apex of colonial adjudication, standardised into a unified format:

- **Common Law (JCPC).** The corpus comprises the complete digitised records of the Judicial Committee of the Privy Council (1890–1974) restricted to Sub-Saharan African appeals. As the final court of appeal for the British Empire, these 167 cases constitute the authoritative doctrine on the integration of native title. The original digital archive consisted of 493 fragmented documents (separate files for appellant submissions, respondent submissions, and judgments within the same case). I reconstructed unified case files by matching fragments on case title and year.
- **Civil Law (RJCB).** The corpus is sourced from the *Revue Juridique du Congo Belge* (1924–1963). Given the Civil Law tradition’s reliance on doctrine over pure precedent, this source is a hybrid collection containing both jurisprudence (court rulings) and doctrine (administrative commentary). The raw data consist of 38 digitised volumes (the first three volumes each span two calendar years, and the 1939 volume is missing). To recover individual legal decisions from the continuous text stream, I applied a segmentation algorithm that splits volumes at French legal headers (e.g., *TRIBUNAL*,

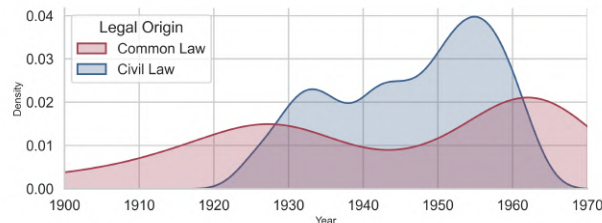


Figure B.1: Temporal Distribution of JPC and RJC Court Cases

ARRÊT, *CONSEIL*) followed by a valid citation year, yielding $N = 1,448$ discrete case units.

As shown in Figure B.1, the two corpora overlap in time from 1924 to 1963, ensuring that semantic differences reflect legal tradition rather than historical era.

B.2.2 Pre-processing and Text Normalisation

Both corpora were digitised from archival scans comprising over 25,000 pages. The raw OCR output required two cleaning steps before embedding:

1. **De-hyphenation and OCR Repair.** Archival scanning introduces systematic “shattering”: words split across line breaks or layout columns (e.g., *juris- diction* $\rightarrow$ *jurisdiction*). I applied regex-based de-hyphenation and a targeted correction dictionary for recurring entity misreadings (e.g., *Privy Coun- cil* $\rightarrow$ *Privy Council*; *ordon nance* $\rightarrow$ *ordonnance*).
2. **Ghost Filtering.** Reconstructed segments containing fewer than 50 tokens, typically table-of-contents entries, metadata stubs, or scanning noise, were excluded to prevent low-information fragments from diluting the embedding space.

B.2.3 Word Embedding Analysis (Word2Vec)

The semantic topology results in Section 6.2 are based on separate Word2Vec (Skip-Gram) models trained on each corpus. This approach captures the internal associative structure of each legal tradition independently.

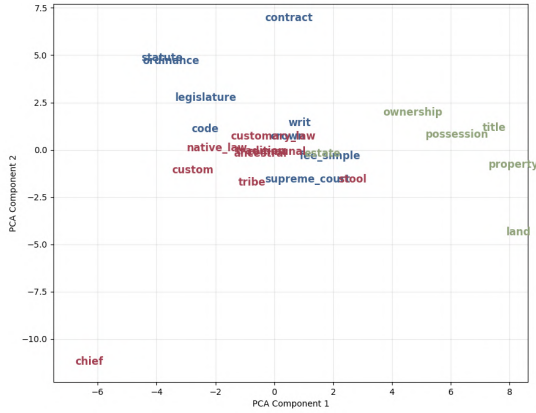
Architecture. Each model maps words into a 150-dimensional vector space where geometric proximity represents semantic similarity. I set a wide context window ($w = 10$) to capture thematic associations between words appearing in the same paragraph, appropriate for legal argumentation where key concepts are linked across sentences rather than through strict syntactic adjacency. Words appearing fewer than 5 times are excluded.

Dictionary Construction. To identify the semantic fields of *Custom*, *Land*, and *Law*, I started with seed terms reflecting each tradition’s vocabulary (e.g., *customary*, *stool*, *chief* for the JCPC; *coutume*, *indigène*, *chefferie* for the RJCB). I iteratively refined these dictionaries by examining nearest neighbours in the trained vector space, adding terms that appeared in the top-20 neighbourhood of the seeds while discarding false positives.

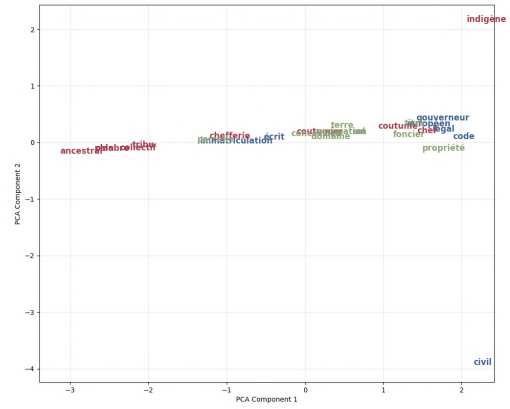
Cosine Similarity Metric. I measure the *Integration Gap* as the cosine similarity between the centroid vector of *Custom* terms and the centroid vector of *Land* terms in each corpus. Higher similarity indicates greater porosity (i.e., the legal system’s capacity to recognise customary tenure as a form of property).

Semantic Maps. Figure B.2 visualises the first two principal components from a principal component analysis (PCA) of the 150-dimensional embedding space for each corpus. The topological patterns align with the paper’s core hypotheses: the Civil Law corpus displays a large semantic distance between state concepts (e.g., *code*, *gouverneur*) and the indigenous population (e.g., *indigène*, *chefferie*), reflecting the rigid institutional dualism of the Belgian administration. Conversely, the Common Law corpus positions the *Chief* as a distinct, autonomous semantic entity that overlaps with both customary and formal legal concepts, consistent with the parallel authority structures characterising Indirect Rule.

Bootstrap Validation. To ensure the observed divergence is not an artefact of sampling, I re-sampled each corpus 100 times with replacement and re-trained the Word2Vec model on



(a) Common Law semantic topology



(b) Civil Law semantic topology

Figure B.2: The Semantic Topology of Colonial Law

Notes. The figure visualises the first two principal components from a principal component analysis (PCA) of the 150-dimensional word embedding models trained separately on each corpus. Words are coloured by semantic category: *Formal Law* (blue), *Customary/Kinship* (red), and *Land/Property* (green). In panel (a), customary terms spatially overlap with formal legal concepts, while property forms a distinct cluster. In panel (b), customary and formal themes remain separated, while property overlaps with formal law.

each bootstrap draw. The resulting distributions of cosine similarity scores between *Custom* and *Land* are shown in Figure 10 (main text). The Common Law and Civil Law distributions are non-overlapping ($p < 0.01$), confirming that the divergence in the semantic integration of customary tenure is a robust structural feature of the legal traditions rather than a product of corpus composition.

B.2.4 Cross-Lingual Transformer Analysis

The Legitimacy Axis results in Section 6.3 require a methodology that can compare English and French legal texts in a shared semantic space. For this, I employ the *paraphrase-multilingual-mpnet-base-v2* Sentence Transformer (Reimers and Gurevych, 2019), a pre-trained model that maps text in 50+ languages into a common 768-dimensional vector space. This architecture enables direct cross-lingual comparison without translation.

Context Window Segmentation. Unlike Word2Vec, which operates on individual words, the Transformer encodes full text passages. I segment each case into sliding windows of 300 words with a 50-word overlap. This window size captures sufficient local semantic context, generally a judge’s complete argument on a specific point of custom or property, while remaining within the token limits of the architecture. Segments shorter than 20 words after cleaning are discarded.

Legitimacy Axis Construction. I construct the normative dimension from two bilingual sets of anchor terms. The *Legitimacy* pole comprises terms associated with the recognition of customary law (e.g., *valid*, *coutume*, *recognised*, *droit indigène*). The *Repugnancy* pole comprises terms associated with the rejection of custom (e.g., *repugnant*, *ordre public*, *void*, *sauvage*). I encode both keyword sets using the same Transformer model, compute the centroid of each set, and define the axis as the normalised difference vector (Equation 4 in the main text). Each 300-word segment is then scored as its scalar projection onto this axis, with higher values indicating greater proximity to the legitimacy concept.

Case-Level Aggregation. The unit of observation for the regression analysis (Table 9) is the legal case, not the text segment. I aggregate segment-level scores to the case level by taking the mean Legitimacy Score across all windows belonging to a given case.

Topic Classification. To isolate disputes over land and customary tenure from unrelated criminal or procedural matters, I employ automated topic classification using a bilingual keyword dictionary. Cases are classified as *Land & Property* if any of their first ten text segments contain domain-specific terms (e.g., *land*, *title*, *ownership*, *customary*, *stool* for Common Law; *propriété*, *terrain*, *terre* for Civil Law). This restriction is applied in Columns 4–6 of Table 9.

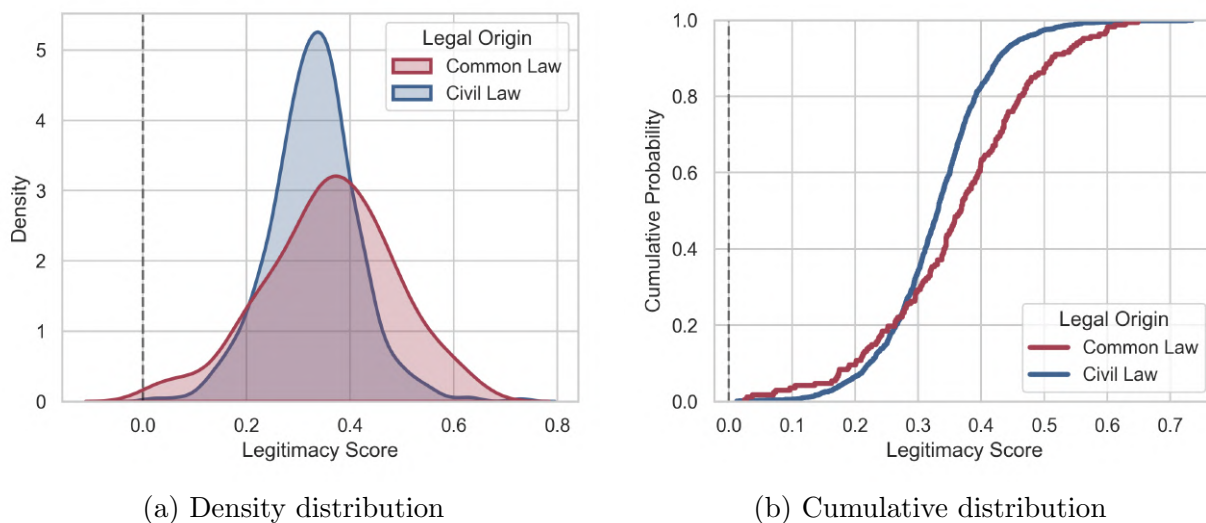


Figure B.3: The Distributional Divergence in Colonial Legal Legitimacy

Notes. Panel (a) displays the kernel density of Legitimacy Scores for the full sample ($N = 1,615$), showing a systematic shift toward validity in the Common Law (red) relative to Civil Law (blue). Panel (b) displays the cumulative distribution.

Distributional Results. Figure B.3 displays the density and cumulative distributions of case-level Legitimacy Scores. Common Law cases are systematically shifted toward the validity pole relative to Civil Law cases, with a higher mean and greater variance. The distributional gap is visible across the full support, indicating that the divergence is not driven by a small number of outlier cases.

B.3 Conflict Actor Classification

To validate the mechanism of strategic withdrawal, it is helpful to distinguish between state-enforced order, separatist insurgency, and external destabilisation. I utilise the *Armed Conflict Location & Event Data Project* (ACLED) to classify conflict events based on the actors involved.

I construct three binary indicators (*State Forces*, *Separatist Rebels*, and *Boko Haram*) by searching for specific keywords in both the **Actor 1** and **Actor 2** fields of the ACLED event logs. This dual-field search ensures I capture all events where a specific group was either the

perpetrator or the target. The classification logic is defined as follows:

1. State Forces (Civil Law Enforcers)

Events are classified as involving State Forces if the actor name contains references to the formal security apparatus of the central government. This includes both regular forces and elite units often deployed in the Anglophone regions:

- **Regular Forces:** “Military Forces of Cameroon”, “Police Forces of Cameroon”, “Government of Cameroon”.
- **Elite Units:** “Rapid Intervention” (capturing the *Bataillon d’Intervention Rapide* or BIR), “Gendarmerie”.

2. Separatist Rebels (Ambazonia)

Events are classified as involving Separatist Rebels if the actor name corresponds to known armed groups advocating for the secession of the Anglophone regions. This classification captures both the umbrella movements and specific local militias identified in the dataset:

- **Umbrella Terms:** “Ambazonia”, “Southern Cameroons”.
- **Specific Militias:** “SOCADEF” (Southern Cameroons Defence Forces), “SCACUF” (Southern Cameroons Ambazonia Consortium United Front), “Red Dragon”, “Tigers”.

3. Boko Haram / ISWAP (Placebo)

Events are classified as involving the Islamist insurgency in the Far North (Civil Law zone) to serve as a placebo test for general conflict instability. This category includes:

- **Insurgents:** “Boko Haram”, “Islamic State” (capturing ISWAP factions).

B.4 State Trust Index Construction

The *State Trust Index* serves as the primary measure of institutional legitimacy. It is constructed as the unweighted average of ten distinct components from the Afrobarometer survey (Round 8).

To ensure comparability and equal weighting across institutions, each component is first standardised (z-scored) to have a mean of zero and a standard deviation of one across the sample before aggregation. The index is composed of the following ten elements:

- **Trust in President** (Source: Q41A)
- **Trust in Parliament** (Source: Q41B)
- **Trust in Electoral Commission** (Source: Q41C)
- **Trust in Local Council** (Source: Q41D)
- **Trust in Ruling Party** (Source: Q41E)
- **Trust in Opposition Political Parties** (Source: Q41F)
- **Trust in Police** (Source: Q41G)
- **Trust in Army** (Source: Q41H)
- **Trust in Courts of Law** (Source: Q41I)
- **Trust in Tax Office** (Source: Q41J)

B.5 Political Participation Index Construction

The *Political Participation Index* captures formal civic engagement. It is constructed as the unweighted average of three standardised components from the Afrobarometer survey (Round 8):

- **Attended a Community Meeting** (Source: Q11A)
- **Joined Others to Raise an Issue** (Source: Q11B)
- **Attended a Demonstration or Protest March** (Source: Q11C)

Each component is first standardised to have a mean of zero and a standard deviation of one before aggregation. The index is the unweighted average of these standardised components and is not itself re-standardised; because the components are positively but imperfectly correlated, its standard deviation is 0.72 rather than one.

B.6 Summary Statistics

Kinship Index Construction. Tables [B.2](#) and [B.3](#) validate the internal consistency of the Kinship Intensity Index (KII). Table [B.2](#) reports summary statistics and the correlation matrix for the five constituent traits. Table [B.3](#) confirms via Principal Component Analysis that a single latent factor explains the majority of the variance, justifying the use of the first principal component as the index.

Table B.2: Summary Statistics and Correlations of Kinship Components

Panel A: Summary Statistics					
Variable	Mean	SD	Min	Max	Obs
(1) Cousin Marriage Preference	0.53	1.07	0	3	678,666
(2) Polygamy	0.69	0.86	0	2	697,061
(3) Co-residence of Families	1.35	0.54	0	2.5	700,924
(4) Lineage Organisation	0.49	0.50	0	1	699,494
(5) Community Organisation	0.33	0.47	0	1	667,073
(6) Kinship Intensity Index (KII)	-0.75	1.25	-2.46	2.13	701,255
Panel B: Correlation Matrix					
	(1)	(2)	(3)	(4)	(5)
(1) Cousin Marriage Preference	1.00				
(2) Polygamy	0.36	1.00			
(3) Co-residence of Families	0.10	0.36	1.00		
(4) Lineage Organisation	0.44	0.68	0.53	1.00	
(5) Community Organisation	0.26	0.47	0.47	0.61	1.00
(6) Kinship Intensity Index (KII)	0.58	0.80	0.64	0.90	0.75

Notes. Panel A reports summary statistics for the subcomponents of the Kinship Intensity Index (reported in their raw, unstandardised scales for interpretability) and the final Kinship Intensity Index (reported in the standardised units used in the regression analysis). Panel B reports pairwise Pearson correlation coefficients for the subcomponents of the Kinship Intensity Index, using the standardised versions of all subcomponents. The unit of observation is a grid cell ($0.125^\circ \times 0.125^\circ$). The strong positive correlations between the index and its components, combined with a Cronbach's alpha of 0.80, confirm the internal consistency of the index.

Global Spatial Sample (200 km Bandwidth). Table B.4 presents summary statistics for the primary spatial sample, restricted to grid cells within 200 km of a legal origin boundary. Panel A documents the economic gap between Civil Law and Common Law jurisdictions in this border region. Panel B shows that Kinship Intensity does not differ significantly across the boundary ($p > 0.10$), supporting the validity of the border design.

Afrobarometer Mechanism Sample. Table B.5 summarises the individual-level micro-data used to test the mechanism of strategic exit. The sample comprises 15,770 respondents located within 43 partitioned ethnic homelands. Panel A reports demographic characteristics; Panels B and C report baseline levels for the key outcome variables.

Table B.3: Principal Component Analysis of Kinship Subcomponents

Panel A: Eigenvalues		
Component	Eigenvalue	Proportion of Variance
Component 1	2.81	0.562
Component 2	0.90	0.180
Component 3	0.55	0.111
Component 4	0.48	0.096
Component 5	0.25	0.050
Panel B: First Component Loadings (Eigenvectors)		
Variable	Loading	Unexplained Variance
Lineage Organisation	0.54	0.18
Polygamy	0.47	0.38
Community Organisation	0.47	0.39
Co-residence of Families	0.40	0.54
Cousin Marriage Preference	0.33	0.69

Notes. The table reports the results of a Principal Component Analysis (PCA) performed on the five standardised subcomponents of the Kinship Intensity Index ($N = 655,888$). Panel A shows that only the first component has an eigenvalue greater than 1 (satisfying the Kaiser criterion) and explains 56% of the total variation. Panel B reports the eigenvectors for the first component, demonstrating that all subcomponents contribute positively and substantially to the single latent trait. Column (2) of Panel B lists the unexplained variance share ($1 - \lambda_1 \cdot \text{Loading}^2$), where λ_1 is the first eigenvalue, representing the share of each variable's variance not captured by the first component.

Table B.4: Global Summary Statistics by Legal Origin (200 km Bandwidth)

Variable	(1) Full Sample Mean/SD	(2) Civil Law Mean/SD	(3) Common Law Mean/SD	(4) Difference (2)-(3)
Panel A: Economic Outcomes				
Probability of Light	0.56 (0.50)	0.50 (0.50)	0.63 (0.48)	-0.136* [0.074]
Log Nighttime Luminosity	-8.14 (5.54)	-9.23 (5.00)	-6.87 (5.85)	-2.358** [1.028]
Panel B: Institutional Inputs				
Kinship Intensity Index	-0.15 (0.71)	-0.11 (0.70)	-0.19 (0.73)	0.088 [0.120]
Jurisdictional Hierarchy	3.17 (1.35)	3.19 (1.40)	3.15 (1.28)	0.043 [0.462]
Panel C: Geographic Controls				
Absolute Latitude	18.75 (9.41)	18.93 (10.16)	18.53 (8.44)	0.402 [2.996]
Elevation (m)	1115.5 (1439.4)	1512.1 (1763.0)	655.7 (690.7)	856.3 [685.9]
Ruggedness Index	133.7 (182.0)	159.8 (201.5)	103.4 (150.9)	56.4 [46.8]
Distance to Coast (km)	558.31 (452.48)	625.73 (480.28)	480.14 (404.12)	145.591 [105.501]
Distance to River (km)	210.1 (323.5)	151.6 (274.5)	277.9 (360.6)	-126.3 [83.4]
Temperature (°C)	21.05 (9.02)	18.47 (10.83)	24.05 (4.78)	-5.578 [4.099]
Precipitation (mm)	840.1 (844.2)	886.6 (898.2)	786.3 (773.4)	100.2 [188.1]
Agricultural Suitability	0.96 (0.04)	0.96 (0.04)	0.96 (0.04)	0.005 [0.004]
Malaria Suitability	5.73 (7.78)	5.71 (7.37)	5.76 (8.22)	-0.054 [1.886]
Observations (Grid Cells)	78,370	42,075	36,295	78,370

Notes. The table reports summary statistics for the 200 km bandwidth grid-cell sample used in the OLS estimation. Columns 1-3 report means with standard deviations in parentheses. Column 4 reports the difference in means between Civil Law and Common Law jurisdictions, estimated via OLS with standard errors clustered two-way at the country and ethnolinguistic family level reported in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Summary Statistics: Afrobarometer Mechanism Sample

Variable	Mean	SD	Min	Max	Obs
Panel A: Respondent Characteristics					
Age (Years)	36.32	14.52	18.00	112.00	15,770
Female (=1)	0.50	0.50	0.00	1.00	15,770
Education Level (0-9)	3.39	2.18	0.00	9.00	15,749
Lived Poverty Index (0-4)	1.29	0.90	0.00	4.00	15,766
Panel B: State Legitimacy & Representation					
State Trust Index	1.50	0.78	0.00	3.00	15,685
Trust in Courts	1.58	1.09	0.00	3.00	15,117
Trust in Parliament	1.38	1.10	0.00	3.00	15,093
Trust in Police	1.40	1.14	0.00	3.00	15,555
Trust in Army	1.91	1.10	0.00	3.00	15,421
Trust in Tax Office	1.38	1.06	0.00	3.00	14,622
Trust in Electoral Comm.	1.46	1.13	0.00	3.00	15,058
Trust in Ruling Party	1.49	1.18	0.00	3.00	15,197
Trust in Opposition	1.14	1.05	0.00	3.00	15,126
MP Listens	0.58	0.80	0.00	3.00	15,281
MP Performance	2.29	0.94	1.00	4.00	14,686
Panel C: Civic Engagement					
Political Particip. Index	0.44	0.33	0.00	1.00	15,770
Attended Comm. Meeting	0.63	0.48	0.00	1.00	15,770
Joined Others to Raise Issue	0.55	0.50	0.00	1.00	15,770
Attended Demonstration	0.14	0.35	0.00	1.00	15,770
Panel D: Local Intermediaries					
Trust in Trad. Leaders	1.85	1.08	0.00	3.00	15,208
Trad. Leader Influence	3.00	0.71	1.00	5.00	15,487
Trad. Leader Corruption	1.13	0.84	0.00	3.00	14,645
Trust in Local Council	1.42	1.07	0.00	3.00	14,923
Local Cncl. Listens	0.78	0.91	0.00	3.00	15,230
Local Cncl. Performance	2.42	0.94	1.00	4.00	13,003
Relative Trust (Courts-Chiefs)	-0.26	1.12	-3.00	3.00	14,707

Notes. The table reports summary statistics for the Afrobarometer respondents included in the main RDD specification. The sample is restricted to individuals residing within the 200 km bandwidth of the legal origin border in partitioned ethnic homelands. Relative Trust is calculated as Trust in Courts minus Trust in Traditional Leaders. Educational attainment is coded on a ten-category ordinal scale ranging from 0 to 9. Lived Poverty Index is an average of shortages of basic necessities (0=Never, 4=Always).

C Extended Robustness

C.1 Local OLS Estimates

The main RD strategy discretises social structure into binary ‘High’ and ‘Low’ categories via a median split. To verify that this functional form does not drive the results, I re-estimate the baseline OLS specification using the continuous Kinship Intensity Index within the restricted 200 km spatial sample. As reported in Table C.1, the local estimates are virtually unchanged from the global benchmark: the negative relationship between kinship and development is concentrated under Civil Law, while the interaction with Common Law remains positive and statistically significant. The mismatch penalty persists when modelling the interaction continuously within the border region.

C.2 Full Sample RD Estimates

Tables C.2 and C.3 present the RDD estimates for the unrestricted global sample, including all partitioned ethnic homelands within the 200 km bandwidth. As reported in Table 3 of the main text, the Low Kinship group fails the multidimensional balance check, while the High Kinship sample displays reasonable balance.

In this full sample, the High Kinship results match the main findings: Common Law assignment is associated with large and statistically significant increases in economic activity. The Low Kinship estimates are small in magnitude and statistically indistinguishable from zero.

The Low Kinship estimates are confounded by the China-India border, which coincides with the Himalayas rather than an arbitrary colonial boundary. This topographic discontinuity creates a mechanical advantage for the Common Law side (India, lower plains) over the Civil Law side (China, Tibetan Plateau) (Storeygard, 2016). The confounder reverses the sign of the Low Kinship coefficient, from -0.567 in the restricted No-China

Table C.1: OLS Estimates of Kinship Intensity on Economic Activity
(200 km Bandwidth)

<i>Dependent Variable: Log Nighttime Luminosity</i>			
	(1) Baseline	(2) Controls	(3) Hierarchy
Panel A: Aggregate Relationship			
Kinship Intensity	0.545 (0.564)	0.116 (0.224)	0.202 (0.222)
Panel B: Split by Legal Origin			
Kinship Intensity	-0.489*** (0.128)	-0.373*** (0.101)	-0.275** (0.125)
Common Law $\times$ Kinship	1.640*** (0.539)	0.914*** (0.228)	0.907*** (0.256)
Jurisdictional Hierarchy			0.176* (0.091)
<i>Marginal Effects of Kinship:</i>			
Slope: Civil Law	-0.489***	-0.373***	-0.275**
Slope: Common Law	1.151**	0.541**	0.632**
Observations	78,364	77,681	75,134
Adj. R-Squared	0.423	0.624	0.627
Country FEs	✓	✓	✓
Geographic Controls		✓	✓
Population Controls		✓	✓

Notes. The table reports OLS estimates restricted to ethnic homelands located within a 200 km bandwidth of a legal origin boundary. The specification mirrors Table 2. Panel A reports the aggregate relationship between Kinship Intensity and Log Nighttime Luminosity. Panel B reports the interaction estimates. Controls include land suitability, malaria, distance to coast/rivers, temperature, rainfall, ruggedness, elevation, absolute latitude, and population density. Standard errors are two-way clustered at the ethnolinguistic family and country levels and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Spatial Regression Discontinuity Estimates by Kinship Intensity
(Full Sample)

	<i>Dependent Variable: Log Nighttime Luminosity</i>			
	(1)	(2)	(3)	(4)
Specification:	CCT	2.0× CCT	Fixed BW	Quadratic
Bandwidth:	Optimal	Wide	200 km	Optimal
Panel A: High Kinship				
RD Estimate	1.803** (0.783)	1.891** (0.748)	1.838** (0.769)	1.850** (0.863)
Control Mean	-9.74	-9.78	-10.08	-9.76
Observations	9,965	18,590	35,327	13,270
Clusters	50	52	52	51
Bandwidth (km)	44.4	88.9	200.0	60.5
Panel B: Low Kinship				
RD Estimate	0.282 (0.334)	0.521 (0.345)	-0.024 (0.371)	0.203 (0.546)
Control Mean	-8.43	-8.66	-8.54	-8.54
Observations	11,297	20,184	39,807	14,354
Clusters	49	50	50	49
Bandwidth (km)	38.9	77.8	200.0	51.3
Polynomial Order	1	1	1	2
Kernel	Tri	Tri	Uni	Tri
Country Pair FEs	✓	✓	✓	✓
Full Controls	✓	✓	✓	✓

Notes. The table reports spatial RD estimates of the effect of Common Law relative to Civil Law. The unit of observation is a $0.125^\circ \times 0.125^\circ$ grid cell. Panel A restricts the sample to High Kinship groups; Panel B restricts to Low Kinship groups. Column 1 uses the MSE-optimal bandwidth (Calonico et al., 2014). Column 2 doubles this bandwidth. Column 3 uses a fixed 200 km bandwidth with a uniform kernel to grant equal weight to all observations in the broader border region. Column 4 fits a local quadratic polynomial. Coefficients are bias-corrected RD estimates; robust standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Spatial Regression Discontinuity Estimates by Kinship Intensity
(Full Sample)

Specification: Bandwidth:	<i>Dependent Variable: Probability of Light</i>			
	(1)	(2)	(3)	(4)
	CCT Optimal	2.0× CCT Wide	Fixed BW 200 km	Quadratic Optimal
Panel A: High Kinship				
RD Estimate	0.182** (0.074)	0.185** (0.073)	0.162** (0.064)	0.190** (0.083)
Control Mean	0.45	0.43	0.41	0.44
Observations	8,499	15,756	35,330	12,369
Clusters	50	52	52	51
Bandwidth (km)	36.7	73.3	200.0	55.8
Panel B: Low Kinship				
RD Estimate	-0.004 (0.039)	0.022 (0.038)	0.006 (0.029)	-0.006 (0.052)
Control Mean	0.59	0.57	0.57	0.58
Observations	10,117	18,323	39,807	14,142
Clusters	49	50	50	49
Bandwidth (km)	34.4	68.9	200.0	50.4
Polynomial Order	1	1	1	2
Kernel	Tri	Tri	Uni	Tri
Country Pair FEs	✓	✓	✓	✓
Full Controls	✓	✓	✓	✓

Notes. The table reports spatial RD estimates of the effect of Common Law relative to Civil Law on the extensive margin of economic activity. The unit of observation is a $0.125^\circ \times 0.125^\circ$ grid cell. Panel A restricts the sample to High Kinship groups; Panel B restricts to Low Kinship groups. Column 1 uses the MSE-optimal bandwidth (Calonico et al., 2014). Column 2 doubles this bandwidth. Column 3 uses a fixed 200 km bandwidth with a uniform kernel to grant equal weight to all observations in the broader border region. Column 4 fits a local quadratic polynomial. Coefficients are bias-corrected RD estimates; robust standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

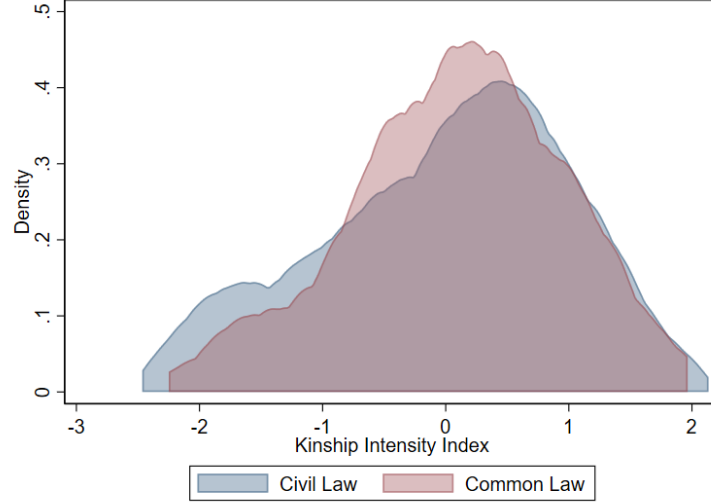


Figure C.1: Distribution of Kinship Intensity by Legal Origin

Notes. The figure presents kernel density estimates of Kinship Intensity for Civil Law (navy) and Common Law (maroon) jurisdictions. The substantial overlap between the two distributions ensures that the estimated effects are not driven by cultural differences between the two legal regimes.

sample (Table 4) to +0.282 in the full sample. The Low Kinship estimates in the full sample should therefore be interpreted with caution.

Robustness of the Mismatch. The mismatch penalty for High Kinship groups is unaffected by this issue. As shown in Panel A of Table C.2, the coefficient remains positive, large (1.803, $p < 0.05$), and statistically significant, confirming that the Common Law premium for high-kinship societies is not a geographic artefact of the Himalayas.

C.3 Placebo Boundary RD Estimates

I re-estimate the spatial RDD at placebo cutoffs located 50 km into the interior of both jurisdictions. As reported in Table C.4, these placebo borders yield estimates that are statistically indistinguishable from zero. The absence of discontinuities at these arbitrary locations confirms that the mismatch penalty is unique to the actual legal boundary.

Table C.4: Robustness: Placebo Border Tests by Kinship Intensity

	<i>Dependent Variable: Log Nighttime Luminosity</i>		
	(1)	(2)	(3)
Test Location:	Real Border	Placebo Common	Placebo Civil
Shift Distance:	0 km	+50 km	-50 km
Panel A: High Kinship			
RD Estimate	1.908** (0.822)	-0.420 (0.530)	-0.620 (0.613)
Control Mean	-9.60	-7.44	-9.54
Observations	8,950	3,391	3,003
Clusters	48	46	43
Bandwidth (km)	43.8	17.9	17.0
Panel B: Low Kinship			
RD Estimate	-0.567 (0.392)	0.292 (0.338)	0.537 (0.599)
Control Mean	-8.92	-7.29	-10.17
Observations	8,246	5,671	4,239
Clusters	48	45	37
Bandwidth (km)	31.5	23.5	22.4
Sample Restriction	–	> 0 km	< 0 km
Optimal Bandwidth	✓	✓	✓
Full Controls	✓	✓	✓
Country Pair FEs	✓	✓	✓

Notes. The table reports spatial RD estimates for real and placebo borders, split by Kinship Intensity. Column 1 reports the baseline estimate at the real legal origin boundary (0 km). Column 2 shifts the cutoff 50 km into the Common Law side (restricting sample to Distance > 0). Column 3 shifts the cutoff 50 km into the Civil Law side (restricting sample to Distance < 0). Panel A shows that the significant negative effect of Civil Law is unique to the real border in High Kinship areas; the Column 1 baseline corresponds to Column 1 of Table 4. Panel B confirms that in Low Kinship areas, there are no significant discontinuities at either the real or placebo borders. Coefficients are bias-corrected RD estimates; robust standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.4 Donut RD Estimates

To address concerns about cross-border spillovers (migration, shared infrastructure, trade linkages), I implement a donut RD that excludes all grid cells within 5, 10, and 25 km of the legal boundary and re-estimates the baseline specification.

Columns (1) through (4) in Table C.5 report the results for the High Kinship group. The intensive margin estimates remain positive, large, and statistically significant at the 5% level across all exclusion radii (ranging from 1.426 to 1.915), demonstrating that the mismatch penalty is not driven by border-zone contamination. The extensive margin estimates are robust through the 10 km exclusion but lose statistical significance at 25 km, consistent with a mechanical reduction in precision at this aggressive exclusion radius. The point estimates remain positive throughout, suggesting that the attenuation reflects reduced power rather than a vanishing effect.

For the Low Kinship group (Columns 5–8), the estimates are small, mostly insignificant, and unstable in sign across all donut radii, confirming the null.

C.5 Permutation Test

A deeper identification concern is that the estimated discontinuity may reflect persistent geographic clustering: Common Law countries happening to occupy more economically favourable locations for reasons unrelated to legal institutions. I conduct a permutation test that randomly reassigns legal origin labels at the country level 1,000 times, preserving the within-country spatial correlation structure. For each permutation, I recompute which borders constitute cross-legal-origin boundaries, reconstruct the paired sample, and re-estimate the baseline RD specification. The permutation p -value is the share of permuted estimates whose absolute value exceeds the actual estimate.

Figure C.2 reports the results for the High Kinship sample. For both the extensive margin (Panel A, actual estimate = 0.185) and intensive margin (Panel B, actual estimate = 1.794;

Table C.5: Donut RD Estimates

	High-Kinship				Low-Kinship			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Donut Radius	0 km	5 km	10 km	25 km	0 km	5 km	10 km	25 km
Panel A: Probability of Light								
RD Estimate	0.185** (0.075)	0.179** (0.079)	0.172* (0.099)	0.078 (0.078)	-0.081 (0.050)	-0.088 (0.057)	-0.105 (0.079)	0.100* (0.056)
Bandwidth (km)	36.5	36.8	29.6	54.2	30.6	29.0	30.6	71.8
Eff. Observations	7,702	7,448	4,049	5,489	8,042	7,317	5,432	10,183
Panel B: Log Nightlight Intensity								
RD Estimate	1.794** (0.781)	1.641** (0.802)	1.915** (0.838)	1.426** (0.726)	-0.657 (0.414)	-0.964** (0.462)	0.360 (0.594)	0.661 (0.617)
Bandwidth (km)	43.3	43.6	33.2	58.1	33.2	30.1	36.2	75.3
Eff. Observations	8,884	8,608	4,750	6,120	8,696	7,603	6,875	10,876
Polynomial Order	1	1	1	1	1	1	1	1
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri
Country Pair FEs	✓	✓	✓	✓	✓	✓	✓	✓
Full Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes. The table reports spatial donut RD estimates of the effect of Common Law relative to Civil Law for High- and Low-Kinship groups, dropping China from the global sample to achieve balance among Low Kinship groups. Treatment = assignment to Common Law jurisdiction; Control = Civil Law jurisdiction. The unit of observation is a $0.125^\circ \times 0.125^\circ$ grid cell. All specifications use the MSE-optimal bandwidth (Calonico et al., 2014). Coefficients are bias-corrected RD estimates. In addition to the baseline covariates, all specifications include continent indicators. Standard errors are clustered at the country-pair level. * p<0.10, ** p<0.05, *** p<0.01.

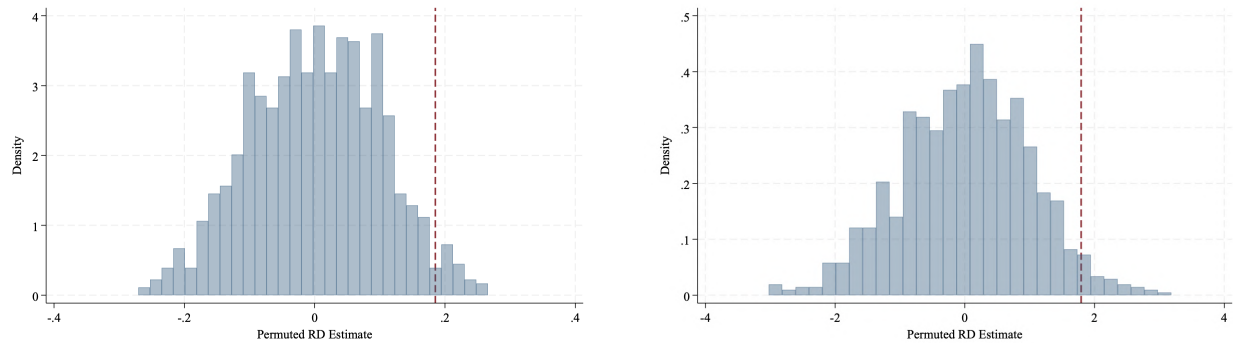


Figure C.2: High Kinship Sample Country-Level Permutation Test

Notes. The figure displays the distribution of bias-corrected RD estimates from 1,000 country-level permutations of legal origin. Panel A reports results for the Probability of Light (extensive margin); Panel B reports results for Log Nightlight Intensity (intensive margin). The dashed red line marks the actual estimate.

both estimated under the donut-baseline specification of Table C.5), the true estimate falls in the right tail of the permutation distribution, with p -values of 0.031 and 0.035, respectively. The test is inherently conservative: because legal origin is permuted across a finite set of countries, many random draws partially reproduce the true assignment, injecting real signal into the null distribution and widening it. For the Low Kinship sample, the actual RD estimates fall well within the centre of the permutation distribution ($p > 0.5$).

D Afrobarometer Outcomes

Figure D.1 decomposes the aggregate Political Participation Index reported in the main text (Figure 8) into its individual components. Panel A reports estimates for collective civic actions; Panel B reports estimates for direct contact with officials and traditional leaders.

Table D.1 reports the full RD estimates for corruption-related outcomes, extending the governance results summarised in the main text. The estimates decompose the trust gap into specific dimensions of perceived government performance, including bribery, corruption of traditional leaders, and the perceived quality of local service delivery.

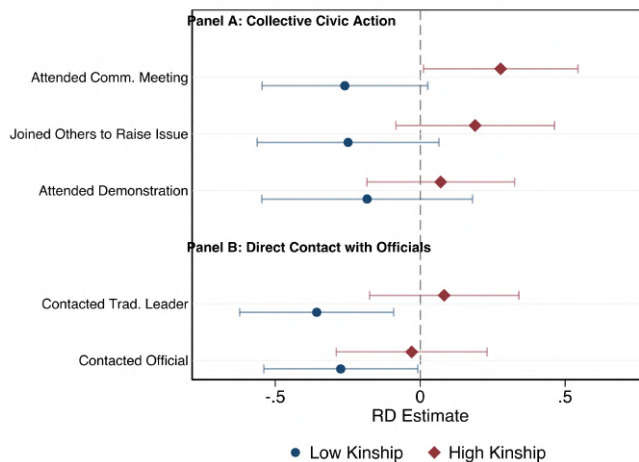


Figure D.1: Decomposition of the Political Participation Gap

Notes. The figure provides a granular decomposition of the Political Participation Index presented in Figure 8. Panel A reports estimates for collective civic actions (attending meetings, joining others to raise an issue, attending demonstrations). Panel B reports estimates for direct individual contact with officials and traditional leaders. Coefficients are sorted by the magnitude of the High Kinship effect. 95% confidence intervals are displayed.

Table D.1: RD Estimates for Mechanisms of Institutional Mismatch

Dependent Variable	(1) Low Kinship	(2) High Kinship
Panel A: Corruption		
Corruption: Civil Servants	0.010	−0.309***
Corruption: Parliament	0.017	−0.378**
Panel B: Bureaucratic Predation		
Paid Bribe: Identity Doc.	−0.068	−0.446***
Capacity to Report Corrup.	−0.042	0.131***
Country Pair FEs	✓	✓
Full Controls	✓	✓

Notes. The table reports RD estimates of the effect of legal origin (Common vs. Civil Law). The unit of observation is an individual, geocoded at the village or town level. The sample is split by the median Kinship Intensity Index to contrast the effect in low-kinship societies (Column 1) vs. high-kinship societies (Column 2). Panel A reports bureaucratic friction; Panel B reports experiences of corruption. Positive coefficients indicate better outcomes under Common Law, except for Corruption and Bribery variables, where negative coefficients indicate better outcomes (lower corruption). Standard errors are clustered at the country-pair level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Cameroon

E.1 Stacked Difference-in-Differences

The main Cameroon event-study uses the doubly robust estimator of [Callaway and Sant’Anna \(2021\)](#). For the Anglophone Crisis analysis, this specification drops the two Northern regions affected by Boko Haram and estimates dynamic effects using not-yet-treated and never-treated cells from the remaining Cameroon panel. As a complementary check, I re-estimate the core result using a stacked difference-in-differences design. This approach constructs a separate event window around each local crisis-onset cohort, stacks those cohort-specific panels, and includes stack-by-cell and stack-by-year fixed effects. The design therefore asks whether newly exposed cells lose nighttime light relative to cells that are untreated within the same cohort-specific window.

Table [E.1](#) reports four variants. Column 1 is the preferred stacked specification: the

non-North sample with a $[-3, +3]$ event window. This window preserves six eligible cohorts and around 197 focal treated cells, balancing dynamic comparability against the short 2013–2024 panel. Because the stacked design requires complete event-window support, these estimates should be read as support-restricted robustness checks when compared to the exact full-sample estimand reported by the CS-DiD specification. Column 2 restricts the comparison to the two Anglophone regions, holding the Common Law colonial legacy fixed but using fewer controls. Column 3 uses a local five-region comparison set around the Anglophone area. Column 4 repeats the preferred non-North specification with a wider $[-5, +5]$ window; this is a stricter but lower-powered check because only two cohorts and 102 focal treated cells have full support.

Across specifications, the high-kinship treatment effect remains large and negative, while the low-kinship effect is smaller and less precise. The pre-treatment averages are close to zero, indicating that the stacked design does not reveal systematic pre-onset divergence within the support-restricted samples. The pooled interaction estimates similarly indicate that the post-onset decline is concentrated in high-kinship cells. These results support the interpretation of the main CS-DiD estimates as reflecting a kinship-mediated response to the institutional mismatch shock rather than an artefact of the particular staggered-adoption estimator.

E.2 Ghost Towns as an Urban Phenomenon

The main text documents the aggregate economic collapse using the extensive margin and the urban-rural decomposition (Table 11). This section provides the complementary intensive margin results and formalises the triple-difference specification.

The Ghost Town mechanism (a general strike aimed at paralysing the commercial economy) is structurally an urban phenomenon. Table E.2 replicates the analysis using the intensive margin and restricts the sample to urban centres (above-median population density). The estimated coefficient in the urban sample is more than twice as large as the

Table E.1: Stacked Difference-in-Differences Robustness for the Anglophone Crisis

	<i>Dependent Variable: Probability of Light</i>			
	(1)	(2)	(3)	(4)
	Primary	Anglophone Only	Local	Long Window
Sample	Non-North	NW/SW	Five Regions	Non-North
Event Window	$[-3, +3]$	$[-3, +3]$	$[-3, +3]$	$[-5, +5]$
Panel A: Split Stacked DiD Estimates				
Low Kinship: Post $\times$ Treated	-0.028* (0.017)	-0.024 (0.018)	-0.028 (0.017)	-0.021 (0.012)
High Kinship: Post $\times$ Treated	-0.124*** (0.030)	-0.125*** (0.030)	-0.124*** (0.030)	-0.114*** (0.030)
Low Kinship: Pre Avg.	0.011 (0.014)	0.009 (0.013)	0.013 (0.014)	0.006 (0.020)
High Kinship: Pre Avg.	0.028 (0.030)	0.029 (0.032)	0.028 (0.031)	-0.000 (0.077)
Panel B: Pooled Heterogeneity Estimates				
Post $\times$ Treated $\times$ High Kinship	-0.096*** (0.035)	-0.100** (0.036)	-0.095** (0.036)	-0.093*** (0.028)
Post $\times$ Treated $\times$ KII	-0.146*** (0.026)	-0.158*** (0.026)	-0.141*** (0.028)	-0.114*** (0.029)
Observations	557,060	51,912	193,382	289,146
Clusters	54	13	35	54
Cohorts	6	6	6	2
Focal Treated Cells	197	195	197	102
Grid Cells in Stack	13,493	1,464	4,834	13,229
Stack $\times$ Grid Cell FEs	✓	✓	✓	✓
Stack $\times$ Year FEs	✓	✓	✓	✓

Notes. The table reports stacked difference-in-differences estimates for the Anglophone Crisis. The unit of observation is a $0.05^\circ \times 0.05^\circ$ grid cell-year. Treatment cohorts are defined by the first year in which a grid cell records an Anglophone Crisis event. Each column stacks cohort-specific event windows and compares newly treated cells to never-treated cells and cells first treated after the focal event window. Cells enter only when the full event window is observed, so the stacked estimates are support-restricted compared to the full-sample CS-DiD estimates. Panel B reports pooled heterogeneity specifications; interaction coefficients are differential treatment effects relative to the omitted low-kinship group, or marginal effects per unit of KII. Pre averages report the mean of available pre-treatment event-time coefficients, excluding $e = -1$. Column 1 uses the non-North sample, matching the main Anglophone CS-DiD comparison set that excludes Nord and Extreme-Nord (Boko Haram conflict regions). Column 2 restricts the comparison to the two Anglophone regions (Nord-Ouest and Sud-Ouest). Column 3 uses the five-region local comparison set (Nord-Ouest, Sud-Ouest, Ouest, Littoral, and Centre). Column 4 repeats the primary non-North specification with a wider $[-5, +5]$ event window. Standard errors are clustered at the department level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.2: Heterogeneous Impact of Anglophone Crisis by Population Density

<i>Dependent Variable: Log Nighttime Luminosity</i>		
	(1) Full Sample	(2) Urban Only
Triple-Differences Estimates		
Anglophone $\times$ Post $\times$ Kinship (<i>Mismatch Penalty</i>)	−0.003 (0.007)	−0.007 (0.010)
Anglophone $\times$ Post (<i>Crisis Main Effect</i>)	0.002 (0.009)	−0.001 (0.011)
Post $\times$ Kinship (<i>Counterfactual Trend</i>)	−0.005* (0.003)	−0.004 (0.004)
Observations	203,463	101,621
Clusters	58	58
Grid Cell FEs	✓	✓
Year FEs	✓	✓

Notes. The table reports estimates from triple-difference specifications using the intensive margin of economic activity (log nighttime luminosity). Column 1 reports the pooled sample estimates. Column 2 restricts the sample to Urban grid cells (above median population density). *Anglophone $\times$ Post $\times$ Kinship* captures the additional decline in economic activity for high-kinship groups in the crisis zone after 2016. Standard errors are clustered at the department level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

estimate for the full sample, although it is imprecisely estimated. The pattern agrees with the event-study estimates in Table 11, which concentrate the collapse in urban grid cells, and suggests that the null result in the aggregate intensive margin is driven by rural areas where the strike mechanism has limited bite.

To formalise this, I estimate the following Triple-Difference (DDD) specification:

$$\begin{aligned}
y_{it} = & \alpha + \beta_1(\text{Anglophone}_i \times \text{Post}_t) \\
& + \beta_2(\text{Post}_t \times \text{Kinship}_i) \\
& + \beta_3(\text{Anglophone}_i \times \text{Post}_t \times \text{Kinship}_i) \\
& + \mathbf{X}'_{it}\Gamma + \delta_i + \gamma_t + \varepsilon_{it}
\end{aligned} \tag{E.1}$$

Anglophone_i is an indicator for cells located in the Common Law (Southwest/Northwest)

regions. Post_t is an indicator for years following the 2016 judicial imposition (2017–2024). Kinship_i is the continuous standardised index of kinship intensity. The specification includes grid cell fixed effects (δ_i) to absorb all time-invariant geographic and cultural characteristics, and year fixed effects (γ_t) to absorb national economic shocks. The coefficient of interest is β_3 . A negative β_3 would indicate that the economic collapse was deeper in high-kinship areas, consistent with the hypothesis that tight social networks facilitated the enforcement of Ghost Town strikes.

E.3 The Boko Haram Conflict

Figure E.1 presents the dynamic event study estimates for the Boko Haram insurgency, serving as a placebo test. Table E.3 reports the full coefficient estimates. As in the aggregate results in the main text (Table 10), the onset of the Boko Haram conflict does not trigger systematic economic collapse for either kinship group. The slightly positive coefficients are consistent with a solidarity effect in which kinship networks provide resilience during conventional conflict.

E.4 Solidarity or Coercion

The aggregate collapse documented in Table 10 could theoretically arise from two sources: (i) strategic Ghost Town strikes enforced by local networks, or (ii) collateral damage from military clashes being concentrated in high-kinship areas. To distinguish between these channels, Table E.4 decomposes the effect by type of violence. I restrict the sample to the Anglophone conflict zone and estimate a triple-difference specification interacting the post-period dummy with kinship intensity and specific conflict indicators from ACLED.

Column (1) focuses on *Violence Against Civilians*, which proxies for the enforcement of strikes (e.g., militias targeting shopkeepers who refuse to close). The triple interaction is large, negative, and significant at the 1% level ($\beta = -0.599^{***}$). This implies that when coercive force is applied in high-kinship networks, economic activity falls substantially.

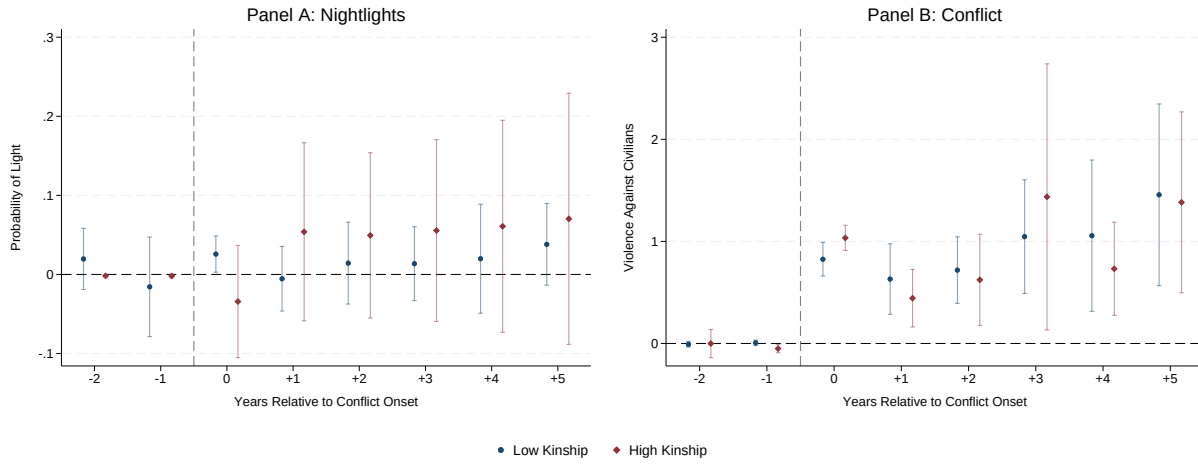


Figure E.1: Impact of Conflict Onset on the Probability of Light by Kinship Intensity

Notes. The figure reports dynamic Average Treatment Effects on the Treated (ATT), estimated using the doubly robust difference-in-differences estimator of [Callaway and Sant'Anna \(2021\)](#). The sample is stratified by the median Kinship Intensity Index (KII). Panel A reports estimates for the Probability of Nighttime Light (Extensive Margin). Panel B reports estimates for the number of events classified as Violence Against Civilians in the ACLED database. The x-axis represents years relative to the first recorded Boko Haram conflict event ($e = 0$) in the grid cell. Blue circles represent estimates for the Low Kinship subsample; red diamonds represent estimates for the High Kinship subsample. Bars indicate 95% confidence intervals clustered at the department level.

Table E.3: Event Study Estimates of Institutional Mismatch vs. State Failure

<i>Dependent Variable: Probability of Light</i>		
	(1)	(2)
Time Relative to Onset	Low Kinship	High Kinship
Panel A: Anglophone Crisis		
$e = -5$	0.002	-0.010
$e = -4$	-0.002	-0.002
$e = -3$	0.006	0.029***
$e = -2$	-0.019**	-0.022
$e = -1$	0.000	-0.015
$e = 0$ (Onset)	-0.013	-0.038**
$e = 1$	-0.020***	-0.062***
$e = 2$	-0.010	-0.055
$e = 3$	-0.016	-0.113***
$e = 4$	-0.033	-0.065
$e = 5$	-0.071***	-0.149***
Panel B: Boko Haram Conflict		
$e = -2$	0.020	-0.002**
$e = -1$	-0.016	-0.002
$e = 0$ (Onset)	0.026**	-0.034
$e = 1$	-0.005	0.054
$e = 2$	0.014	0.049
$e = 3$	0.014	0.056
$e = 4$	0.020	0.061
$e = 5$	0.038	0.070
DR-IPW Estimator	✓	✓

Notes. The table reports the dynamic event study coefficients estimated using the Doubly Robust Inverse Probability Weighting (DR-IPW) method ([Callaway and Sant'Anna, 2021](#)). The dependent variable is the probability of nighttime light, observed over the 2013–2024 panel. Panel A reports the effect of the Anglophone Crisis (Mismatch) with a window of $[-5, +5]$. Panel B reports the effect of the Boko Haram conflict (State Failure) with a window of $[-2, +5]$, reflecting the earlier onset of the insurgency. Standard errors are clustered at the department level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Because violence against civilians is virtually absent in the pre-crisis period, this coefficient is identified primarily by the post-period contrast between violent and peaceful high-kinship cells; it should be read as descriptive of where coercive enforcement was concentrated rather than as a dynamic treatment effect.

In contrast, Column 2 uses *Battles and Explosions* to proxy for kinetic warfare. Here, the triple interaction is a precise zero ($\beta = -0.001$). This null result is inconsistent with the alternative explanation that high-kinship areas simply suffered more physical destruction from military combat, and favours a strategic withdrawal of commerce enforced through the social network.

Placebo: Boko Haram. Is this resistance multiplier a universal feature of African conflict? It is possible that high-kinship groups simply retreat more readily during any period of instability. To test this, Columns (3) and (4) of Table E.4 repeat the specific mechanism analysis for the Boko Haram insurgency in Northern Cameroon. This conflict is driven by religious extremism within a Civil Law framework. As shown in these columns, the triple interaction terms are positive and statistically insignificant. Whether looking at violence against civilians or battles, high kinship does not amplify the economic cost of violence in the North. If anything, the positive coefficients again suggest a solidarity effect in which kin networks provide resilience during conventional conflict.

Table E.4: Heterogeneous Impact of Conflict by Kinship Intensity and Conflict Type

<i>Dependent Variable: Probability of Light</i>				
	Anglophone Crisis		Boko Haram	
	(1)	(2)	(3)	(4)
	Enforcement	War	Enforcement	War
	(Predation)	(Battles)	(Predation)	(Battles)
Triple-Differences Estimates				
Post $\times$ High Kinship	−0.008	−0.007	−0.012	−0.012
<i>(Solidarity/Peaceful)</i>	(0.009)	(0.008)	(0.014)	(0.014)
Post $\times$ Violence	−0.020**	−0.016	−0.092	−0.063
<i>(Direct Effect)</i>	(0.009)	(0.010)	(0.089)	(0.138)
Post $\times$ High Kinship $\times$ Violence	−0.599***	−0.001	0.102	0.075
<i>(Network Multiplier)</i>	(0.011)	(0.015)	(0.076)	(0.122)
Implied Mechanism				
	Enforcement	None	None	None
Observations	19,071	19,071	45,695	45,695
Clusters	13	13	10	10
Grid Cell FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓

Notes. The table reports estimates from triple-difference specifications disentangling the mechanism of economic collapse. Columns 1-2 restrict the sample to the Anglophone conflict zone; Columns 3-4 restrict to the Boko Haram conflict zone (Placebo). *Enforcement* defines violence as Violence Against Civilians; *War* defines violence as Battles and Explosions. *Post $\times$ High Kinship* captures the effect in peaceful cells (solidarity). *Post $\times$ Violence* captures the effect of violence in low-kinship cells. The triple interaction captures the amplification effect of kinship networks when violence is present. Standard errors are clustered at the department level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.