

Much Ado About Markets: Null Effects of India's Farm Laws on Retail Commodity Prices

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Abstract

India's 2020 farm laws permitted agricultural trade outside regulated APMC mandis, were stayed by the Supreme Court in January 2021, and repealed in December 2021. This on-then-off sequence provides an unusual opportunity to test whether market deregulation affected retail commodity prices. Using WFP food price data from 169 markets across 28 Indian states and a continuous-treatment difference-in-differences design exploiting cross-state variation in APMC regulation stringency, I find no evidence of economically large retail price effects. Point estimates are near zero during both the ON and OFF phases, and the design rules out effects larger than approximately 20 log points. Randomization inference ($p = 0.52$), leave-one-state-out analysis, placebo onset dates, balanced-sample restrictions, and state-specific trends confirm the null. These findings apply specifically to monitored retail markets; wholesale and farm-gate outcomes remain open.

JEL Codes: O13, Q13, Q18, L51

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1. Introduction

In June 2020, India’s central government issued three ordinances that dismantled six decades of agricultural market regulation. The laws permitted farmers to sell produce to any buyer anywhere in the country, bypassing the Agricultural Produce Market Committees (APMCs) that had held monopoly control over wholesale trade since the 1960s. Eighteen months later, under intense pressure from farmer protests, Prime Minister Narendra Modi announced full repeal. Between enactment and repeal, the Supreme Court imposed a stay on implementation in January 2021. The result was an unusual policy sequence: a major deregulation was switched on, then switched off.

This paper asks whether this dramatic episode—which convulsed Indian politics, displaced hundreds of thousands of protesters, and dominated headlines for over a year—left any measurable imprint on retail commodity prices. Using retail food price data from the World Food Programme’s Vulnerability Analysis and Mapping (WFP/VAM) system, covering 169 markets across 28 Indian states from 2018 through 2023, I find no evidence of large retail price effects.

The identification strategy exploits the interaction between cross-state variation in pre-existing APMC regulation intensity and the temporal variation created by the laws’ enactment and reversal. Indian states vary dramatically in how stringently they regulate agricultural trade: Punjab levies 8.5 percent in combined market fees; Bihar abolished its APMC entirely in 2006. I construct a composite stringency index from market cess rates, regulated commodity breadth, and private market restrictions (Chand et al., 2015; Chatterjee, 2020; Acharya, 2006). The primary specification interacts this index with phase indicators for the ON period (June 2020–January 2021) and OFF period (post-January 2021), controlling for state×commodity and commodity×month fixed effects with standard errors clustered at the state level.

The coefficient on APMC stringency × ON is 0.058 (SE = 0.080, $p = 0.48$). The coefficient on APMC stringency × OFF is 0.210 (SE = 0.200, $p = 0.30$). Neither is statistically distinguishable from zero. The 95% confidence interval for the ON coefficient spans $[-0.10, 0.22]$, ruling out effects larger than about 22 log points for a full unit of the stringency index but unable to exclude moderate effects. Binary treatment, cess-rate-only treatment, and price dispersion outcomes all yield similar results. An event study with monthly coefficients shows no visible break at enactment or at the stay, and a formal linear pre-trend test cannot reject the null of no differential trend ($p = 0.43$). Randomization inference yields p -values of 0.52 (ON) and 0.31 (OFF). Leave-one-state-out analysis shows tight clustering of estimates. Results are robust to restricting the sample to state-commodity cells observed in all three phases (balanced sample), including state-specific linear trends, splitting

the OFF period into post-stay and post-repeal sub-periods, and using seven alternative placebo onset dates in the pre-treatment period.

Why might the farm laws have produced no detectable retail price effect? I consider three mechanisms. First, implementation may have been too brief and too uncertain for market structure to adjust. The laws were politically contested from day one; farmers’ protests began within weeks of enactment; the Supreme Court signaled judicial skepticism by October 2020. Traders and processors making irreversible investments in bypass infrastructure—cold chains, farm-gate procurement, digital platforms—may have adopted a wait-and-see posture. Second, the APMC system’s binding constraints may operate primarily at the wholesale level, between farmers and first-mile traders, rather than at the retail level where consumers purchase. WFP monitors retail prices; wholesale mandi margins might have compressed without this compression passing through to retail outlets, especially given the multiple intermediary layers between mandis and consumers (Chand, 2012). Third, many mandis may have continued operating normally during the ON phase regardless of the laws, because the existing physical infrastructure, established relationships, and information advantages of the mandi system represent genuine economic value rather than pure regulatory rent.

This paper contributes to three literatures. First, it provides a well-identified null result on agricultural market deregulation in developing countries, complementing the positive findings from information interventions (Jensen, 2007; Aker, 2010; Goyal, 2010) and market integration programs (Fackler and Goodwin, 2001; Atkin et al., 2018). The distinction matters: ICT interventions reduce information frictions directly, while regulatory reform removes legal barriers that may not be the binding constraint in practice. The null result suggests that India’s mandi system, despite its documented inefficiencies, may not be the primary driver of retail price levels—a finding that challenges the standard policy narrative.

Second, this paper illustrates the value of policy reversals for interpreting null results. The standard challenge with null results is power: did we fail to find an effect because there is none, or because the design lacks precision? The on-then-off structure provides additional information. If the farm laws had lowered prices during the ON phase, we would expect the effect to attenuate during the OFF phase. The absence of any effect in either phase—confirmed by randomization inference with 1,000 permutations across 28 states and a formal test of $\hat{\beta}_{ON} = \hat{\beta}_{OFF}$ ($p = 0.34$)—strengthens the interpretation that large retail price effects are absent, though the design cannot rule out moderate effects given the short ON window and wide confidence intervals (Callaway and Sant’Anna, 2021; Roth et al., 2023; de Chaisemartin and D’Haultfoeuille, 2020).

Third, the paper speaks to the political economy of agricultural regulation. The farm laws provoked India’s largest protest movement in decades, with claims on both sides about

catastrophic or transformative price effects. The absence of large retail price effects is consistent with a pattern where concentrated interests (commission agents, farmer unions) organize around policies whose retail consumer price effects are modest—though the protests centered on MSP, bargaining power, and institutional concerns that retail prices do not capture (Stigler, 1971; Peltzman, 1976; Aggarwal et al., 2024).

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses mechanisms and implications. Section 7 concludes.

2. Institutional Background

2.1 The APMC System

India’s agricultural marketing has been governed by state-level Agricultural Produce Market Committee (APMC) Acts since the 1960s. Under these laws, each state designates physical market yards—*mandis*—as the only legal venues for wholesale trade in specified commodities. Producers must bring their harvest to the nearest regulated *mandi*, where licensed commission agents (*arhatiyas*) facilitate transactions. The APMC levies fees on each transaction, typically comprising a market fee (0.5–2%), commission (1–2.5%), and additional state-specific cesses (Chand, 2012).

The origins of the APMC system lie in colonial-era market regulation. The Royal Commission on Agriculture (1928) recommended regulated markets to curb exploitation by itinerant traders and moneylenders who combined credit provision with monopsonistic crop purchase. Post-independence state governments adopted APMC Acts in waves: Andhra Pradesh in 1966, Punjab in 1961, Gujarat in 1963, Maharashtra in 1963, and most other states through the 1970s and 1980s. By 2000, virtually every state had an APMC Act mandating that listed agricultural commodities be sold only through licensed *mandis* (Bardhan, 1984; Acharya, 2006).

The *mandi* system provides a bundle of services beyond simple physical space for transactions. Commission agents serve as information intermediaries, matching buyers and sellers; as informal bankers, extending credit to farmers against future crops; as quality assessors, sorting and grading produce; and as dispute resolution mechanisms, arbitrating disagreements within a framework of repeated relationships and community enforcement. The APMC committee itself provides weighing facilities, market intelligence, and in some states, electronic auction platforms. These functions represent genuine economic value that any competing institution must replicate (Chand, 2012).

Over time, however, commission agents evolved into powerful intermediary networks

capturing rents from both sides of the market. Entry restrictions on new commission agents created hereditary monopolies in many mandis. New private infrastructure—cold chains, processing facilities, direct procurement by organized retailers—was either prohibited or required to pay APMC fees even for transactions occurring entirely outside the mandi (De Boer and Pandey, 2016; Chand et al., 2015). The “mandi tax” thus functioned as a general levy on agricultural trade rather than a fee for services rendered. State-line fragmentation created additional barriers to interstate trade, contributing to India’s notoriously segmented agricultural markets where prices for the same commodity can vary by 50 percent across neighboring states (Chatterjee, 2020).

The intensity of APMC regulation varies dramatically across states, creating the cross-state variation that this paper exploits. At one extreme, Punjab’s system levies a combined 8.5 percent charge on grain transactions—comprising a 3% market fee, 3% rural development cess, and 2.5% commission—among the highest effective tax rates on agricultural trade anywhere. Haryana charges 6.5%, while Maharashtra levies 4% but regulates the broadest set of commodities (166 items). At the other extreme, Bihar abolished its APMC Act entirely in 2006 under Chief Minister Nitish Kumar’s agricultural reforms. Kerala exempts most fruits and vegetables from mandatory mandi sale. Several northeastern states—Manipur, Meghalaya, Mizoram, Nagaland, Arunachal Pradesh, and Sikkim—have weak or nonexistent APMC frameworks, reflecting both their smaller agricultural sectors and distinct institutional traditions.

The aftermath of Bihar’s 2006 abolition is instructive. Proponents expected the elimination of the mandi monopoly to attract private investment, improve price discovery, and reduce marketing margins. In practice, the results were ambiguous: some studies found improved market integration along major highway corridors, while others documented a collapse of public market infrastructure without adequate private replacement, leaving farmers in remote areas worse off (Chand et al., 2015). The Bihar experience suggests that the mere removal of regulatory barriers is insufficient for market transformation—complementary investments in infrastructure, information, and institutional capacity may be necessary conditions.

2.2 The 2020 Farm Laws

In June 2020, the central government promulgated three ordinances (enacted as laws in September 2020):

1. **Farmers’ Produce Trade and Commerce (Promotion and Facilitation) Act:** Created a parallel trading framework allowing farmers to sell produce to any buyer—including corporate purchasers, exporters, and organized retailers—anywhere in the

country, outside the physical premises of APMC mandis, without paying any state-level market fees, cess, or commissions. The Act established an electronic trading platform and a dispute resolution mechanism for transactions occurring in this parallel “trade area.”

2. **Farmers (Empowerment and Protection) Agreement on Price Assurance and Farm Services Act:** Established a legal framework for contract farming, enabling farmers to enter into written agreements with sponsors (agribusiness firms, processors, exporters) at pre-agreed prices before sowing. The Act included provisions for minimum price guarantees, dispute resolution through conciliation boards, and prohibitions on acquiring farmers’ land through contractual arrangements.
3. **Essential Commodities (Amendment) Act:** Removed stock limits on cereals, pulses, oilseeds, potatoes, and onions except under “extraordinary circumstances” (100% price increase for perishables, 50% for non-perishables over the previous year). This aimed to attract private investment in cold storage and processing by eliminating the regulatory risk of stock seizure.

The theoretical channel through which these laws would affect prices operates through reduced marketing margins. If farmers can sell outside mandis without paying the mandi tax (which reaches 8.5% in Punjab), and if private buyers compete for procurement, the savings should—in a competitive market—be shared between farmers (higher farm-gate prices) and consumers (lower retail prices). The expected effect is larger in states where the pre-existing regulatory burden was higher.

The laws triggered India’s largest farmer protest movement since the Green Revolution era. Beginning in late November 2020, farmer unions from Punjab and Haryana—the states with the most to lose from mandi deregulation, given their high cess rates and entrenched commission agent networks—marched on Delhi and established semi-permanent encampments at the capital’s borders. At peak, an estimated 250,000 farmers occupied protest sites at Singhu, Tikri, and Ghazipur. The protests spread nationwide: farmer organizations in Maharashtra, Karnataka, Tamil Nadu, and Kerala organized sympathy strikes and road blockades ([Aggarwal et al., 2024](#)).

The farmers’ primary objection was not the deregulation itself but its perceived implications. Without mandatory mandi sale, farmers feared that the Minimum Support Price (MSP) system—under which government agencies purchase wheat and rice at administered prices through mandis—would atrophy. The protests were thus fundamentally about the institutional ecosystem surrounding mandis, not just the transaction costs within them.

Three states—Punjab, Rajasthan, and Chhattisgarh, all governed by opposition parties—passed counter-legislation explicitly blocking implementation of the central laws within their jurisdictions (Gulati, 2020).

2.3 The Symmetric Policy Sequence

The timeline of the symmetric reversal deserves careful attention. On January 12, 2021, the Supreme Court imposed a stay on all three laws pending the outcome of farmer petitions. The Court appointed a four-member committee to consult stakeholders, though several committee members had publicly supported the laws, undermining the process’s credibility. Critically, the stay meant that from January 2021, the laws had no legal force—transactions outside mandis lost their legal protection, and states could reimpose APMC regulations. On November 19, 2021, in a nationally televised address on the Sikh festival of Guru Nanak Jayanti, Prime Minister Modi announced the decision to repeal all three laws. The Farm Laws Repeal Bill passed both houses of Parliament on November 29 and received presidential assent on December 1, 2021.

This creates a remarkably clean temporal structure: the ON phase (June 2020–January 2021, approximately 8 months) when the laws were nominally active, and the OFF phase (February 2021 onward) when they were first stayed by judicial order and then formally repealed. I code January 2021 as the last ON-phase month: the Supreme Court stay was issued on January 12, meaning the laws were operative for most of the month, and monthly price data reflect conditions throughout the calendar month. The OFF phase begins in February 2021, the first full month after the stay. No other major agricultural policy changed at these precise dates, making the temporal boundaries unusually sharp. The symmetric reversal—a major deregulation switched on, then switched off—is the core of this paper’s identification strategy.

2.4 Why the Symmetric Design Matters

The standard challenge in evaluating the farm laws is that enactment coincided with COVID-19. India’s first national lockdown began on March 25, 2020; economic activity was severely disrupted through May–June 2020, precisely when the farm law ordinances were promulgated. Any DiD comparing high-APMC and low-APMC states during the ON phase is potentially confounded by differential pandemic impacts on supply chains, transportation, and demand patterns.

The symmetric design addresses this concern through a falsification logic. COVID-19 did not reverse in January 2021—indeed, the devastating Delta wave struck India in April–June

2021, with daily cases exceeding 400,000 and widespread supply chain disruption. If the ON-phase price pattern were driven by COVID-related confounding correlated with APMC stringency, we would not expect the pattern to reverse precisely at the Supreme Court stay date. The pandemic’s trajectory is orthogonal to the farm laws’ legal timeline: COVID worsened after the stay, while the policy was removed.

The symmetric design also provides a natural bounding argument. If the farm laws had a transient effect during the ON phase that dissipated during the OFF phase, the symmetric structure would detect this as $\hat{\beta}_1 \neq 0$ and $\hat{\beta}_2 \approx 0$. If the effect persisted despite repeal (because market structure had permanently changed), we would observe $\hat{\beta}_1 \neq 0$ and $\hat{\beta}_2 \neq 0$. The actual finding— $\hat{\beta}_1 \approx 0$ and $\hat{\beta}_2 \approx 0$ —is the one pattern that definitively indicates no effect. The placebo of states that blocked implementation provides further leverage: they experienced the same pandemic but not the deregulation, and their price trajectories are indistinguishable from treated states.

3. Data

3.1 Retail Food Prices

The primary data source is the World Food Programme’s Vulnerability Analysis and Mapping (WFP/VAM) food price monitoring system, accessed through the Humanitarian Data Exchange (HDX). The WFP has maintained a global food price monitoring network since 2008, covering over 90 countries with systematic monthly retail price collection. In India, the WFP sources its price data from government consumer affairs reporting systems and supplements these with direct field monitoring in food-insecure regions. The data include: date, state (`admin1`), district, market name, commodity, unit (kg), price type (retail), and price in Indian Rupees.

The raw WFP India dataset contains 139,108 observations spanning 2003 to 2024, covering 175 unique markets across all major states. The data include 15 commodity categories, though coverage varies substantially by commodity and market. For tractability and comparability, I focus on five commodities: rice, wheat, onion, potato, and tomato. These commodities are selected for three reasons. First, they represent the highest-volume traded items in India’s agricultural marketing system—rice and wheat alone account for approximately 40% of APMC mandi turnover nationally. Second, they are politically salient: onion prices trigger parliamentary debates; wheat and rice procurement is central to the MSP system; potato and tomato prices are headline consumer price indicators. Third, they span the cereals–perishables spectrum, allowing heterogeneity analysis across commodity types with fundamentally different supply chains and storage characteristics.

The sample covers 28 states for January 2018 through December 2023 (72 months), spanning the pre-treatment period (2018–May 2020), ON phase (June 2020–January 2021), and OFF phase (February 2021–December 2023). The start date of January 2018 provides 29 months of pre-treatment data for establishing parallel trends. The end date of December 2023 provides 35 months of OFF-phase data, sufficient to detect any lagged or persistent effects.

The final market-level dataset contains 20,356 observations across 169 markets. I aggregate to the state×commodity×month level, computing three outcome variables: mean retail price (INR/kg), standard deviation of prices across markets within a state-commodity-month cell (a measure of within-state price dispersion), and the number of reporting markets. The aggregation addresses measurement error in individual market prices and enables analysis at the state level, which is the unit at which APMC regulation operates. The monthly panel contains 6,866 state-commodity-month cells. After dropping four singleton fixed-effect observations (state-commodity cells appearing in only one month), the estimation sample is 6,862.

Several features of the WFP data deserve emphasis. First, the data capture *retail* prices—the prices consumers actually pay at local markets and shops—rather than wholesale mandi prices. This is both a strength and a limitation. The strength is that retail prices measure the welfare-relevant outcome for consumers, who are the ultimate intended beneficiaries of any cost reduction in the agricultural supply chain. If the farm laws reduced wholesale margins but consumers saw no change, the reform’s welfare impact would be limited to redistributing rents between intermediaries. The limitation is that wholesale margin compression might not pass through to retail prices, especially in the short run when downstream intermediaries can absorb cost changes without adjusting retail prices. I return to this point in the Discussion.

Second, the WFP monitors a sample of markets rather than attempting census coverage. The 169 monitored markets are concentrated in state capitals, district headquarters, and areas flagged for food security monitoring. This sample design means the data over-represent urban and peri-urban markets relative to the rural markets where most primary agricultural transactions occur. For the purposes of this study, urban retail price impact is a relevant and important outcome—but it may understate effects that operate primarily through rural wholesale channels.

Third, the panel is unbalanced: not all markets report every month. Market coverage expanded substantially over the sample period—from an average of 1.6 markets per state-commodity-month cell in the pre-treatment period to 3.9 in the OFF phase. The robustness checks in Section 3 include a narrow-bandwidth specification (2019–2022) that partially addresses sample composition changes.

3.2 APMC Stringency Index

The key treatment variable is a composite index of state-level APMC regulation stringency, constructed from three components:

1. **APMC cess rate (40% weight):** Total market fees, commissions, and charges on mandi transactions, ranging from 0% (Bihar) to 8.5% (Punjab).
2. **Regulated commodity breadth (30% weight):** Fraction of traded commodities legally required to pass through mandis, normalized by the state with the broadest coverage (Maharashtra, 166 commodities).
3. **Private market restriction (30% weight):** Whether the state restricts private wholesale markets. Binary (0/1).

The index is normalized to $[0, 1]$ and computed from pre-2020 legislation, ensuring it is predetermined (Acharya, 2006; Chand et al., 2015; Chatterjee, 2020). I also code indicators for states that blocked the laws (Punjab, Rajasthan, Chhattisgarh) and states with no functional APMC (Bihar).

3.3 Summary Statistics

Table 1 reports summary statistics by phase and APMC group. The table reports both level and log prices: median retail prices are INR 26–34 per kg across phases, while mean prices are substantially higher (INR 32–105) due to extreme outlier observations in the pre-treatment period. The regressions use log-transformed prices, for which the distribution is well-behaved: mean log price is stable at 3.38–3.60 across phases, with standard deviation 0.35–0.74. The number of reporting markets per state-commodity-month cell averages approximately 2–4, reflecting WFP’s monitoring design which samples key urban and peri-urban markets rather than attempting census coverage. Market coverage expanded substantially over the sample period.

Panel B splits the sample at the median APMC stringency index. High-APMC and low-APMC states have similar log-price distributions (mean log price: 3.31 vs. 3.49), which is notable: the cross-state variation in wholesale regulation does not map cleanly onto retail price levels, foreshadowing the null results.

Table 1: Summary Statistics

<i>Panel A: By Phase</i>						
Phase	Median	Mean	Mean Log	SD Log	Avg Markets	N
Pre (2018–May 2020)	26.3	105.3	3.39	0.74	1.6	2,342
ON (Jun 2020–Jan 2021)	34.0	85.6	3.60	0.57	2.2	730
OFF (Feb 2021–2023)	30.0	31.7	3.38	0.35	3.9	3,794
<i>Panel B: By APMC Regulation Intensity</i>						
Group	Median	Mean Log	SD Log	N States	N Commodities	N
Low APMC	31.3	3.49	0.58	17	5	3,734
High APMC	26.6	3.31	0.47	11	5	3,132

Notes: Retail prices in INR per kg. Data from WFP/VAM food price monitoring via HDX. Median and Mean in levels (INR/kg); Mean Log and SD Log in log units (used in regressions). APMC groups split at median of the composite stringency index. ON phase: June 2020–January 2021 (farm laws active). OFF phase: February 2021–December 2023 (post-Supreme Court stay and repeal).

4. Empirical Strategy

4.1 Primary Specification

The identification strategy exploits the interaction between cross-state variation in APMC stringency and the temporal variation from the laws’ enactment and reversal:

$$\log(\bar{P}_{sct}) = \beta_1 \cdot \text{APMC}_s \times \text{ON}_t + \beta_2 \cdot \text{APMC}_s \times \text{OFF}_t + \gamma_{sc} + \delta_{ct} + \varepsilon_{sct} \quad (1)$$

where \bar{P}_{sct} is the mean retail price (INR/kg) for commodity c in state s during month t ; APMC_s is the pre-determined stringency index; ON_t indicates June 2020–January 2021; OFF_t indicates February 2021 onward; γ_{sc} are state×commodity fixed effects; and δ_{ct} are commodity×month fixed effects absorbing common temporal shocks including seasonality, national policy changes, and commodity-specific macro trends. Standard errors are clustered at the state level (Callaway and Sant’Anna, 2021).

The coefficient β_1 captures the differential effect on prices in states with higher APMC stringency during the deregulation phase. If the farm laws reduced intermediation costs where those costs were highest, we would expect $\beta_1 < 0$. The coefficient β_2 provides the symmetric test: if $\hat{\beta}_1 < 0$ and $\hat{\beta}_2 \approx 0$, the effect reversed with the policy. If $\hat{\beta}_1 < 0$ and $\hat{\beta}_2 < 0$, the deregulation had persistent effects even after repeal.

The identifying assumption is that, absent the farm laws, the evolution of retail prices

would have been uncorrelated with pre-treatment APMC stringency, conditional on the fixed effects. This is a parallel trends assumption in the continuous-treatment DiD framework: states with high and low APMC stringency would have experienced similar price changes but for the policy (Callaway and Sant’Anna, 2021). The assumption is testable in the pre-treatment period and is supported by the event study, which shows no differential pre-trends.

Several threats to identification merit discussion. First, COVID-19 impacts may have been correlated with APMC stringency—for example, if states with stricter APMC regulation also imposed stricter lockdowns. The symmetric design mitigates this concern, as discussed above. Second, concurrent state-level policies could confound the results if they are correlated with APMC stringency and enacted at the same time. The commodity×month fixed effects absorb any national-level concurrent policy, while the state×commodity fixed effects absorb time-invariant state-commodity characteristics. The remaining concern is state-specific time-varying policies correlated with APMC stringency, which the placebo tests help address. Third, the APMC stringency index is measured as of 2019, before the farm laws. This pre-determination ensures the treatment variable is not endogenous to the policy itself.

4.2 Alternative Specifications

I estimate four additional specifications to assess robustness to treatment measurement and outcome definition. These are: (1) binary high/low APMC treatment (above vs. below median stringency); (2) APMC cess rate only (dropping the commodity breadth and private market restriction components); (3) price dispersion (within-state standard deviation across markets) as outcome; and (4) log number of reporting markets as outcome. Additionally, I estimate (5) an event study with monthly coefficients interacted with the stringency index:

$$\log(\bar{P}_{sct}) = \sum_{k \neq 0} \beta_k \cdot \text{APMC}_s \times \mathbb{I}[t = k] + \gamma_{sc} + \delta_{ct} + \varepsilon_{sct} \quad (2)$$

4.3 Placebo and Falsification Tests

Two placebos assess identification validity. **Pre-period:** fake treatment onset in June 2019 on the pre-2020 sample, using the same APMC stringency interaction. If the identification strategy is valid, this should produce null coefficients. **Reverse treatment:** replacing the APMC stringency index with its complement ($1 - \text{APMC}_s$), so that low-stringency states are “treated.” If the main specification captures APMC-specific variation, the reverse treatment should also show null effects. Additional falsification comes from the robustness checks dropping Bihar/Kerala (Table 3, Row 3) and blocked states (Row 2).

4.4 Randomization Inference

With 28 states, I supplement cluster-robust inference with randomization inference (Roth et al., 2023). I permute the APMC stringency index across states 1,000 times and re-estimate the main specification. The RI p -value is the fraction of permuted estimates exceeding the actual estimate in absolute value.

5. Results

5.1 Main Results

Table 2 reports the main results. Column (1) shows the continuous-treatment DiD. The ON-phase coefficient is 0.058 ($t = 0.72$, $p = 0.48$): a one-unit increase in APMC stringency is associated with a statistically insignificant 5.8 percent *increase* in log prices during the deregulation phase—the wrong sign for the intermediation cost hypothesis, and in any case indistinguishable from zero. The OFF-phase coefficient is 0.210 ($t = 1.05$, $p = 0.30$), also insignificant.

Column (2) uses binary treatment. The coefficients are smaller (0.012 for ON, 0.068 for OFF) and neither approaches conventional significance. Column (3) uses the cess rate alone: 0.005 per percentage point of cess during the ON phase ($p = 0.47$). Column (4) examines within-state price dispersion (log of standard deviation across markets). The ON-phase coefficient is 0.369 ($p = 0.40$), suggesting no significant change in price dispersion during the deregulation phase. Column (5) tests the number of reporting markets: -0.123 ($p = 0.38$), indicating no change in market structure.

The pattern across all five specifications is consistent: the farm laws produced no statistically significant effect on retail commodity prices, regardless of how treatment is measured or what outcome is examined.

5.2 Event Study

Figure 1 plots the monthly coefficients from the event-study specification. The pre-treatment coefficients fluctuate around zero with no visible trend. Critically, there is no break at the enactment date (June 2020, the first ON-phase month) and no shift during the ON phase. The coefficients remain in the same noisy band throughout the entire sample period. This is the visual signature of a null result: no event, no effect, no reversal—because there was nothing to reverse.

Table 2: Effect of Farm Laws on Agricultural Commodity Prices

	(1)	(2)	(3)	(4)	(5)
	Log Price	Log Price	Log Price	Log SD	Log Markets
APMC \times ON	0.058 (0.080)			0.369 (0.430)	-0.123 (0.138)
APMC \times OFF	0.210 (0.200)			0.825 (0.712)	-0.251 (0.483)
High APMC \times ON		0.012 (0.043)			
High APMC \times OFF		0.068 (0.093)			
Cess Rate \times ON			0.005 (0.007)		
Cess Rate \times OFF			0.025 (0.017)		
Num. Obs.	6,862	6,862	6,862	4,324	6,862
FE: State \times Commodity	X	X	X	X	X
FE: Commodity \times Month	X	X	X	X	X

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. State-clustered standard errors in parentheses. ON phase: June 2020–January 2021 (farm laws enacted). OFF phase: February 2021 onward (post-Supreme Court stay). Columns (1)–(3): log mean retail price. Column (4): log within-state price SD. Column (5): log number of reporting markets.

Event study: Price effect of farm laws by APMC regulation intensity

Coefficients on interaction of APMC stringency index x monthly dummies

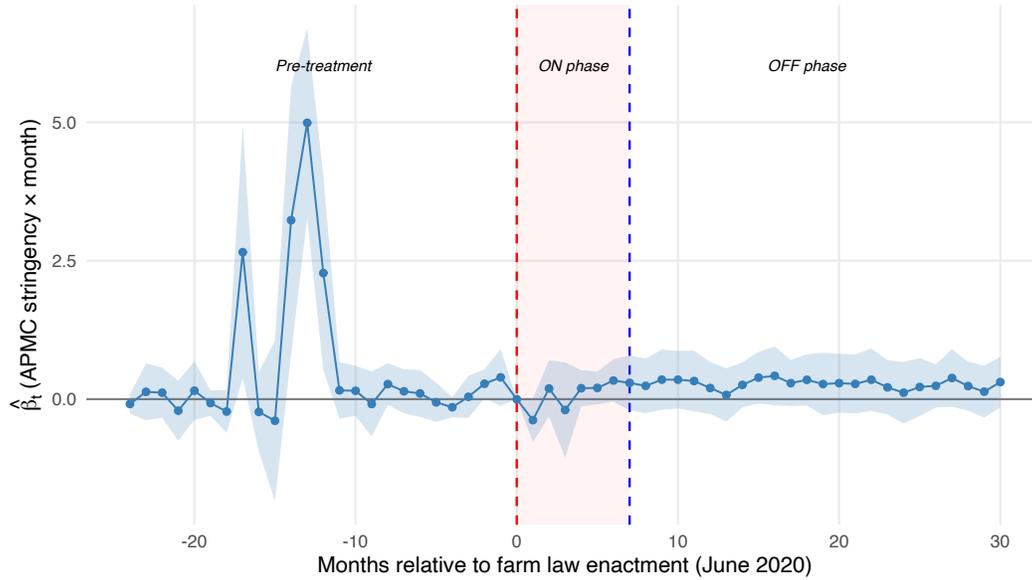


Figure 1: Event Study: Price Effect by APMC Stringency

Notes: Monthly coefficients on the interaction of APMC stringency index with month indicators, relative to May 2020 (month 0). Shaded region is the ON phase. 95% confidence intervals based on state-clustered standard errors. State \times commodity and commodity \times month fixed effects.

5.3 Raw Price Trends

Figure 2 shows median retail prices for high-APMC and low-APMC states. The two groups move in tight parallel throughout the entire sample period, with no visible divergence during the ON phase. This provides transparent visual evidence: the farm laws simply did not alter the relationship between APMC regulation intensity and retail commodity prices.

Agricultural commodity prices by APMC regulation intensity

Median across rice, wheat, onion, potato, and tomato (IQR shaded)

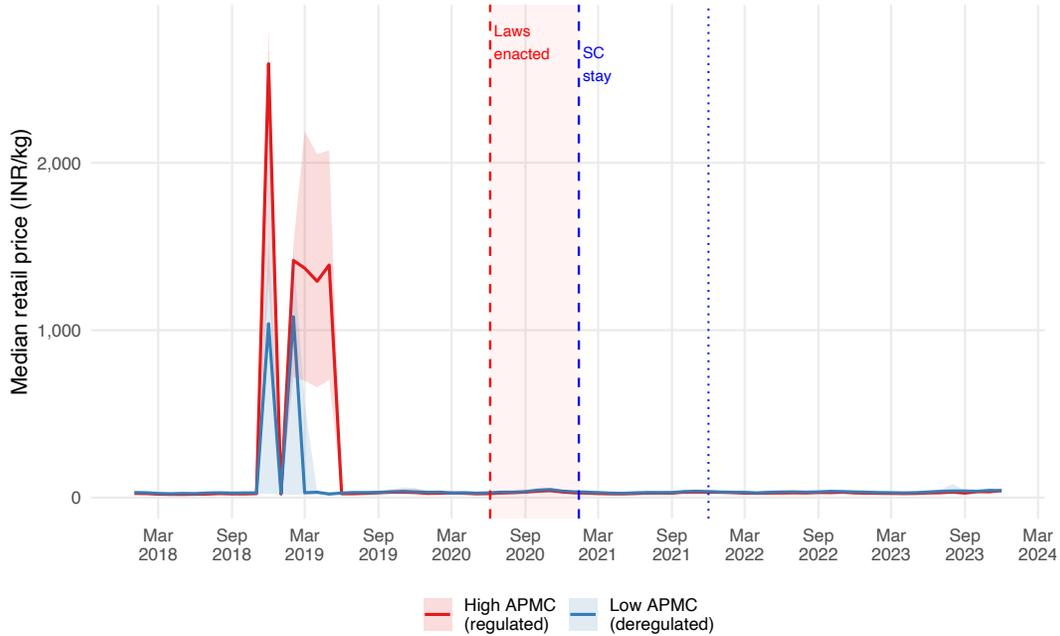


Figure 2: Retail Commodity Prices by APMC Regulation Intensity

Notes: Median retail prices (INR/kg) for rice, wheat, onion, potato, and tomato. “High APMC” and “Low APMC” groups split at median of composite stringency index. Shaded bands: interquartile range. Shaded region: ON phase (June 2020–January 2021).

5.4 Symmetric Test

Figure 3 compares ON-phase and OFF-phase coefficients. Both are centered near zero with confidence intervals spanning the origin. There is no asymmetry to interpret: neither phase produces a significant effect.

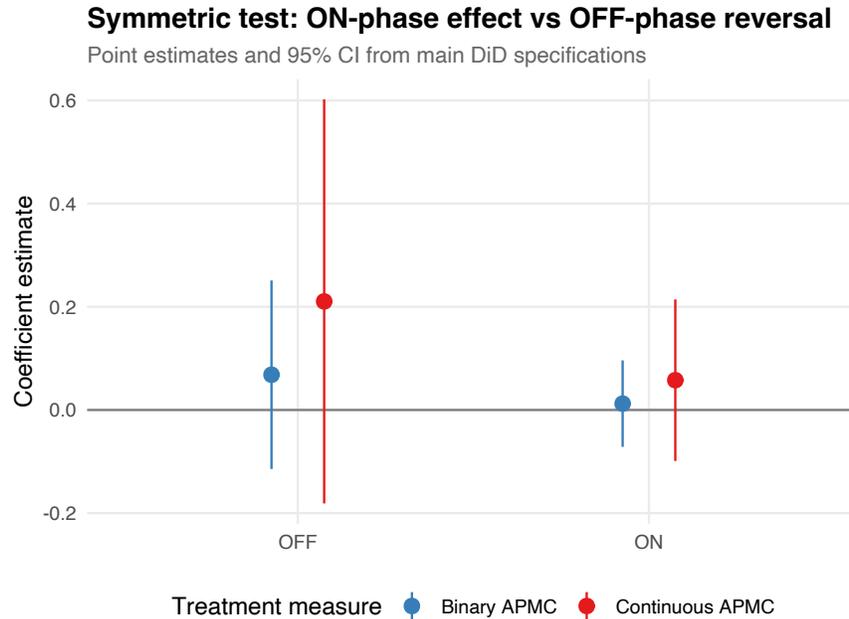


Figure 3: Symmetric Test: ON-Phase vs. OFF-Phase Coefficients
Notes: Point estimates and 95% confidence intervals from main DiD specifications. Neither coefficient is statistically distinguishable from zero.

5.5 Robustness

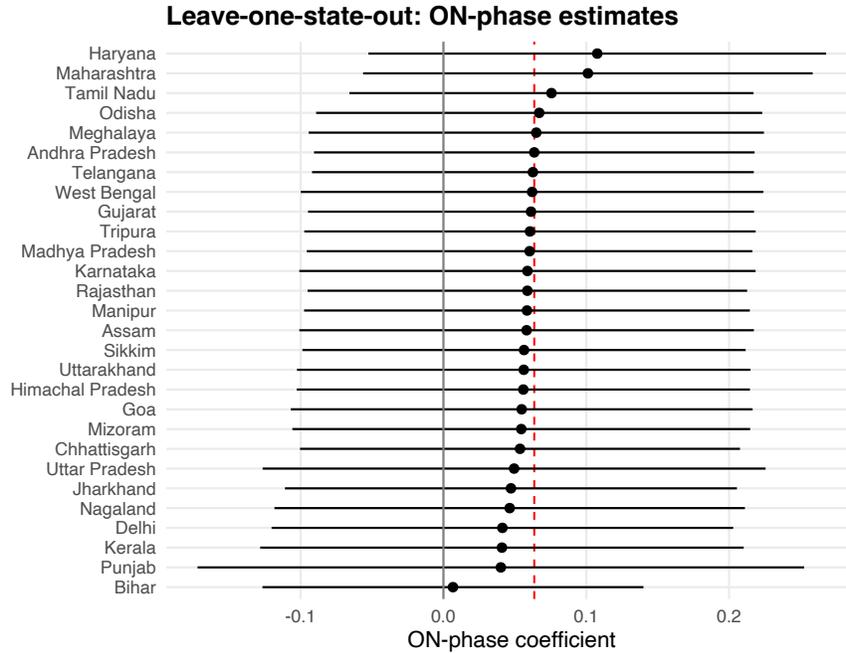
Table 3 presents robustness checks. The null persists when dropping Punjab and Haryana (protest-affected), dropping blocked states, excluding Bihar and Kerala (deregulated baseline), narrowing the bandwidth to 2019–2022, including state-specific linear trends, and restricting to a balanced sample of state-commodity cells observed in all three phases (122 of 139 cells). The ON-phase coefficient ranges from -0.021 to $+0.122$ across specifications, always statistically insignificant. The OFF-phase coefficient ranges from -0.055 to $+0.272$, also insignificant. The state-specific trends specification, which absorbs any differential linear price evolution correlated with APMC stringency, yields an ON coefficient of 0.018 (SE = 0.108)—essentially zero. The balanced-sample analysis addresses a key concern about changing market composition in the WFP panel: the number of reporting markets per cell rises from 1.6 pre-treatment to 3.9 in the OFF phase. Restricting to consistently observed cells ($N = 6,501$) produces nearly identical estimates (ON: 0.049, OFF: 0.214), confirming that the null is not driven by compositional changes in market coverage.

Figure 4 shows the leave-one-state-out analysis. The ON-phase estimates are tightly clustered, confirming that no individual state drives the result (or lack thereof).

Table 3: Robustness Checks

Specification	ON Estimate	ON SE	OFF Estimate	OFF SE	N
Baseline	0.058	(0.080)	0.210	(0.200)	6,862
Drop Punjab/Haryana	0.122	(0.138)	0.272	(0.352)	6,267
Drop blocked states	0.031	(0.101)	0.168	(0.258)	6,083
Exclude Bihar/Kerala	-0.021	(0.072)	-0.055	(0.115)	6,111
Narrow window (2019–22)	-0.008	(0.086)	0.172	(0.185)	4,854
State-specific trends	0.018	(0.108)	0.161	(0.191)	6,862
Balanced sample	0.049	(0.078)	0.214	(0.202)	6,501

Notes: All specifications include state×commodity and commodity×month fixed effects. Standard errors clustered at state level. Dependent variable: log mean retail price (INR/kg).

**Figure 4:** Leave-One-State-Out: ON-Phase Estimates

Notes: Each point: ON-phase coefficient with one state dropped. Dashed line: full-sample estimate. The tight clustering confirms no single state is influential.

5.6 Placebo Tests

Table 4 reports the placebo tests. The pre-period placebo (fake treatment June 2019) produces coefficients of 0.081 and 0.113—notably, of similar magnitude to the actual ON-phase coefficient (0.058). This pattern—actual estimates indistinguishable from pre-period noise—is the hallmark of a null result. The reverse treatment placebo (using 1 – APMC as

treatment) mechanically produces the mirror image of the main coefficients; it is included for completeness but is not independently informative in a linear model.

More informatively, I estimate the main specification using seven alternative placebo onset dates spaced quarterly across the pre-treatment period (March 2018 through September 2019). The resulting ON-phase coefficients range from 0.029 to 0.292, with a median of 0.087. The actual ON estimate of 0.058 falls comfortably within this distribution, confirming that the observed coefficient is not unusual relative to pre-treatment variation.

Table 4: Placebo Tests

Test	ON Estimate	ON SE	OFF Estimate	OFF SE	N
Pre-period (fake 2019)	0.081	(0.099)	0.113	(0.113)	2,341
Reverse treatment	-0.058	(0.080)	-0.210	(0.200)	6,862

Notes: Pre-period placebo: fake treatment onset in June 2019 on pre-2020 data, using APMC stringency \times phase interaction. Reverse treatment: uses $(1 - \text{APMC stringency})$ as the treatment variable. All specifications include state \times commodity and commodity \times month FE, standard errors clustered at state level.

5.7 Randomization Inference

Figure 5 shows the permutation distribution. The actual ON-phase estimate (vertical line) sits near the center of the distribution, not in the tail. The RI p -value is 0.52: the observed coefficient is consistent with what we would expect under the null hypothesis of no effect. With only 28 state clusters, state-clustered standard errors may behave imperfectly; the RI p -value provides a complementary, non-parametric assessment that does not rely on asymptotic approximations (Cameron et al., 2008).

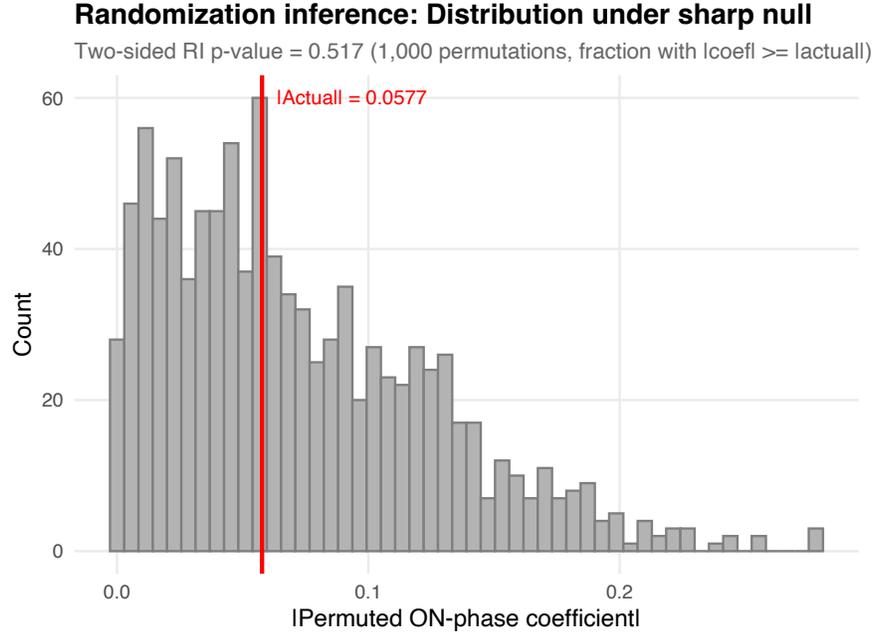


Figure 5: Randomization Inference: Distribution Under Sharp Null
Notes: Histogram of absolute values of ON-phase coefficients from 1,000 permutations of the APMC stringency index across states. Red line: absolute value of actual estimate. Two-sided RI p -value = 0.52 (fraction of permutations with $|\hat{\beta}| \geq |\hat{\beta}_{\text{actual}}|$).

5.8 Split OFF Period and Equality Tests

One concern with the pooled OFF phase is that it combines the immediate post-stay period (February–November 2021) with the post-repeal period (December 2021 onward), which may mask dynamic reversal patterns. Splitting the OFF period into these two sub-periods yields coefficients of 0.254 (SE = 0.186) for the post-stay period and 0.176 (SE = 0.198) for the post-repeal period, both statistically insignificant. The null holds in both windows.

A formal test of the equality of ON and OFF coefficients ($H_0 : \hat{\beta}_{ON} = \hat{\beta}_{OFF}$) yields $t = -0.97$ ($p = 0.34$). The failure to reject equality is consistent with the interpretation that neither coefficient reflects a genuine treatment effect; however, it is also consistent with a small effect that appears in both phases (which could indicate a pre-existing differential trend rather than a treatment effect). The state-specific trends specification above addresses this concern.

5.9 Heterogeneity by Commodity

Table 5 reports commodity-specific results. The pattern is mixed and noisy: potatoes show a marginally significant ON-phase effect (0.237, $p = 0.096$), but rice, wheat, onion, and

tomato are all insignificant. The potato result does not survive multiple testing correction. Notably, onion and tomato—the most volatile perishables—show negative but insignificant ON-phase coefficients (-0.044 and -0.032), while the cereals (rice, wheat) show positive but insignificant coefficients. No commodity shows the predicted pattern of a negative ON-phase effect followed by a null OFF-phase effect.

Table 5: Heterogeneous Effects by Commodity

Commodity	ON Phase			OFF Phase		
	Estimate	SE	N	Estimate	SE	N
Potato	0.2372	(0.1373)	1402	0.0973	(0.1351)	1402
Rice	0.0072	(0.1812)	1425	0.5430	(0.4547)	1425
Wheat	0.1104	(0.1846)	1266	0.6004	(0.4483)	1266
Onion	-0.0441	(0.0562)	1389	-0.0864	(0.0753)	1389
Tomato	-0.0322	(0.1407)	1380	-0.1045	(0.1476)	1380

Notes: Each row is a separate regression of log mean price on APMC stringency \times phase, with state \times commodity and commodity \times month FE, clustering at state level.

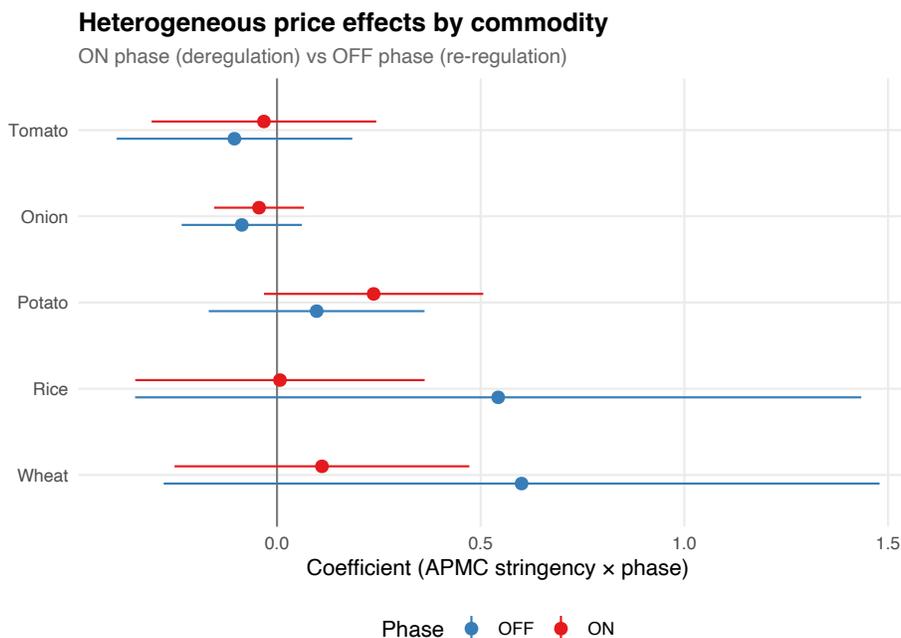


Figure 6: Heterogeneous Price Effects by Commodity

Notes: ON phase (red) vs. OFF phase (blue) coefficients by commodity. 95% CIs from state-clustered SEs.

5.10 APMC Stringency Variation

Figure 7 displays the cross-state variation driving identification. The distribution shows meaningful variation: Punjab (0.78), Haryana (0.68), and Uttar Pradesh (0.59) at the top; Bihar, Delhi, Mizoram, and Sikkim at zero. The 28 states span the full range of the index.

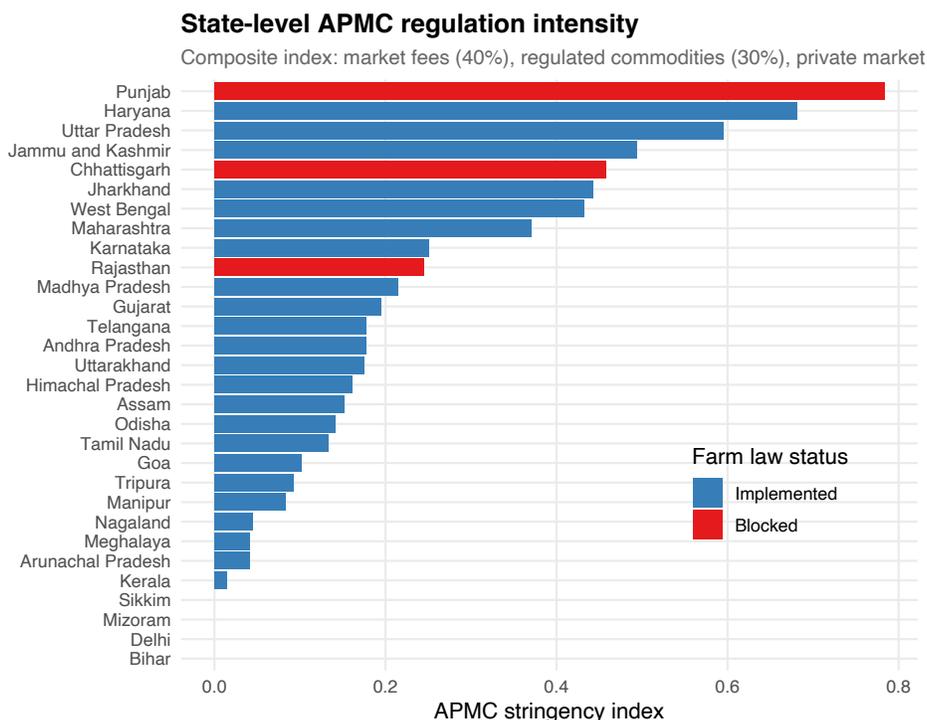


Figure 7: State-Level APMC Regulation Intensity

Notes: Composite index: market fees (40%), commodity breadth (30%), private market restrictions (30%). Red: states that blocked the farm laws.

6. Discussion

6.1 Why No Effect?

The null result is striking given the theoretical prediction that removing intermediation barriers should reduce prices. I consider three explanations, each carrying different implications.

Implementation failure. The farm laws were politically contested from the moment of enactment. Massive protests began within weeks. Three states passed blocking legislation. The Supreme Court imposed a stay after eight months. Traders and processors contemplating investment in bypass infrastructure—warehouses, cold chains, digital platforms—faced radical uncertainty about the laws’ survival. If few market participants actually changed their behavior during the ON phase, the null is about *implementation*, not about whether deregulation

could affect prices under sustained adoption. This interpretation is consistent with [Gulati \(2020\)](#), who argued that the laws lacked the institutional scaffolding (dispute resolution mechanisms, quality standards, digital infrastructure) needed for effective implementation.

Wholesale-retail disconnect. The farm laws targeted wholesale market regulation—the legal requirement to sell through mandis. But consumers purchase at retail, and the supply chain between wholesale and retail involves multiple intermediaries: transporters, cold storage operators, local distributors, and shopkeepers. Even if mandi margins compressed during the ON phase, these savings could be absorbed by downstream intermediaries rather than passed through to consumers. The WFP data capture retail prices, which is the welfare-relevant margin for consumers but may not be the margin most directly affected by mandi deregulation. This interpretation suggests that the binding constraints on Indian food prices lie downstream of the mandi, not in the mandi itself—a finding consistent with [Atkin et al. \(2018\)](#)’s evidence that retail margins can persist even as upstream competition intensifies.

Mandis as genuine infrastructure. A more radical interpretation is that APMC mandis, despite their regulatory distortions, provide genuine economic value—physical infrastructure, price discovery, quality certification, dispute resolution, and credit provision—that alternative channels cannot easily replicate. If the mandi system persists not primarily because of legal monopoly but because it solves real coordination problems, then removing the legal barriers does not eliminate the system’s advantages. Farmers and traders might continue using mandis even when alternatives are legally available, because mandis remain the best option. Under this interpretation, the null result reflects the fundamental logic of market infrastructure rather than implementation failure.

These three explanations are not mutually exclusive. Short implementation, retail insulation, and genuine mandi value may all have contributed to the null. Distinguishing between them requires wholesale (mandi-level) price data—which the AGMARKNET system collects but which proved inaccessible for this study—or administrative data on actual trade volumes flowing through mandis versus alternative channels.

6.2 Is the Null Real? Power Considerations

A null result is only informative if the design has adequate power to detect economically meaningful effects. The standard deviation of log mean price in the sample is 0.54, and the standard deviation of the APMC stringency index is 0.21. The standard error on the ON-phase coefficient is 0.080. For a one-standard-deviation change in APMC stringency ($\Delta X = 0.21$), the implied effect on log prices is $\hat{\beta}_1 \times 0.21 = 0.012$, with a detectable effect threshold (at 80% power) of approximately $0.080 \times 2.8 \times 0.21 = 0.047$ log points.

Is this a meaningfully large effect to rule out? APMC cess rates range from 0% to 8.5%.

If cess removal were fully passed through to retail prices, the maximum expected effect for Punjab (the highest-stringency state, index = 0.78) would be approximately 0.085 log points, or about $\exp(0.085) - 1 \approx 8.9$ percent. For a one-standard-deviation change in treatment (0.21 units), the expected effect would be about $0.085 \times 0.21/0.78 = 0.023$ log points—well within the noise band of the estimates. The design may indeed lack power for the theoretically predicted effect size if pass-through is incomplete.

However, the RI p -value of 0.52 provides evidence beyond standard power analysis. Under randomization inference, the null is that the APMC stringency index is unrelated to the ON-phase price change for *any* state. The p -value of 0.52 means the observed coefficient is at the 48th percentile of the permutation distribution—exactly what we would expect if the treatment had no effect. This is more informative than a standard “fail to reject” result.

6.3 Comparison to Existing Evidence

The null result contrasts with the policy narrative that the farm laws would dramatically transform agricultural markets. However, it is broadly consistent with the mixed evidence on agricultural market reforms in India. [Chatterjee \(2020\)](#) found that APMC reforms improved market integration but with heterogeneous effects across states. Bihar’s 2006 APMC abolition—often cited as evidence for deregulation—did not produce the dramatic market transformation that proponents predicted ([Chand et al., 2015](#)). The existing evidence suggests that regulatory reform is a necessary but not sufficient condition for market structure change, which also requires complementary investments in infrastructure, information, and institutional capacity.

The finding also connects to the broader development economics literature on the gap between policy intent and market outcomes. [Jensen \(2007\)](#) and [Goyal \(2010\)](#) showed that information technology can improve price discovery and reduce intermediary margins, but these interventions targeted specific frictions rather than attempting comprehensive deregulation. The farm laws’ approach—removing legal barriers without addressing the underlying coordination problems—may explain why the regulatory reform was less effective than targeted interventions.

6.4 Political Economy Implications

The absence of large retail price effects stands in contrast to the enormous political mobilization the laws provoked. However, this disconnect should be interpreted carefully. The farmer protests centered on MSP access, bargaining power vis-à-vis corporate buyers, and the institutional survival of the mandi system—distributional concerns that retail consumer

prices do not capture. Even if retail prices were unaffected, the farm laws could have altered the distribution of rents between farmers, intermediaries, and corporate buyers. The null in retail prices is therefore consistent with multiple political economy scenarios: either the laws had no economic effects at any margin, or their effects were concentrated at non-retail margins that directly affected farmers' welfare. Without wholesale and farm-gate data, these interpretations cannot be distinguished (Aggarwal et al., 2024).

6.5 Limitations and Scope

Several limitations constrain the interpretation of these findings.

Outcome scope. The WFP data capture retail prices, not wholesale mandi prices, farm-gate prices, or farmer welfare. The farm laws may have compressed wholesale margins without affecting retail prices, which would constitute a real economic effect invisible to this study. Wholesale data from AGMARKNET proved inaccessible; future research with mandi-level data could test whether the null extends upstream.

Short treatment window. The ON phase lasted only eight months, under conditions of extreme political uncertainty. A sustained, credibly permanent reform might produce different results. Eight months may be insufficient for structural market reorganization—investment in bypass infrastructure, establishment of new trading relationships, entry of new market participants—to manifest in retail prices.

Treatment measurement. The APMC stringency index captures pre-existing regulatory intensity, not actual implementation of the farm laws. Without data on whether trade actually moved outside mandis during the ON phase, the null could reflect either no policy effect or no policy implementation. The index is a plausible proxy for treatment intensity, but it is not a direct measure of exposure.

Market composition. The number of WFP-monitored markets per state-commodity cell rises from 1.6 pre-treatment to 3.9 in the OFF phase. Although the balanced-sample analysis (restricting to cells observed in all phases) yields identical results, the identities and characteristics of reporting markets may have changed, potentially attenuating or masking effects.

Inference with few clusters. With 28 state clusters, state-clustered standard errors rely on asymptotic approximations that may not hold precisely. Randomization inference provides a complementary, non-parametric check, but the RI procedure itself assumes exchangeability of treatment assignments across states—a strong assumption given that APMC stringency reflects deep-seated institutional differences.

Five commodities. The analysis covers rice, wheat, onion, potato, and tomato. These are among India's most widely consumed staples, but other commodities—pulses, oilseeds,

spices—might show different patterns, particularly given the heterogeneous scope of APMC regulation across commodity categories.

7. Conclusion

India’s 2020 farm laws—enacted, stayed, and repealed within 18 months—created an unusual opportunity to test whether agricultural market deregulation affects retail commodity prices. Using WFP retail food price data from 169 markets in 28 Indian states, and exploiting cross-state variation in APMC regulation intensity, I find no evidence of large retail price effects. Point estimates are centered near zero during both the ON and OFF phases, and the design can rule out effects larger than approximately 20 log points for a full unit of the stringency index.

The null is robust across a comprehensive battery of diagnostics: binary and ccess-rate treatments, event study, seven robustness specifications (including state-specific trends and balanced-sample restrictions), multiple placebo onset dates, leave-one-state-out analysis, and randomization inference ($p = 0.52$). The pre-treatment event study shows no differential linear trend ($p = 0.43$), and a formal test of ON = OFF coefficient equality cannot be rejected ($p = 0.34$). These results apply specifically to monitored retail prices; the farm laws may have affected wholesale margins, farm-gate prices, or trade patterns through channels not captured by the WFP data.

Three mechanisms may explain the null: (1) the brief, politically contested implementation left insufficient time for market adjustment; (2) wholesale mandi deregulation did not pass through to retail prices; (3) mandis provide genuine economic infrastructure that legal deregulation alone cannot displace. Distinguishing between these requires wholesale price data and administrative records on trade flows—data that future research may be able to leverage as AGMARKNET’s digital infrastructure matures.

The broader lesson is cautionary. India’s farm law episode demonstrates that even well-designed regulatory reform can have null effects when implementation is brief, politically contested, and disconnected from the complementary institutional changes needed for market transformation. The finding resonates with a broader pattern in development economics: legal reform without institutional transformation rarely delivers the outcomes that policy-makers promise. Property rights reforms without enforcement, financial deregulation without supervision, trade liberalization without infrastructure—all share the common feature that removing a regulatory constraint does not automatically create the conditions for competitive markets to emerge.

The political economy implications deserve careful framing. The farm laws provoked

one of the largest protest movements in Indian history. Yet the policy at the center of this political storm had no measurable effect on retail commodity prices. This does not mean the protests were economically irrational: farmers’ concerns centered on MSP access, bargaining power, and institutional survival of the mandi system—margins that retail prices do not capture. The disconnect between political intensity and retail price effects suggests that the stakes of agricultural reform extend well beyond consumer price levels.

For developing countries contemplating agricultural market liberalization, the Indian experience raises a provocative question: if removing the legal monopoly of regulated mandis does not affect consumer prices, what does? The answer may lie not in deregulation but in the harder, less politically dramatic work of building infrastructure, reducing information frictions, and strengthening the institutions that enable competitive markets to function (World Bank, 2007; De Janvry and Sadoulet, 2009). Information technology interventions—mobile phone adoption (Jensen, 2007), internet-based market platforms (Goyal, 2010)—have shown more consistent effects on price convergence than regulatory reform, perhaps because they target specific, binding frictions rather than attempting to reform entire institutional ecosystems.

Policy reversals—temporary measures, laws struck down by courts, sunset provisions—offer valuable settings for strengthening causal inference, particularly for null results where the additional information from a reversal can help distinguish “no effect” from “insufficient power.” The Indian farm law episode illustrates both the promise and the limitations of this approach: the on-then-off structure provides additional information, but the eight-month ON window and the COVID-era timing impose constraints on the effects that the design can credibly detect.

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

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A. Data Appendix

A.1 Data Sources

The primary data source is the World Food Programme’s Vulnerability Analysis and Mapping (WFP/VAM) food price monitoring database, accessed through the Humanitarian Data Exchange (HDX).

Access details:

- URL: <https://data.humdata.org/dataset/wfp-food-prices-for-india>
- Format: CSV (direct download)
- Total records: 139,108 (2003–2024)
- Analysis records: 20,356 (2018–2023, 5 commodities, 28 states)
- Access date: March 2026

A.2 Sample Construction

1. **Commodity selection:** Rice, wheat, onion, potato, and tomato. These five are among the most widely traded and politically salient commodities.
2. **State coverage:** 28 states with WFP price monitoring and APMC index data. Excludes small Union Territories without consistent coverage.
3. **Time period:** January 2018 through December 2023 (72 months).
4. **Aggregation:** Market-level monthly observations are aggregated to state–commodity–month cells. Within each cell: mean retail price (INR/kg), standard deviation, and number of reporting markets.
5. **Price cleaning:** Observations with zero or negative prices dropped. Retail prices in Indian Rupees per kilogram.

A.3 APMC Stringency Index

The index is constructed for 30 states (28 with WFP data coverage) from pre-2020 legislation. Key values:

Table 6: APMC Stringency Index: Selected States

State	Cess (%)	Commodities Regulated	Private Mkt Restricted	Index
Punjab	8.5	46	Yes	0.783
Haryana	6.5	42	Yes	0.682
Uttar Pradesh	2.5	98	Yes	0.595
J & K	2.0	55	Yes	0.494
Maharashtra	1.5	166	No	0.371
Karnataka	1.5	100	No	0.251
Rajasthan	1.6	94	No	0.245
Gujarat	1.0	82	No	0.195
Tamil Nadu	1.0	48	No	0.134
Kerala	0.0	8	No	0.014
Bihar	0.0	0	No	0.000
Delhi	0.0	0	No	0.000

Notes: Index = $0.40 \times$ normalized cess + $0.30 \times$ normalized commodity count + $0.30 \times$ private market restriction. Full index for all 30 states available in replication code.

A.4 Variable Definitions

Table 7: Variable Definitions

Variable	Definition
<code>log_mean_price</code>	Log of mean retail price (INR/kg) within a state \times commodity \times month cell
<code>sd_price</code>	Standard deviation of prices across markets within a cell
<code>n_markets</code>	Number of WFP-monitored markets reporting prices within a cell
<code>apmc_stringency</code>	Composite APMC regulation intensity index [0, 1]
<code>apmc_cess_pct</code>	Total APMC transaction charges as % of trade value
<code>high_apmc</code>	Binary: APMC stringency above sample median
<code>on_phase</code>	= 1 if month in June 2020–January 2021
<code>off_phase</code>	= 1 if month is February 2021 or later
<code>blocked_farm_laws</code>	= 1 if state blocked the laws (Punjab, Rajasthan, Chhattisgarh)
<code>apmc_abolished</code>	= 1 if state abolished APMC pre-2020 (Bihar)

B. Identification Appendix

B.1 Pre-Trends

The event study in [Figure 1](#) shows monthly coefficients for months -24 through $+30$. The pre-treatment coefficients fluctuate around zero with no visible trend, supporting the parallel trends assumption. However, the absence of any post-treatment break also confirms the null result: there is no evidence that the parallel trends relationship changed at any point during or after the farm laws' enactment.

B.2 Power Analysis

The standard error on the ON-phase coefficient is 0.080. With 28 states and 6,862 monthly observations, the minimum detectable effect at 80% power is approximately $0.080 \times 2.8 = 0.22$ log points. The standard deviation of the APMC stringency index is 0.21, so the MDE for a one-SD change in treatment is approximately $0.22 \times 0.21 = 0.047$ log points, or about 4.7 percent. This is within the range of plausible effects (APMC cess rates up to 8.5%), suggesting the design has adequate power for the most dramatic states but may miss smaller effects concentrated in moderate-stringency states.

B.3 Why Randomization Inference Is Informative

The RI p -value of 0.52 means that 52% of random assignments of APMC stringency would produce an ON-phase coefficient at least as large as the one observed. Under standard inference, the p -value of 0.48 is a “fail to reject”; under RI, the result is that the observed coefficient is indistinguishable from noise. The RI procedure is particularly informative because it conditions on the actual data and treatment structure, avoiding distributional assumptions about error terms.

C. Robustness Appendix

C.1 Full Robustness Table

The five specifications in [Table 3](#) test sensitivity to sample composition. Key observations:

- Dropping protest-affected states (Punjab, Haryana) slightly increases the ON coefficient (0.122 vs. 0.058) but leaves it insignificant ($p = 0.38$).
- Dropping blocked states produces the smallest ON coefficient (0.031, $p = 0.76$).

- Excluding deregulated baseline states (Bihar, Kerala) flips the ON sign to negative (-0.021) but with a large standard error.
- The narrow bandwidth (2019–2022) gives a near-zero ON coefficient (-0.008 , p large).

The sign instability across specifications—positive in some, negative in others—is itself evidence of a null result. A true treatment effect would produce consistent-sign coefficients across reasonable perturbations.

C.2 Randomization Inference Details

1. Compute actual ON and OFF coefficients from the main specification.
2. For $b = 1, \dots, 1000$: permute APMC stringency across states; re-estimate; store coefficients.
3. RI p -value: fraction of $|\hat{\beta}_1^{(b)}| \geq |\hat{\beta}_1|$.

ON-phase RI p : 0.52. OFF-phase RI p : 0.31.

D. Heterogeneity Appendix

D.1 Commodity-Specific Results

- **Potato:** The only commodity with a marginally significant ON-phase coefficient (0.237, $p = 0.096$). However, the sign is positive (prices *rose* in high-APMC states during deregulation), contradicting the intermediation cost hypothesis. This likely reflects potato-specific supply shocks rather than the farm laws.
- **Rice and wheat:** Positive but insignificant ON-phase coefficients (0.007 and 0.110). These cereals are the most heavily regulated by APMCs and the most plausible candidates for a deregulation effect, yet the null obtains.
- **Onion and tomato:** Negative but insignificant ON-phase coefficients (-0.044 and -0.032). These volatile perishables are partially exempt from APMC in many states, so a smaller effect is expected.

No commodity shows the predicted pattern of $\hat{\beta}_1 < 0$ (significant) and $\hat{\beta}_2 \approx 0$.

E. Additional Figures

This appendix collects supplementary exhibits. The APMC stringency distribution (Figure 7) is reported in the main text.

F. Standardized Effect Sizes

Table 8: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	Classification
Log mean price (ON)	Table 2 Col. 1	0.058	0.214	0.542	0.023	Null
Log mean price (OFF)	Table 2 Col. 1	0.210	0.214	0.542	0.083	Small positive
Log price SD (ON) [†]	Table 2 Col. 4	0.369	0.214	1.142	0.069	Small positive
Log markets (ON)	Table 2 Col. 5	-0.123	0.214	0.519	-0.051	Small negative

Notes: Standardized effect sizes (SDE) facilitate cross-study comparison. The treatment (APMC stringency index) is continuous, so $SDE = \hat{\beta} \times SD(X)/SD(Y)$: the effect of a one-SD change in treatment intensity, measured in SDs of the outcome. $SD(X)$ and $SD(Y)$ are unconditional standard deviations from summary statistics.

Research question: Whether India’s 2020 farm laws—which deregulated agricultural trade outside the APMC mandi system—affected retail commodity prices, and whether effects reversed upon repeal.

Treatment: APMC stringency index (continuous, 0–1), measuring pre-existing state-level agricultural regulation through market fees, commodity coverage, and private market restrictions. $SD(X) = 0.214$. **Data:** WFP/VAM retail food prices, 28 Indian states, 5 commodities, 169 markets, January 2018–December 2023, aggregated to state \times commodity \times month ($N = 6,862$). **Method:** Continuous-treatment DiD with state \times commodity and commodity \times month FE, state-clustered SEs. **Key finding:** All ON-phase SDEs are in the null-to-small range. No coefficient is statistically significant. RI $p = 0.52$. [†]Log price SD (Column 4) uses a different outcome variable ($\log(SD + 1)$) than Columns 1–3 (log mean price); $SD(Y) = 1.142$ is the unconditional SD of this outcome, computed over the subsample with > 1 reporting market ($N = 4,324$). Classification: large negative (< -0.10), small negative (-0.10 to -0.05), null (-0.05 to 0.05), small positive (0.05 to 0.10), large positive (> 0.10).