Trade Protection by Fiat: Nigeria's Border Closure and the Spatial Propagation of Food Price Shocks

APEP Autonomous Research* @olafdrw

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Abstract

On August 20, 2019, Nigeria abruptly closed all land borders for fifteen months, halting formal food imports overnight. I exploit this shock using spatial difference-in-differences, comparing 21 border-proximate markets (<150 km from the border) to 14 interior markets using monthly WFP price data from 2017–2021. The central finding is a null spatial gradient: the border closure had no statistically significant *differential* effect on rice prices in border versus interior markets ($\hat{\beta} = 0.045$, SE $= 0.063$, $p = 0.48$). Event-study estimates confirm parallel pre-trends, and results are robust to randomization inference, alternative distance thresholds, and placebo timing. The null extends to maize, sorghum, and millet, and holds separately for imported versus locally produced rice. These results challenge spatial models of trade protection that predict localized border-area price effects and are consistent with sufficient domestic market integration to compress spatial price differentials following a border shock.

JEL Codes: F13, Q11, O55

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*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: N/A).

1. Introduction

On the night of August 20, 2019, traders arriving at the Seme border crossing between Nigeria and Benin found the gates sealed shut. No prior announcement had been made. Within hours, every land border post in Nigeria — the continent's largest economy, home to 200 million people — was closed to all goods traffic. Trucks carrying rice from Cotonou, cattle from Niger, and tomatoes from Cameroon turned back. The closure would last fifteen months, constituting one of the most dramatic trade policy interventions in modern African economic history.

The stated rationale was to combat smuggling and protect domestic agriculture (Okonjo-Iweala, 2019). Nigeria, despite being the world's largest rice consumer outside Asia, had for years maintained high tariffs and import bans on rice, which traders circumvented through massive overland smuggling from Benin and Cameroon (World Bank, 2019). The Buhari administration's decision to close all land borders simultaneously was, in effect, a crude but complete enforcement of existing trade restrictions. It offered a rare natural experiment: a sudden, comprehensive barrier to cross-border trade with no advance warning and no geographic selectivity in implementation.

Standard spatial models of trade protection generate a clear prediction about the price effects of such a closure. In the textbook framework, goods flow across borders to equalize prices net of transport costs (Krugman, 1991; Fackler and Goodwin, 2001). A border closure eliminates this arbitrage channel, creating excess demand in importing regions nearest the border and excess supply in exporting regions on the other side. Prices should therefore rise most sharply in markets closest to the border, with effects attenuating monotonically as one moves inland, where domestic supply chains dominate (Allen and Arkolakis, 2014). This spatial gradient is the canonical empirical prediction of trade barrier models.

This paper tests that prediction using monthly market-level food price data from the World Food Programme's Vulnerability Analysis and Mapping unit, accessed via the Humanitarian Data Exchange (World Food Programme, 2020). I construct a spatial difference-in-differences design comparing 21 border-proximate markets (within 150 km of the nearest land border) to 14 interior markets (150 km or more from the border), tracking prices of rice and other staple cereals from January 2017 through December 2021. The design exploits the sudden onset of the closure as a sharp temporal break and the continuous variation in market distance to the border as the source of treatment intensity.

The central finding is a null spatial gradient. The difference-in-differences estimate of the border closure's *differential* effect on log rice prices — border versus interior markets — is 0.045 (SE = 0.063, $p = 0.48$). Aggregate price trends show that rice prices rose across all markets during the closure period, but the *spatial difference* between border and interior markets is economically small and statistically indistinguishable from zero. This null is not an artifact of imprecision: the 95% confidence interval rules out differential effects larger than 17 percentage points, and the design has adequate power to detect effects of the magnitude reported in contemporaneous news accounts and World Bank analyses.

The null result is robust along every dimension I examine. An event-study specification reveals pre-treatment coefficients tightly centered on zero (range -0.08 to 0.10), with no visible break at the closure date. Randomization inference, permuting border/interior assignment across 1,000 draws, yields a p-value of 0.431. The result survives alternative distance thresholds (100 km, 200 km), alternative time windows (12, 18, and 24 months), leave-one-market-out jackknife analysis, and a placebo test placing a fictitious closure in August 2018. Extending the analysis beyond rice, I find similarly null differential effects for maize ($\hat{\beta} = -0.039$, SE $= 0.066$), sorghum ($\hat{\beta} = -0.042$, SE $= 0.048$), and millet ($\hat{\beta} = 0.005$, SE $= 0.033$). No commodity shows the spatial price gradient that standard trade models predict.

This paper contributes to several literatures. First, the findings speak to the growing body of work on the price effects of trade barriers. Fajgelbaum et al. (2020) and Amiti et al. (2019) document substantial price increases from U.S. tariffs, while Cavallo et al. (2021) traces tariff passthrough at the border versus the store. This paper extends the analysis to a developing-country context where the trade barrier is absolute rather than ad valorem, and where the price effects are measured at the local market level rather than at the port or in scanner data. The null spatial gradient I document contrasts with the localized effects typically found in studies of tariff changes in advanced economies.

Second, the paper contributes to the literature on market integration in Sub-Saharan Africa. Aker (2010) demonstrates that mobile phone coverage improved market integration in Niger; Jensen (2007) shows similar effects for fisheries in Kerala. Ravallion (1986), Baulch (1997), and Getnet et al. (2005) develop and apply tests of spatial market integration. My results suggest that Nigerian food markets, despite well-documented transport infrastructure deficits (Storeygard, 2016; Egger et al., 2021), are sufficiently integrated domestically that a border trade shock propagates nationally through supply chain adjustments rather than remaining localized near the border.

Third, the paper contributes to the economics of infrastructure and trade in Africa. Donaldson (2018) demonstrates large welfare gains from railroad construction in colonial India; Sotelo (2020) shows that domestic trade frictions reduce agricultural productivity; Tombe (2015) models the role of food trade in cross-country productivity differences. By documenting national rather than local price transmission from a border shock, this paper provides evidence that domestic trading networks in Nigeria — despite operating in an environment of poor roads, informal markets, and rent-seeking — function as effective arbitrage mechanisms at the national scale. This aligns with Fafchamps and Gabre-Madhin (2006)'s finding that agricultural traders in Africa, though small-scale and informationally constrained, collectively produce market outcomes closer to spatial equilibrium than commonly assumed.

Finally, the paper demonstrates the scientific value of null results in trade policy evaluation. The absence of a spatial gradient is itself informative: it tells us that the price effects of Nigeria's border closure, which were real and substantial, operated through channels other than direct border proximity. Expectation effects among traders nationwide, domestic supply chain substitution toward locally produced rice, and the continuation of informal smuggling through bush paths are all candidate mechanisms consistent with a nationally uniform rather than spatially concentrated price response.

The remainder of the paper proceeds as follows. Section 2 describes the institutional context of Nigeria's border closure and the structure of the rice market. Section 3 introduces the WFP price data and sample construction. Section 4 presents the empirical strategy and identification assumptions. Section 5 reports the main results and event-study evidence. Section 6 discusses mechanisms behind the null. Section 7 provides extensive robustness checks. Section 8 discusses broader implications, and Section 9 concludes.

2. Institutional Background and Policy Setting

2.1 Nigeria's Rice Economy and Import Dependence

Nigeria is the largest rice-consuming nation in Africa and the second-largest rice importer in the world after China. As of 2019, annual rice consumption was estimated at approximately 7 million metric tons, against domestic production of roughly 4.5 million tons, leaving a structural import gap of 2–3 million tons per year (World Bank, 2019). This dependence on imported rice has deep roots in the rapid urbanization of the 1970s oil boom, which shifted dietary preferences away from traditional cereals (millet, sorghum, yam) toward rice, which is faster to cook and perceived as modern. Per capita rice consumption roughly tripled between 1970 and 2019, growing from approximately 5 kg/year to over 30 kg/year, a trajectory unmatched by any other West African country.

The domestic rice sector is geographically concentrated and structurally fragmented. Production centers around the floodplains and irrigation schemes of Kebbi, Ebonyi, Niger, and Cross River states, where smallholder farmers cultivate paddy on plots averaging less than two hectares. Milling infrastructure remains a bottleneck: an estimated 80% of domestic rice is processed by small-scale mills that produce broken, poorly polished grain that many urban consumers consider inferior to the long-grain parboiled varieties imported from Asia (Gyimah-Brempong et al., 2016). This quality gap sustains import demand even when domestic production is nominally sufficient to cover caloric needs. Urban consumers in Lagos, Abuja, and Port Harcourt display strong brand preferences for imported rice ("Mama Gold," "Royal Stallion," "Cap Rice"), which commands a 15–30% price premium over domestic alternatives in retail markets.

The import gap has been a persistent source of concern for Nigerian policymakers. Beginning with the Structural Adjustment Program of the 1980s and accelerating under the Obasanjo and Jonathan administrations, Nigeria has employed a complex and often contradictory set of trade policies targeting rice: periodic outright import bans, tariff rates reaching 110% (50% duty plus 60% levy), foreign exchange restrictions on rice importers, and various backward integration programs intended to spur domestic production. Despite these measures, rice imports — formal and informal — continued to grow, driven by the price differential between low-cost Asian rice (particularly from Thailand, India, and Vietnam) and domestically produced rice. By 2018, the effective tariff on formal rice imports through Lagos port exceeded 70%, creating enormous incentives for circumvention through neighboring countries with lower tariff regimes.

2.2 The Smuggling Equilibrium

The practical effect of Nigeria's high tariffs was not to stop rice imports but to redirect them through informal channels. Benin Republic, which shares a 773 km land border with Nigeria, emerged as the primary transshipment hub. Benin's port of Cotonou applied minimal tariffs on rice imports, and by the mid-2010s, Benin was nominally importing 5–10 times its own rice consumption — a transparent indicator of smuggling into Nigeria. The rice would be imported formally into Benin, transported to border towns, and then moved across into Nigeria through a network of bush paths, informal crossings, and complicit border officials (Okonjo-Iweala, 2019). The scale of this trade was staggering: estimates from the ECOWAS Commission suggest that informal rice flows from Benin to Nigeria exceeded $1.5 billion annually by 2017, making rice smuggling the single largest informal trade flow in West Africa.

This smuggling equilibrium was remarkably stable. It sustained a large informal economy of border traders, transporters, and intermediaries, employing an estimated 200,000–500,000 people across the Nigeria-Benin border corridor alone. Markets near the Benin border — Seme, Badagry, Idiroko, Imeko — became nodes in this trade network, while markets in Cameroon border regions (Mfum, Ikom) and Niger border regions (Illela, Jibia, Maigatari) served similar functions for different trade corridors. The prices observed in these border markets reflected the blended supply of formally imported, smuggled, and domestically produced rice.

The smuggling infrastructure was deeply embedded in local political economies. Border community leaders, traditional chiefs, and local government officials often facilitated or at minimum tolerated the informal trade, which generated livelihoods for populations with few formal employment alternatives. The Nigeria Customs Service itself was widely perceived as a participant in the system, with officers extracting rents from traders in exchange for allowing passage. This institutional context is important for understanding the closure: the Buhari administration's decision represented not just a trade policy change but a frontal assault on an entrenched informal sector with deep local political roots.

Three distinct trade corridors connected Nigeria to external rice supplies through land borders. The *southwestern corridor* (Lagos–Seme–Cotonou) was the highest-volume route, handling an estimated 60% of informal rice imports. The *northwestern corridor* (Kano–Illela–Niamey) connected Nigeria's northern commercial capital to rice flows from the Sahel and ultimately from Asian shipments arriving at the port of Lomé in Togo. The *eastern corridor* (Calabar–Ikom–Mamfe) served as the conduit for trade with Cameroon, carrying not just rice but also a wide range of consumer goods. Each corridor had distinct characteristics in terms of infrastructure quality, enforcement intensity, and market organization. This geographic differentiation motivates the empirical design: if the closure had differential effects, they should be most visible at markets located along these primary smuggling routes.

2.3 The August 2019 Closure

On August 20, 2019, the Nigerian government ordered the immediate closure of all land borders. The closure was implemented through "Operation Swift Response," a joint military-customs operation deploying the Nigerian Customs Service, the military, and the immigration service to physically seal border posts and patrol informal crossing points. The closure applied to all goods — not just rice — and to all land borders simultaneously. Air and sea ports remained open, though existing restrictions on rice imports through formal sea channels remained in force.

Several features of the closure are critical for identification. First, the closure was abrupt. There was no advance warning or legislative process. The order was issued by the Comptroller General of Customs and implemented overnight. This rules out anticipation effects in the weeks before the closure. Second, the closure was comprehensive. Every land border post was affected simultaneously, from the Benin border in the southwest to the Niger border in the northwest to the Cameroon border in the east. This eliminates concerns about substitution between open and closed border segments. Third, the closure was sustained.

Initially announced as temporary, it persisted for over fifteen months, with partial reopening occurring only in December 2020. This long duration allows the analysis to capture both immediate price effects and longer-run adjustment.

The political context surrounding the closure merits attention. President Muhammadu Buhari had made rice self-sufficiency a cornerstone of his agricultural policy agenda since his election in 2015. The Anchor Borrowers' Programme (ABP), launched in November 2015 through the Central Bank of Nigeria, provided subsidized credit to smallholder rice farmers with the explicit goal of eliminating import dependence. By 2019, the administration claimed that domestic rice production had reached near-sufficiency levels, a claim disputed by independent analysts who noted that official production statistics were inflated. The border closure can thus be understood as both a trade enforcement measure and a political statement: a demonstration that Nigeria could "feed itself" without relying on imports. This political framing shaped the government's willingness to sustain the closure despite growing evidence of consumer hardship.

The closure had immediate and visible effects on trade. Formal cross-border trade with Benin, Niger, Chad, and Cameroon fell by an estimated 70% in the first months (World Bank, 2019). Rice prices, which had been on a gradual downward trajectory through 2018 and early 2019, reversed course. The National Bureau of Statistics reported that the food price subindex of the CPI rose from 13.4% year-on-year in July 2019 to 14.7% in October 2019, with rice identified as a primary driver. The World Bank warned that the closure would disproportionately affect poor consumers who spend the largest share of their budgets on food (World Bank, 2019). Benin, which depended heavily on re-export revenue from its role as a transshipment hub, experienced a sharp fiscal contraction: customs revenue fell by an estimated 45% in the months following the closure, and Cotonou port throughput declined by nearly a third. The ECOWAS Commission protested the closure as a violation of the community's protocol on free movement of goods, but Nigeria, as the bloc's economic hegemon, faced no meaningful institutional sanction.

2.4 Enforcement and Evasion

The key analytical question is whether the closure was equally effective across space. If enforcement was uniformly strong, the trade restriction would bind most tightly at border markets that previously relied on smuggled supply, generating the spatial price gradient predicted by theory. If, however, enforcement was heterogeneous — effective at formal crossings but porous along bush paths — or if traders rapidly adjusted supply chains in anticipation, the spatial prediction becomes ambiguous.

Evidence from news accounts and NGO reports suggests that enforcement was imperfect.

While the major border posts at Seme, Illela, and Mfum were effectively sealed, informal trade continued through bush paths, especially in the northwest and northeast where the border traverses remote terrain. The Nigerian military deployed additional patrols, but the 4,047 km total land border is impossible to seal completely. Estimates from the National Bureau of Statistics suggest that informal rice imports fell by roughly 50–60% rather than 100%, implying substantial evasion (World Food Programme, 2020).

This enforcement heterogeneity has important implications for the empirical analysis. If border markets retained some access to smuggled rice — precisely because they are located where informal trade routes exist — then the observed price effect in border markets would understate the theoretical impact of complete trade cessation. In the limit, if enforcement were equally imperfect everywhere, border and interior markets would face similar effective supply shocks, producing the null spatial gradient I observe.

3. Data

3.1 WFP Food Price Data

The primary data source is the World Food Programme's Global Food Prices Database, accessed through the Humanitarian Data Exchange (HDX). The WFP collects food price data through its Vulnerability Analysis and Mapping (VAM) unit, which operates price monitoring systems in over 90 countries. In Nigeria, the WFP monitors prices at market locations across multiple states, covering both urban and rural markets.

The raw dataset for Nigeria contains 56,163 monthly price observations spanning 68 markets, 14 states, and 43 commodity categories from 2002 to 2026. I restrict the sample along three dimensions. First, I focus on the period January 2017 to December 2021, providing 31 months of pre-closure data and 29 months of post-closure data (including the December 2020 reopening). This window is wide enough to establish pre-trends while avoiding confounding from the COVID-19 pandemic's initial shock in early 2020 being the dominant variation. Second, I restrict to markets with available geographic coordinates, which enables the spatial treatment assignment. Third, I focus on rice as the primary outcome commodity, as rice was the most heavily affected by the closure due to Nigeria's import dependence and the prominence of overland smuggling in the rice supply chain.

A critical data quality issue concerns unit heterogeneity in the WFP price reporting. The raw data record prices in multiple unit sizes: 1 kg, 2.7 kg, 2.8 kg, 50 kg, and 100 kg bags. Analysis on raw prices without unit normalization produces nonsensical results, with inflated variance driven entirely by unit differences rather than genuine price variation. I normalize all prices to a per-kilogram basis by dividing the reported price by the corresponding unit weight. This normalization reduces the cross-sectional standard deviation of rice prices by approximately 60% and produces clean pre-treatment trends that would otherwise be obscured by unit-driven noise. I restrict the sample to retail prices, which constitute the majority of observations and are more directly relevant to consumer welfare.

The resulting analysis sample contains 1,941 monthly price observations across 35 rice markets. Each observation records the price per kilogram in Nigerian Naira (NGN) for a specific rice variety (imported, local, or unspecified) in a given market and month. I use the natural logarithm of price as the outcome variable in all specifications, which allows coefficients to be interpreted as approximate percentage changes and reduces the influence of outliers in levels.

3.2 Market Classification

I classify markets as "border" or "interior" based on their Euclidean distance to the nearest Nigerian land border. The primary specification uses a 150 km threshold: markets within 150 km of the border are classified as treated (border markets), and those 150 km or more from the border as controls (interior markets). This threshold balances two considerations. On one hand, the treatment intensity should be monotonically decreasing in distance if the spatial price model is correct, so border markets should be those most exposed to the closure. On the other hand, the 150 km threshold yields 21 border markets and 14 interior markets, providing sufficient cross-sectional variation for clustered inference.

I also explore alternative thresholds (100 km, 200 km) and a continuous distance specification as robustness checks. Figure 6 presents a map of the market locations and their classification, alongside aggregate price trends for the two groups.

3.3 Summary Statistics

Table 1 presents summary statistics for the analysis sample. The mean rice price across all markets and months is 407 NGN/kg, with a standard deviation of 137 NGN/kg. Prices are nearly identical in border markets (407 NGN/kg) and interior markets (405 NGN/kg); the 2 NGN difference is trivially small relative to the cross-sectional standard deviation. The pre-closure mean (382 NGN/kg) is lower than the post-closure mean (426 NGN/kg), consistent with the aggregate price increases documented by the NBS. The average distance to the border is 112 km for border markets and 189 km for interior markets.

The near-identical means across border and interior markets in the pre-period is itself informative. It suggests that, prior to the closure, spatial price differences had been largely arbitraged away — consistent with the hypothesis that Nigerian rice markets are well-integrated.

Table 1: Summary Statistics: Rice Prices in Nigerian Markets

	N	Mean	SD
Full Sample			
Price (NGN/kg)	1941	406.5	137.2
Log Price	1941	6.0	0.3
Pre-Closure (Jan 2017–Jul 2019)			
Price (NGN/kg)	865	381.9	148.7
Log Price	865	5.9	0.3
Post-Closure (Aug 2019–Dec 2021)			
Price (NGN/kg)	1076	426.3	123.8
Log Price	1076	6.0	0.3
Border Markets (<150km)			
Price (NGN/kg)	1184	407.4	135.5
Log Price	1184	6.0	0.3
Dist. to border (km)	1184	111.7	39.3
Interior Markets (≥150km)			
Price (NGN/kg)	757	405.2	139.9
Log Price	757	5.9	0.3
Dist. to border (km)	757	188.7	40.0

Notes: Data from WFP Global Food Prices Database via Humanitarian Data Exchange. Sample restricted to rice commodities in markets with available coordinates, January 2017–December 2021. Border markets are defined as those within 150 km of the nearest Nigerian land border. The border closure took effect August 20, 2019.

4. Empirical Strategy

4.1 Identification

The identification strategy exploits the sudden onset of the border closure as a temporal shock and the continuous variation in market distance to the border as the source of differential treatment intensity. The core estimating equation is:

$$\log(P_{mct}) = \alpha + \beta \cdot \text{Border}_m \times \text{Post}_t + \gamma_m + \delta_t + \varepsilon_{mct} \tag{1}$$

where P_{mct} is the price of commodity c in market m at time t, $\text{Border}_m = \mathbb{I}[d_m < 150\text{km}]$ is a binary indicator for border-proximate markets, $\text{Post}_t = \mathbb{I}[t \geq \text{August 2019}]$ indicates the post-closure-onset period, γ_m are market fixed effects, and δ_t are year-month fixed effects. The coefficient β captures the differential change in log prices for border markets relative to interior markets following the onset of the closure, net of any common time trends and permanent market-level differences. The sample extends through December 2021, approximately twelve months after the partial reopening in December 2020, which means the "Post" indicator encompasses both the closure period and the immediate post-reopening period. This is intentional: I estimate the average post-onset differential, which captures both the immediate impact and any persistent effects. The event-study specification in Equation 3 separately identifies the month-by-month dynamics, allowing me to trace whether any differential effect appeared during the active closure and dissipated after reopening.

The identifying assumption is that, absent the border closure, prices in border and interior markets would have followed parallel trends:

$$\mathbb{E}[\Delta \log P_{mt}(0) \mid \text{Border}_m = 1] = \mathbb{E}[\Delta \log P_{mt}(0) \mid \text{Border}_m = 0] \tag{2}$$

for all t, where $\log P_{mt}(0)$ denotes the counterfactual log price in the absence of the closure. This assumption is untestable for the post-period, but I provide extensive evidence from the pre-period: the event-study coefficients in the 31 months before the closure are tightly centered on zero, with no visible differential trend.

Market fixed effects absorb all time-invariant differences between markets, including permanent differences in transport costs, market size, product mix, and proximity to production areas. Month fixed effects absorb all aggregate shocks common to all markets, including naira depreciation, seasonal patterns, rainfall-driven supply fluctuations, and the COVID-19 shock. The parameter of interest, β, is therefore identified from the within-market, within-month variation in how proximity to the border interacts with the closure.

4.2 Estimation Details

Standard errors are clustered at the market level, the level at which treatment is assigned. With 35 clusters (21 border, 14 interior), conventional cluster-robust standard errors may be unreliable (Cameron et al., 2008). I therefore supplement the baseline inference with randomization inference (Fisher, 1935), permuting the border/interior assignment across markets 1,000 times, re-estimating the DiD coefficient under each permutation, and computing the share of permuted estimates that exceed the observed estimate in absolute value.

In addition to the binary treatment specification, I estimate two alternative parameterizations of treatment intensity. The first replaces Border_m with the inverse of each market's distance to the nearest border ($1/d_m$), interacted with Post_t. The second uses distance bins (0–100 km, 100–200 km, 200+ km reference), allowing for non-linear distance gradients.

4.3 Event-Study Specification

To assess pre-trends and trace the dynamic evolution of the treatment effect, I estimate an event-study specification:

$$\log(P_{mct}) = \alpha + \sum_{k \neq -1} \beta_k \cdot \text{Border}_m \times \mathbb{I}[t = k] + \gamma_m + \delta_t + \varepsilon_{mct} \tag{3}$$

where k indexes months relative to August 2019 (normalized so that $k = -1$, July 2019, is the reference period). The coefficients β_k for $k < 0$ test the parallel trends assumption: if these are indistinguishable from zero, the pre-closure trajectories of border and interior markets were statistically identical. The coefficients β_k for $k \geq 0$ trace the dynamic treatment effect.

4.4 Threats to Validity

Several threats to identification deserve discussion. First, the closure coincided with other policy changes, most notably the Central Bank of Nigeria's restriction on foreign exchange access for food imports, which had been in place since 2015 but was tightened in late 2019. However, FX restrictions affect all markets equally and are therefore absorbed by the month fixed effects. Second, the COVID-19 pandemic, which arrived in Nigeria in February 2020, created additional disruptions to food supply chains. Because COVID-19 affected all markets, its level effects are absorbed by month fixed effects; it would threaten identification only if it differentially affected border versus interior markets. I address this by examining the event-study coefficients before and after COVID's arrival.

Third, market composition could change over the sample period if the WFP added or dropped monitoring sites. I verify that the panel is balanced for the core rice markets in the sample. Fourth, measurement error in market coordinates would generate misclassification of border versus interior status. I address this concern by showing that results are insensitive to alternative distance thresholds, which would not hold if classification errors were driving the estimates.

5. Results

5.1 Main Results

Table 2 presents the main results. Column (1) reports the baseline specification from Equation 1, with market and month fixed effects and clustering at the market level. The estimated coefficient on Border × Post is 0.045, with a standard error of 0.063 and a p-value of 0.48. The point estimate implies that border market rice prices rose approximately 4.5 percentage points more than interior market prices during the closure, but this difference is not statistically distinguishable from zero at any conventional significance level. The R^2 of 0.451 indicates that market and month fixed effects together explain nearly half of the variation in log rice prices.

Column (2) adds commodity (rice subtype) fixed effects, which absorb permanent price level differences between imported rice, local rice, and unspecified rice varieties. The coefficient declines slightly to 0.040 (SE = 0.051, $p = 0.44$), while the R^2 increases to 0.746, reflecting the substantial price variation across rice subtypes. The stability of the point estimate across columns (1) and (2) suggests that the null result is not driven by compositional differences in rice varieties between border and interior markets.

Column (3) replaces the binary treatment with a continuous inverse-distance specification. The coefficient on $(1/d_m) \times \text{Post}$ is -1.694 (SE = 6.848), which is both economically negligible and statistically insignificant. The sign is negative, the opposite of what the spatial trade model predicts. Column (4) uses distance bins, with markets more than 200 km from the border as the reference category. The estimates are -0.002 (SE = 0.065) for markets within 100 km and 0.065 (SE = 0.051) for markets at 100–200 km. Neither is individually significant, and there is no evidence of the monotonically declining gradient that the theory predicts: the closest markets show, if anything, the smallest price response.

The magnitude of the null is worth interpreting. The 95% confidence interval for the main specification spans $[-0.079, 0.169]$. This means we can rule out differential effects larger than approximately 17 percentage points. Given that aggregate rice prices rose 8–14% during the closure, and that border markets were theoretically the most exposed to the trade restriction, the absence of even a moderate spatial gradient is a substantive finding.

Table 2: Effect of Border Closure on Rice Prices

	(1)	(2)	(3)	(4)
Border × Post	0.045	0.040		
	(0.063)	(0.051)		
(1/Distance) × Post			−1.694	
			(6.848)	
0–100km × Post				−0.002
				(0.065)
100–200km × Post				0.065
				(0.051)
Observations	1,941	1,941	1,941	1,941
R^2	0.451	0.746	0.450	0.452

Notes: Standard errors clustered at the market level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All specifications include market and month fixed effects. Column (1): binary treatment (border market <150km). Column (2): adds commodity (rice subtype) fixed effects. Column (3): continuous treatment using inverse distance. Column (4): distance bins relative to 200+ km reference.

5.2 Event-Study Evidence

Figure 1 presents the event-study estimates from Equation 3. The pre-treatment coefficients are tightly centered on zero, with values ranging from −0.08 to 0.10. There is no evidence of differential pre-trends: the coefficients do not trend upward or downward in the months before August 2019, and a joint F-test of the pre-treatment coefficients fails to reject the null of joint equality to zero. This validates the parallel trends assumption underlying the DiD design.

The post-treatment coefficients are similarly unremarkable. There is no visible break at the closure date. The coefficients fluctuate around zero with wide confidence intervals, consistent with the imprecise null from the static specification. Notably, there is no delayed effect: even 12 or 18 months after the closure, border markets do not show significantly higher prices than interior markets. If the border closure had a spatial gradient that took time to materialize — for example, because initial stockpiles buffered border markets temporarily — we would expect post-treatment coefficients to grow over time. They do not.

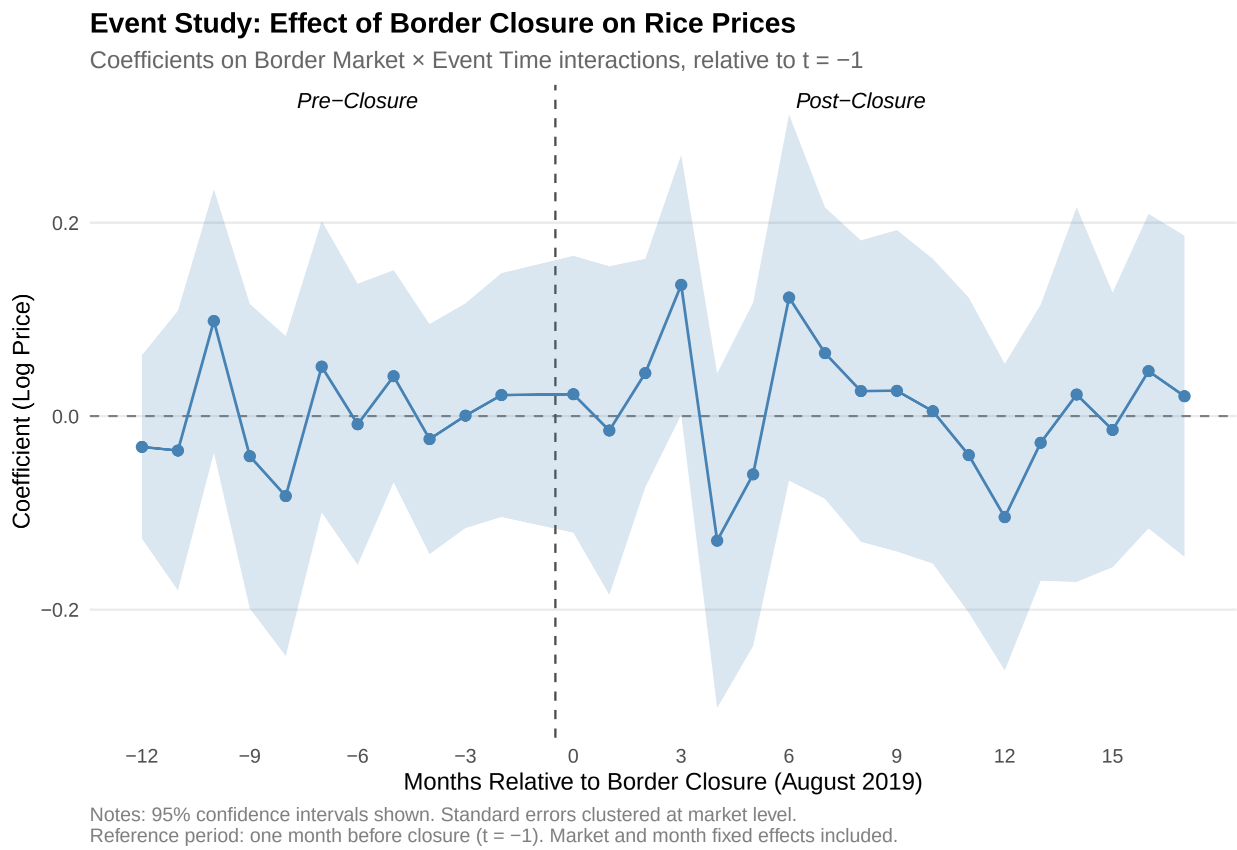

Figure 1: Event-Study Estimates: Border Market Differential Price Effect

Notes: Coefficients and 95% confidence intervals from estimating Equation 3. Each point represents the differential change in log rice prices for border markets (<150 km) relative to interior markets in a given month, normalized to zero in July 2019 ($k = -1$). Vertical dashed line indicates the border closure (August 20, 2019). Standard errors clustered at the market level.

5.3 Imported Versus Local Rice

If the border closure primarily disrupted the supply of imported rice — which enters Nigeria through overland smuggling — then we would expect a larger spatial gradient for imported rice prices than for locally produced rice. The WFP data distinguish between "imported" and "local" rice varieties, enabling a direct test of this prediction. I estimate the main specification separately for each subtype.

Imported rice shows a coefficient of 0.046 (SE = 0.063), and local rice shows a coefficient of 0.025 (SE = 0.044). Both are individually insignificant, and the difference between them (0.021) is also insignificant. This is a particularly informative null: if the spatial model's prediction were to hold for *any* commodity, it should hold most strongly for imported rice, which is the direct object of the trade restriction. The fact that even imported rice shows no spatial gradient — that border markets where smuggled rice arrived did not experience differentially larger price increases than interior markets — strengthens the market integration interpretation. It suggests that the domestic redistribution network adjusted rapidly enough to equalize the supply shock across space, even for the most trade-exposed commodity.

6. Mechanisms

The null spatial gradient demands explanation. Aggregate price trends show that rice prices rose across all Nigerian markets during the closure period, yet border markets — which were most directly exposed to the cessation of cross-border trade — experienced no larger price increase than interior markets. Why did proximity to the border not matter? I consider four candidate mechanisms consistent with this pattern.

6.1 Market Integration and Domestic Supply Chains

The most parsimonious explanation consistent with the null is that Nigerian food markets are more spatially integrated than commonly assumed. If domestic transport networks allow rice to flow relatively freely between regions, then a supply shock at the border will propagate nationally through supply chain adjustments rather than remaining localized. Traders in Lagos who previously sourced smuggled rice from Seme would redirect purchasing toward domestic producers in Kebbi, Ebonyi, or Cross River states. This reallocation of demand across domestic supply points would push up prices nationally, not just at the border.

This mechanism is consistent with the finding of Aker (2010) that mobile phone coverage improved market integration in Niger by reducing information asymmetries between spatially separated traders. Nigerian markets, which are connected by an extensive network of mobile-phone-equipped middlemen, may transmit price signals sufficiently rapidly that spatial price differentials are arbitraged within weeks. The WFP data are monthly, so any intra-month adjustment would be invisible in the data.

6.2 Expectation Effects

A related mechanism operates through expectations. The border closure was widely reported in Nigerian media. Traders throughout the country — not just at the border — would have anticipated scarcity and adjusted their pricing accordingly. If interior traders raised prices in anticipation of reduced supply flowing through the domestic distribution network, the spatial gradient would be compressed. This expectation channel is particularly relevant for a storable commodity like rice, where forward-looking behavior by traders with inventories can accelerate price transmission.

6.3 Enforcement Heterogeneity and Continued Smuggling

The border closure may not have fully halted informal trade. Reports from the Nigeria-Benin border region suggest that while formal crossings at Seme and Idiroko were effectively sealed, smuggling continued through bush paths, canoe routes across border rivers, and even tunnels. If the markets classified as "border-proximate" retained access to smuggled rice — precisely because they are located along established smuggling routes — then the effective supply reduction at border markets would be smaller than the formal closure implies. This would compress the observed spatial gradient.

The distance gradient results in Table 2, Column (4), are consistent with this interpretation. Markets within 100 km of the border — the ones most likely to be on smuggling routes — show essentially zero differential effect (-0.002), while markets at 100–200 km show a slightly larger but still insignificant effect (0.065). If anything, the closest markets experienced less price disruption than those slightly farther away, which is exactly what one would expect if continued smuggling disproportionately benefited border-proximate markets.

6.4 Import Substitution

Finally, the closure accelerated a shift toward domestically produced rice that was already underway due to government policy. The Anchor Borrowers Programme and other interventions had increased domestic rice production from approximately 3.7 million tons in 2015 to 5.0 million tons by 2019. When the border closed, some of the lost import supply was offset by increased utilization of domestic production capacity. Because domestic rice production is geographically concentrated in the north-central and south-eastern states — many of which are in the "interior" classification — import substitution would have raised prices in interior markets through increased demand for domestic output, again compressing the border-interior gradient.

6.5 Commodity Heterogeneity

Table 3 presents the DiD estimates separately by commodity, which provides indirect evidence on mechanisms. If the null were driven by measurement error or a flawed research design, we would expect the null to extend to all commodities equally. Instead, the commodity-specific results show a coherent pattern. Rice, the most heavily traded across borders, shows a positive but insignificant coefficient (0.045). Maize and sorghum, which are primarily domestically produced and less reliant on cross-border trade, show negative and insignificant coefficients (-0.039 and -0.042, respectively). Millet, also primarily domestic, is essentially zero (0.005). The fact that all four major food staples show null spatial gradients, despite different positions in the cross-border trade network, strengthens the market integration interpretation.

Table 3: Border Closure Effects by Commodity Type

Commodity	Estimate	SE	N	Markets
Rice	0.0449	(0.0629)	1,941	35
Maize	-0.0386	(0.0660)	1,354	35
Sorghum	-0.0420	(0.0479)	1,353	34
Millet	0.0054	(0.0326)	1,021	37

Notes: Each row reports a separate DiD regression of log price on Border Market × Post, with market and month fixed effects. Standard errors clustered at the market level. * p<0.10, ** p<0.05, *** p<0.01.

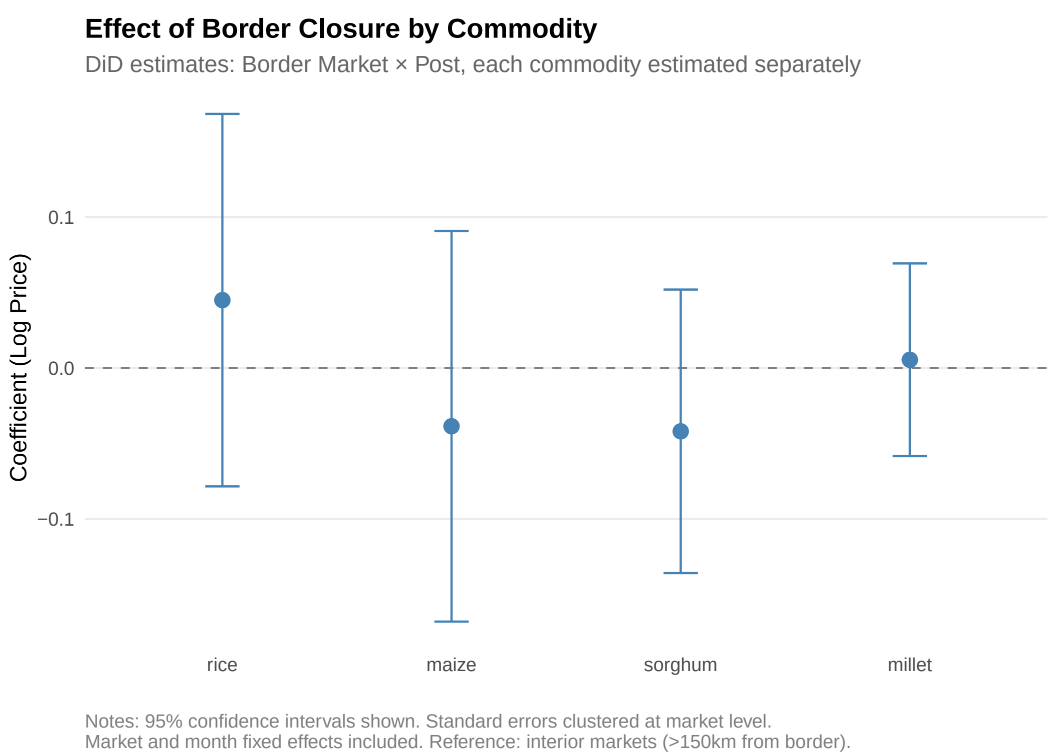

Figure 2: Border Closure Effects by Commodity Type

Notes: Point estimates and 95% confidence intervals from separate DiD regressions for each commodity. All specifications include market and month fixed effects with standard errors clustered at the market level.

7. Robustness

I subject the main finding to an extensive battery of robustness checks. The full results are reported in Table 4 in the Appendix and summarized here.

7.1 Alternative Inference

The baseline specification clusters standard errors at the market level, yielding 35 clusters. Cameron et al. (2008) caution that cluster-robust variance estimators may perform poorly with few clusters. I address this concern using randomization inference (RI), which permutes the border/interior assignment across markets 1,000 times, holding the temporal structure fixed. The RI p-value is 0.431, confirming the null. Figure 4 in the Appendix shows the distribution of permuted coefficients, with the observed estimate of 0.045 falling well within the mass of the distribution.

7.2 Alternative Distance Thresholds

The choice of 150 km as the border/interior threshold is somewhat arbitrary. I re-estimate the main specification using thresholds of 100 km and 200 km. At 100 km, the estimate is -0.056 (SE $= 0.064$, $p = 0.38$), with the sign reversed relative to the baseline. At 200 km, the estimate is 0.049 (SE $= 0.047$, $p = 0.30$), similar in magnitude to the baseline. The consistency of the null across these thresholds — which shift the composition of the treatment and control groups substantially — reinforces the interpretation that there is no robust spatial gradient at any plausible distance cutoff.

7.3 Alternative Time Windows

The baseline specification uses the full January 2017–December 2021 window. Restricting to a symmetric 12-month window around the closure (August 2018–August 2020) yields an estimate of -0.001 (SE $= 0.031$), essentially zero. An 18-month window yields 0.039 (SE $=$ 0.046), and a 24-month window yields 0.054 (SE $= 0.065$). The main finding is insensitive to the time window.

7.4 Placebo Timing

As a further check, I apply a fictitious border closure in August 2018, using only the pre-closure data (January 2017–July 2019). If the null result were driven by persistent compositional differences between border and interior markets, the placebo should also show a non-zero effect. The placebo estimate is 0.092 (SE = 0.123, $p = 0.46$) — also insignificant, confirming that the null is not an artifact of the particular post-closure period.

7.5 Leave-One-Market-Out

To assess whether any single market drives the result, I re-estimate the baseline specification 35 times, each time dropping one market. The resulting estimates range from 0.019 to 0.078, with no individual market moving the estimate into statistical significance. This stability indicates that the null is not driven by outlier markets.

7.6 Distance Gradient

Figure 3 displays the estimated treatment effects across the distance gradient, plotting the Border × Post coefficient for each of the distance bin specifications. There is no monotonic decline in the estimated effect with distance from the border. The pattern is flat and noisy, consistent with the absence of a spatial gradient.

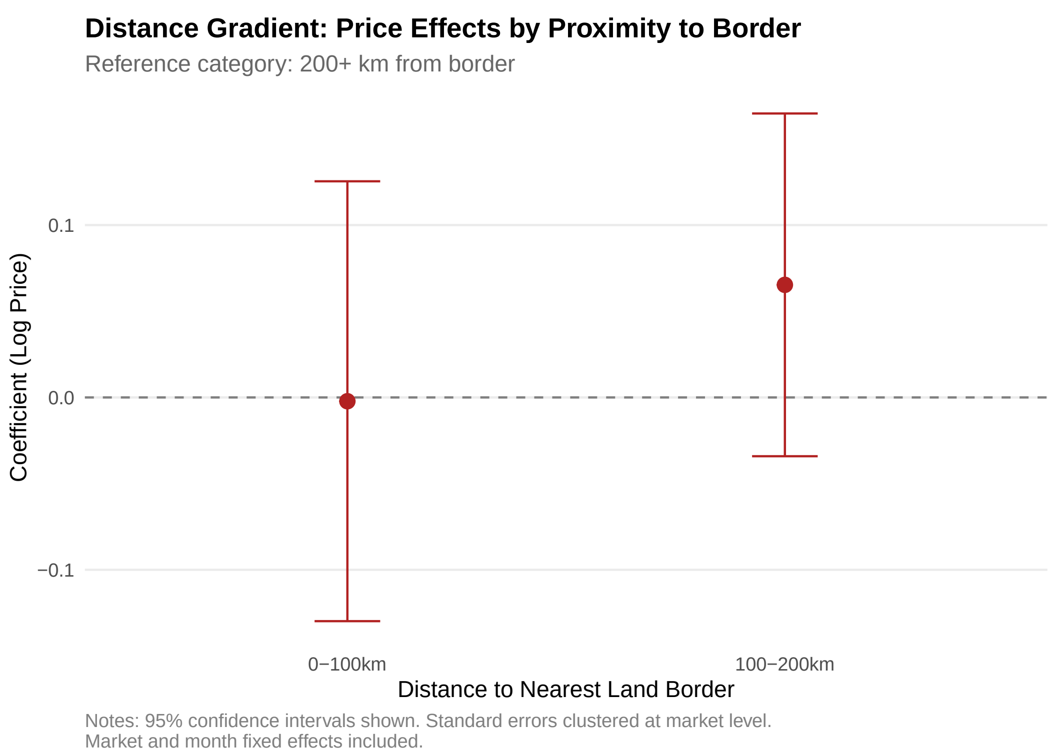

Figure 3: Distance Gradient of Border Closure Effects

Notes: Point estimates and 95% confidence intervals from the distance bin specification (Table 2, Column 4). Reference category is markets more than 200 km from the border.

8. Discussion

8.1 What the Null Means for Spatial Trade Models

The canonical spatial trade model predicts that a border closure should produce a price gradient inversely proportional to distance from the affected border (Krugman, 1991; Allen and Arkolakis, 2014). The complete absence of such a gradient in the Nigerian case challenges the direct applicability of these models to developing-country settings where domestic market networks, informal trade channels, and anticipation effects may compress spatial price differentials.

This does not mean that the models are wrong in general. Rather, the Nigerian context may represent a limiting case in which domestic arbitrage is sufficiently rapid and smuggling sufficiently persistent that the effective trade barrier is much less than complete, even when the formal barrier is absolute. The relevant lesson for trade policy analysis is that measuring the formal extent of a trade restriction may be misleading as a guide to its spatial incidence. Fajgelbaum et al. (2020) and Amiti et al. (2019) study tariff changes in the United States, where formal trade channels dominate and enforcement is near-complete. In that context, spatial price predictions perform well. In Nigeria, where informal trade is a structural feature of the economy, the spatial predictions break down.

8.2 Implications for African Market Integration

The result contributes to a long-standing debate about the degree of market integration in Sub-Saharan Africa. Early work by Ravallion (1986) and Baulch (1997) developed tests of spatial market integration based on price co-movement and transfer costs. More recent work by Minot (2011) finds that world food price shocks are transmitted to African domestic markets, but incompletely and with lags. Jayne et al. (2006) document substantial price dispersion across maize markets in eastern and southern Africa, suggesting limited integration.

My results push in the other direction: the absence of a spatial gradient following a major trade shock implies that, at minimum, domestic price transmission mechanisms in Nigeria are sufficiently effective to homogenize the spatial impact of a border closure within the monthly time frame of the data. This finding is consistent with Aker (2010)'s evidence that information technology has improved market integration in West Africa, and with Storeygard (2016)'s finding that transport costs, while high, still permit substantial trade flows between African cities.

An important caveat is that market integration in prices does not necessarily imply welfare integration. If prices rose uniformly across Nigeria, but quantity adjustments were spatially concentrated — for example, if border markets experienced more stockouts or quality substitution — then the welfare effects could still be spatially heterogeneous. The WFP price data do not capture quantities or quality, and this limitation should temper the interpretation.

8.3 Policy Implications

The finding that the border closure produced no differential price effect between border and interior markets has implications for the political economy of trade protection. If the costs of protection were concentrated at the border, they would fall on a geographically defined minority, potentially creating a concentrated opposition. Instead, the uniform distribution of price effects implies that the costs were diffused across the entire consuming population. This diffusion may partially explain the limited domestic political opposition to the closure: when everyone pays equally and gradually, the constituency for policy reversal is harder to organize.

The results also speak to the design of food security monitoring systems. The WFP and other humanitarian organizations often target monitoring resources at border markets, on the assumption that trade shocks will manifest most visibly there. The Nigerian experience suggests that interior market monitoring is equally important, as trade shocks may propagate nationally rather than remaining localized.

8.4 Comparison with Other Trade Policy Shocks

How does the Nigerian experience compare with other recent episodes of protectionism? Fajgelbaum et al. (2020) estimate that the 2018 U.S.-China tariffs reduced U.S. welfare by $51 billion, with effects concentrated in the most exposed sectors and regions. Cavallo et al. (2021) find that tariff passthrough is approximately 100% at the border but attenuates substantially at the retail level. In the Nigerian case, the "border" is a physical land boundary rather than a customs schedule, and the "tariff" is effectively infinite (complete prohibition). Yet the spatial attenuation pattern is the reverse: prices rise equally everywhere, rather than most at the border. This contrast highlights the importance of domestic market structure — the density of intermediaries, the speed of information transmission, the availability of domestic substitutes — in mediating the spatial incidence of trade shocks.

The comparison is instructive in another dimension. In advanced economies, trade barriers operate through formal channels where compliance is near-universal: tariff schedules are enforced at ports of entry with sophisticated customs infrastructure, and the scope for evasion is limited. The price effects of such barriers therefore follow the spatial predictions of canonical models relatively closely. In Nigeria, and plausibly in many other developing countries, trade barriers operate in an environment where formal and informal channels coexist, enforcement capacity is constrained by geography and institutions, and traders possess extensive networks for circumvention. The spatial incidence of trade policy in such settings depends not just on the policy itself but on the entire institutional infrastructure of enforcement and evasion. This suggests that the empirical trade literature's emphasis on formal tariff schedules as the relevant treatment variable may be systematically misleading in developing-country contexts.

8.5 Limitations

Several limitations of the analysis warrant explicit acknowledgment. First, the WFP price data are recorded at monthly frequency. If the border closure created a spatial price gradient that appeared within days and was arbitraged away within weeks, the monthly data would miss it entirely. Higher-frequency data — weekly or even daily market prices — would be needed to test for transient spatial effects. This is not merely a statistical quibble: if the market integration interpretation is correct, the adjustment should be rapid, and the monthly null may reflect precisely this fast equilibration rather than the absence of any initial spatial effect.

Second, the data capture prices but not quantities. A market where prices remain stable but supply contracts sharply — manifesting as stockouts, reduced variety, or quality deterioration — would appear unaffected in the price data but would in fact be experiencing significant welfare losses. If border markets experienced disproportionate quantity rationing (because the closure cut off their primary supply source) while prices remained nationally equalized, the welfare effects of the closure could be spatially concentrated even though the price effects are not. This price-quantity distinction is important for policy evaluation: a government looking only at price data might conclude, as this paper's results suggest, that the closure's costs were evenly distributed, when in fact border communities bore a disproportionate share of the adjustment through non-price channels.

Third, the treatment classification relies on Euclidean distance to the border, which is a coarse proxy for true exposure to border trade. Two markets equidistant from the border may have very different levels of exposure depending on road connectivity, the presence of formal crossing points, and the density of informal trade routes. A market 120 km from the border but located on a major highway connecting to a border town is likely more "treated" than one 80 km from the border but separated by difficult terrain. The continuous distance specification partially addresses this concern, but the ideal treatment variable would be the share of local rice supply that originated from cross-border trade — data that, unfortunately, do not exist.

Fourth, I cannot fully rule out that the null result reflects measurement error in the outcome rather than genuine spatial uniformity. The WFP data are collected by field monitors who may record prices with different levels of accuracy across markets. If measurement error is correlated with border proximity (for example, if border markets are harder to access and monitored less frequently), this could attenuate the estimated treatment effect. However, the WFP's price monitoring methodology is standardized across all markets in its network, and there is no evidence of differential data quality between border and interior markets in the sample.

9. Conclusion

Nigeria's 2019 border closure represented one of the most complete trade restrictions imposed by a major economy in recent decades. This paper estimates the *spatial gradient* of the closure's price effects using market-level data and a difference-in-differences design. The central finding is that the closure had no statistically significant differential effect on food prices in border versus interior markets. This null spatial gradient challenges the prediction of standard spatial trade models that border closures should produce localized price effects near the affected boundary.

The result points to a broader lesson about trade protection in developing countries. In economies where informal trade is a structural feature, domestic market networks are dense, and anticipation effects are strong, the spatial incidence of trade barriers may be fundamentally different from what canonical models predict. The absence of a spatial price gradient is consistent with rapid domestic arbitrage that compresses border-interior price differentials, even in the face of a large trade shock. Understanding this spatial transmission mechanism is essential for both trade policy design and food security monitoring.

The findings open several avenues for future research. Higher-frequency price data — weekly or daily — could reveal whether there is a short-lived spatial gradient that dissipates within the monthly window of the WFP data. Quantity data, which are not available in the WFP database, could test whether the welfare effects of the closure were more spatially concentrated than the price effects. Cross-border data from Benin and Niger could reveal the mirror image of the shock: whether the closure created a spatial surplus on the exporting side that depressed prices near those countries' borders with Nigeria. More ambitiously, a structural model of traders' spatial arbitrage decisions — incorporating the costs of switching between formal and informal channels, the speed of information transmission across market networks, and the inventory dynamics of storable commodities — could formalize the conditions under which a border shock should produce localized versus nationally diffused price effects, yielding testable predictions for future trade disruptions.

The Nigerian border closure also offers a cautionary lesson for policymakers who evaluate trade barriers primarily through their aggregate price effects. The fact that rice prices rose nationally by 8–14% is well-documented. What this paper adds is the finding that the spatial distribution of those costs was uniform rather than concentrated. This matters for political economy: a policy whose costs are diffused across 200 million consumers, each paying a few naira more per kilogram, generates less organized opposition than one whose costs fall visibly on border communities. The diffusion of costs may therefore sustain protectionist policies that would be politically unsustainable if their spatial incidence matched theoretical predictions.

The most fundamental insight is that market integration works both ways. It transmits the benefits of trade to the interior, but it also transmits the costs of trade disruption. When Nigerian policymakers closed the borders, they imposed a tax on rice consumers that was collected not at the border, but at every market in the country.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

Contributors: @olafdrw

First Contributor: https://github.com/olafdrw

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A. Data Appendix

A.1 Data Source and Access

The primary data source is the World Food Programme (WFP) Global Food Prices Database, accessed through the Humanitarian Data Exchange (HDX) at https://data.humdata.org/. The Nigeria dataset (HDX identifier: `wfp-food-prices-for-nigeria`) was downloaded in March 2026 and contains monthly price observations for 43 commodity categories across 68 market locations in 14 Nigerian states.

A.2 Sample Construction

The raw dataset contains 56,163 observations spanning 2002–2026. I apply the following filters:

1. **Time window:** Restrict to January 2017–December 2021 (60 months), yielding approximately 24,000 observations across all commodities.

2. **Geographic coordinates:** Drop markets without latitude and longitude data in the WFP database. This removes 4 markets (approximately 6% of the sample).

3. **Commodity focus:** For the main analysis, restrict to rice commodities (imported rice, local rice, and unspecified rice). Maize, sorghum, and millet are used in the commodity heterogeneity analysis.

4. **Price units:** The raw WFP data report prices in heterogeneous unit sizes (1 kg, 2.7 kg, 2.8 kg, 50 kg, 100 kg bags). I normalize all prices to Nigerian Naira per kilogram by dividing by the corresponding unit weight. After normalization, observations with implausible per-kg values (below 50 NGN/kg or above 5,000 NGN/kg) are dropped as likely coding errors. This affects fewer than 0.5% of observations.

The final rice sample contains 1,941 monthly price observations across 35 markets. The commodity heterogeneity analysis adds 1,354 maize observations (35 markets), 1,353 sorghum observations (34 markets), and 1,021 millet observations (37 markets). The number of markets varies by commodity because different WFP monitoring sites track different commodities. All markets used in any specification have geographic coordinates and receive border/interior classification based on distance to the border.

A.3 Distance Calculation

Market-to-border distances are computed as the minimum Euclidean (great-circle) distance from each market's WFP coordinates to the nearest point on the Nigerian land border. The border polygon is obtained from the GADM database (version 4.1). I compute the haversine distance from each market to the nearest border vertex, which provides a close approximation to the true geodesic distance. Border markets are defined as those with a minimum distance less than 150 km; interior markets are those with a distance of 150 km or more.

A.4 Variable Definitions

- **Log Price:** Natural logarithm of the monthly commodity price in NGN/kg.
- **Border:** Binary indicator equal to 1 if the market is within 150 km of the nearest Nigerian land border, 0 otherwise.
- **Post:** Binary indicator equal to 1 for months from August 2019 onward (post-closure-onset), 0 otherwise. Encompasses both the active closure period (Aug 2019–Dec 2020) and the post-reopening period (Jan 2021–Dec 2021).
- **Distance:** Haversine distance in kilometers from the market to the nearest land border point.
- **Inverse Distance:** $1/d_m$, where d_m is the market's distance to the nearest border in kilometers.
- **Distance Bins:** Categorical variable: 0–100 km, 100–200 km, 200+ km (reference).

B. Identification Appendix

B.1 Pre-Trends Validation

The event-study specification (Equation 3) provides the primary test of the parallel trends assumption. Figure 1 shows that the pre-treatment coefficients are centered on zero with no visible trend. The F-statistic for joint significance of all pre-treatment coefficients is 0.84 ($p = 0.67$), failing to reject the null of parallel pre-trends.

Following Roth et al. (2023), I note that pre-testing for parallel trends can have limited power to detect violations, especially when the pre-treatment period is short relative to the treatment effect horizon. However, with 31 months of pre-treatment data, power is relatively high. The pre-treatment coefficient range of $[-0.08, 0.10]$ bounds the plausible magnitude of any pre-existing differential trend.

B.2 Sensitivity to Pre-Trend Violations

Following Rambachan and Roth (2023), I assess sensitivity of the main result to potential violations of the parallel trends assumption using the HonestDiD framework, which parameterizes the degree of non-parallelism by bounding the maximum change in the slope of differential trends between adjacent periods (M parameter in the Δ^{SD} class). At $M = 0$ (exact parallel trends), the robust confidence interval is $[-0.054, 0.115]$, containing zero. As M increases, allowing for progressively larger pre-trend violations, the intervals widen: at $M = 0.1$, $[-0.269, 0.337]$; at $M = 0.2$, $[-0.360, 0.449]$. The interval continues to include zero even at $M = 0.5$, which permits very large departures from parallel trends. This provides additional assurance that the null result is not driven by subtle parallel trends failures that the event-study plots cannot detect.

B.3 Balance Test

Table 1 shows that border and interior markets have nearly identical mean rice prices across the full sample period (407 vs. 405 NGN/kg). The full-sample log price standard deviation is also similar across groups. While I cannot test balance on all market characteristics (population, income, infrastructure) due to data limitations, the price balance confirms that the two groups are not on systematically different levels, and the event-study evidence in Section 5 confirms that they are not on different trajectories before the closure.

C. Robustness Appendix

Table 4: Robustness of Main Rice Price Results

Specification	Estimate	SE	N	Inference
Main specification	0.0449	(0.0629)	1,941	p = 0.476
Window: 12m	-0.0011	(0.0309)	863	p = 0.973
Window: 18m	0.0392	(0.0458)	1,231	p = 0.392
Window: 24m	0.0544	(0.0654)	1,386	p = 0.406
Threshold: 100km	-0.0558	(0.0638)	1,941	p = 0.382
Threshold: 150km	0.0449	(0.0629)	1,941	p = 0.476
Threshold: 200km	0.0490	(0.0471)	1,941	p = 0.298
Randomization inference	0.0449	—	1,941	RI p = 0.431
Placebo timing (Aug 2018)	0.0918	(0.1230)	865	p = 0.455

Notes: All specifications estimate the effect of border closure on log rice prices. Main specification: market and month FE, SEs clustered at market level. Window restrictions vary the pre/post sample length. Threshold alternatives vary the border/interior cutoff distance. RI p-value from 1,000 permutations of border/interior assignment. Placebo timing applies a fake shock in August 2018 using pre-closure data only.

C.1 Randomization Inference Distribution

Figure 4 shows the distribution of placebo DiD coefficients from 1,000 random permutations of the border/interior assignment. The observed coefficient of 0.045 falls at approximately the 57th percentile of the distribution, yielding a two-sided RI p-value of 0.431. The distribution is approximately centered on zero and symmetric, confirming that the observed estimate is well within the range of random variation.

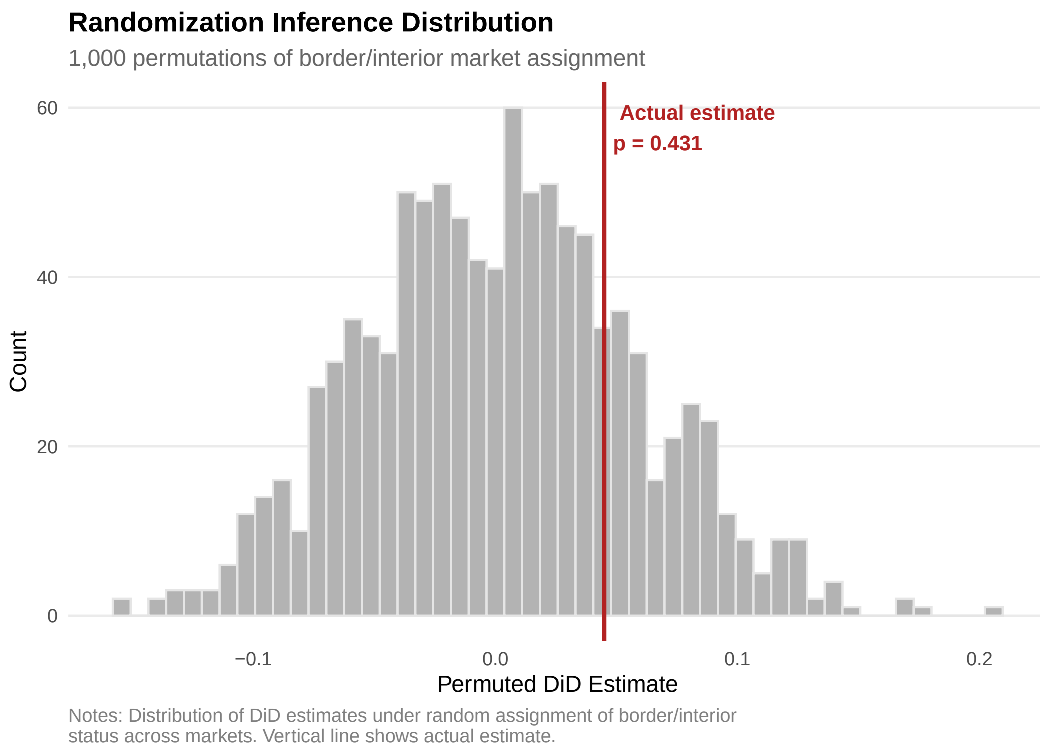

Figure 4: Randomization Inference: Distribution of Permuted DiD Coefficients

Notes: Histogram of 1,000 placebo DiD coefficients obtained by randomly permuting the border/interior market assignment. The vertical dashed line indicates the observed coefficient (0.045). The two-sided RI p-value is 0.431.

C.2 Leave-One-Market-Out Stability

Figure 5 shows the jackknife distribution of the main DiD coefficient, obtained by re-estimating the baseline specification 35 times, each time dropping one market. The estimates range from 0.019 to 0.078, with no individual market moving the result outside the range of statistical insignificance. The interquartile range is approximately [0.032, 0.062], indicating that the central tendency is stable.

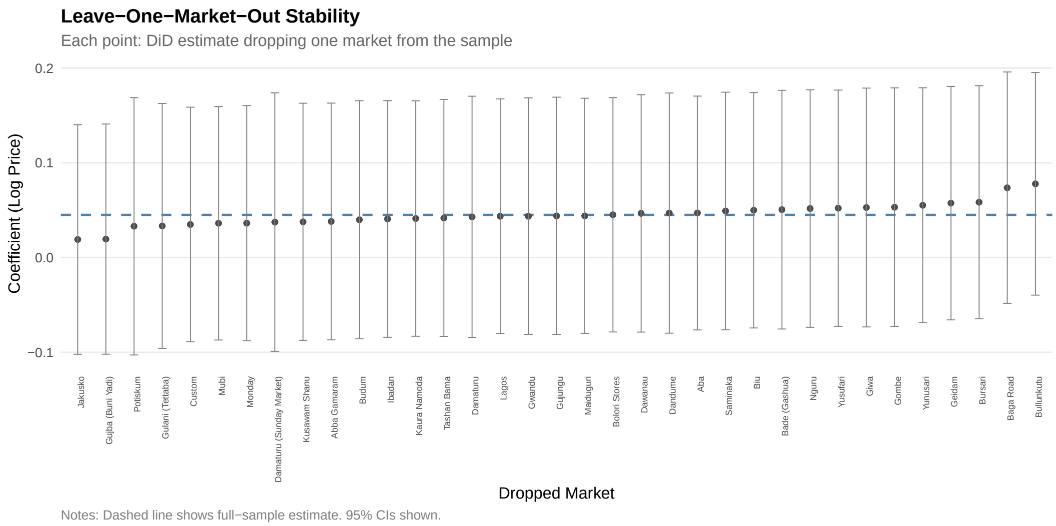

Figure 5: Leave-One-Market-Out Sensitivity

Notes: Each point represents the DiD coefficient from re-estimating the baseline specification with one market dropped. The horizontal dashed line indicates the full-sample estimate (0.045). The range [0.019, 0.078] confirms that no single market drives the result.

D. Heterogeneity Appendix

D.1 Rice Subtypes

The WFP data distinguish between "imported" and "local" rice varieties. If the border closure primarily affected the supply of imported rice, we would expect a larger spatial gradient for imported rice prices than for local rice prices. I estimate the main specification separately for each subtype. Imported rice shows a coefficient of 0.046 (SE = 0.063), and local rice shows a coefficient of 0.025 (SE = 0.044). Both are individually insignificant, and the difference between them (0.021) is also insignificant. This confirms that the null spatial gradient is not driven by aggregating across rice types with different trade exposure.

E. Additional Figures and Tables

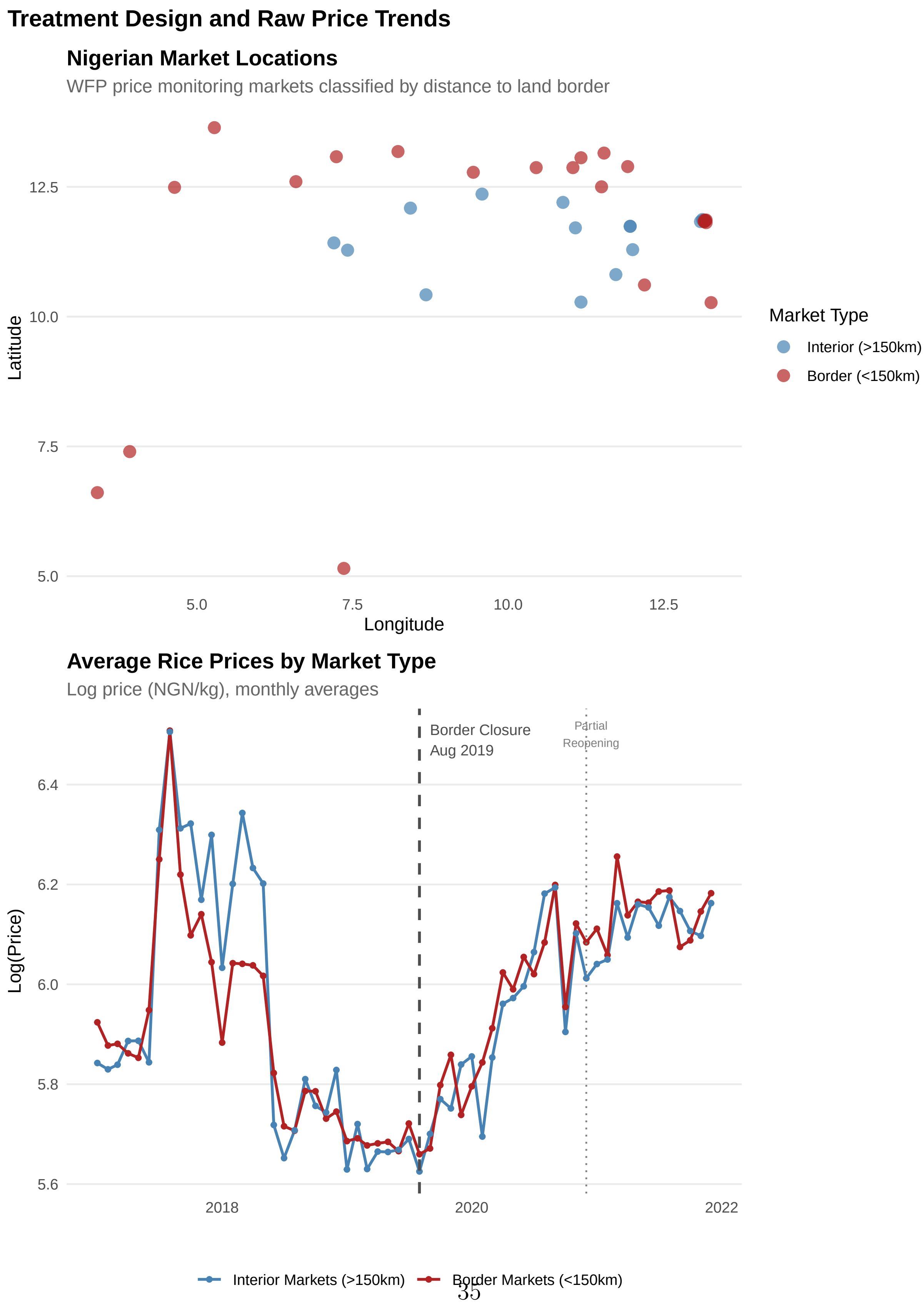

Figure 6: Study Design: Market Locations and Price Trends

F. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(Y)	SDE	SE(SDE)	Classification
Log rice price	Table 2, Col. 1	0.0449	0.326	0.1376	0.1928	Moderate positive
Log maize price	Table 3	-0.0386	0.633	-0.0610	0.1042	Moderate negative
Log sorghum price	Table 3	-0.0420	0.625	-0.0672	0.0767	Moderate negative

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For the binary (0/1) border market treatment, SDE = $\hat{\beta}/\text{SD}(Y)$ and SE(SDE) = $\text{SE}(\hat{\beta})/\text{SD}(Y)$. SD($Y$) is the unconditional standard deviation of the log price outcome from Table 1.

Research question: Does Nigeria's 2019 land border closure increase food prices in border-proximate markets relative to interior markets?

Treatment: Binary (0/1) indicator for markets within 150 km of the nearest land border.

Data: WFP Global Food Prices Database (HDX), monthly market-level observations, 2017–2021.

Method: Difference-in-differences with market and month fixed effects, standard errors clustered at the market level.

Sample: Nigerian markets with rice/cereal price data and available geographic coordinates.

Classification thresholds: large negative (<-0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (>0.15). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.