# Who Gets the Tax Cut? Pass-Through Along the Supply Chain*

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**Abstract**

We study how VAT cuts are transmitted along the supply chain. Exploiting a large set of rate reductions in India, we show that pass-through is nearly complete upstream, while only 56 percent of the tax cut reaches final consumers. Contrary to standard market-power predictions, pass-through is higher among prominent brands and premium products, consistent with greater visibility and enforcement scrutiny. Incomplete retail transmission substantially reduces the consumer welfare gains from the tax cuts.

**JEL Codes**: H32, H22, H25, E31

**Keywords**: VAT, pass-through, tax collection, supply chain

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# 1 Introduction

The Value-Added Tax (VAT) is one of the most widely used indirect taxes around the world, and rate reductions—whether used as targeted stimulus, distributional policy, or broader tax rationalization—are routinely undertaken to lower consumer prices and expand consumption. The effectiveness of such reductions depends critically on the extent to which the statutory tax cut is passed through to the prices consumers actually pay. Recent studies show that pass-through of tax cuts to final consumers may be incomplete (Benzarti *et al.*, 2020; Politi & Mattos, 2011; Carbonnier, 2007). Yet, an important feature of the VAT has received comparatively less attention. VAT is a *multi-stage* tax: it is levied and credited at successive links of the supply chain, generating tax liabilities and incentives for firms well before a product reaches the final consumer. Much of the existing empirical evidence measures pass-through using final consumer prices, leaving less understood about where along the supply chain incomplete pass-through arises. Is incomplete transmission distributed throughout the supply chain, or is it concentrated at particular stages, consistent with stage-specific differences in enforcement, buyer attention, and market power? The answer matters both, for interpreting existing incidence estimates and for the design of tax policy for better enforcement.

A related question concerns the fiscal and welfare consequences of tax reductions. The incidence of consumption taxation depends on pass-through as well as the relative elasticities of supply and demand (Ramsey, 1927; Mirrlees & Adam, 2010; Diamond & Mirrlees, 1971; Kleven, 2004). However, empirical pass-through studies rarely observe product-level tax revenues together with prices and quantities across different stages of the supply chain, making it difficult to jointly quantify incidence, behavioral responses, and fiscal costs. In this paper, we address both questions by studying tax cuts across a broad range of products under India's Goods and Services Tax (GST), introduced in 2017.

We exploit rate reductions announced at the 28th meeting of India's Goods and Services Tax (GST) Council in July 2018, with the revised rates becoming effective on 27 July 2018.

The reform reduced statutory GST rates by an average of approximately ten percentage points across a broad set of goods. We combine three complementary datasets that, together, allow us to study tax pass-through at different stages of the supply chain: proprietary administrative E-Way Bill records on inter-firm shipments, firm-product data from the Annual Survey of Industries (ASI), and proprietary retail audit data.

Administrative E-Way Bill records provide monthly, state × HS-4 product data on wholesale shipment values, shipment counts, and tax revenues for April 2018–March 2020.[1] Our identification strategy exploits the fact that the rate reductions affected some HS-4 products but not others within the same broad HS-2 category. Importantly, we only keep the 13 treated HSN-4 codes where all the sub-categories of goods underwent a tax reduction. We then estimate difference-in-differences specifications comparing treated products to untreated products in the same broad product category, accounting for state-product, state-time, and broad product-time fixed effects. We complement the two-way fixed-effects estimator with the Synthetic Difference-in-Differences (SDID) estimator of Arkhangelsky *et al.* (2021), which reweights control units to match the pre-treatment trajectory of treated units. Results are stable across estimators, robust to dropping close demand substitutes from the control group, and to expanding the treated set to include later tax-cut waves. Second, we use the Annual Survey of Industries (ASI) which provides firm-product-level data on *direct* ex-factory prices, quantities, and revenues, allowing us to verify the E-Way Bill results.

Our first result is that wholesale pass-through is nearly complete. A ten-percentage-point statutory tax cut lowers value per E-Way Bill by roughly 11%, raises shipment counts by 27%, and lowers tax collections by about 18%. At the same time, we find no significant change in pre-tax value per E-Way Bill. Together, these results are consistent with near-complete pass-through of the statutory tax reduction to tax-inclusive wholesale prices, accompanied by a substantial expansion in transaction volume. The ASI data, which observe prices directly, corroborate this conclusion: ex-factory prices of treated products fall substantially following

[1] Under India's E-Way Bill rules, an E-Way Bill is required for the movement of goods when the value of a consignment exceeds INR 50,000, subject to specified exceptions.

the tax cut, with larger manufacturers exhibiting greater pass-through.

Next, using proprietary retail audit data that track final consumer prices, unit sales, and model-level attributes for three consumer-durable categories affected by the tax reduction—refrigerators, washing machines, and electric water heaters—we find substantially incomplete retail pass-through. Final consumer prices for these categories fall by only about 4.4%, corresponding to approximately 56% pass-through of the price reduction implied by the statutory tax cut. This is despite the upstream wholesale prices they face having fallen by the full amount. The gap between wholesale and retail pass-through is large and persists following the reform.

Two leading channels can plausibly rationalize this gap. An *enforcement and attention* channel attributes it to asymmetry in the ability of buyers at each stage to monitor and extract price concessions. At the wholesale stage, GST's invoice-linked input-tax-credit system creates strong incentives for B2B buyers and sellers to document transactions accurately, while business buyers are sophisticated and have greater incentives to monitor tax-inclusive prices and bargain over price changes. At the retail stage, final consumers do not participate in the input-tax-credit chain and may be less informed or attentive to statutory tax changes (Chetty *et al.*, 2009; Bernardino *et al.*, 2025).[2]

A *market-power* channel (Weyl & Fabinger, 2013) instead attributes incomplete retail pass-through to imperfect competition and the curvature of retail demand. These two channels make *opposing* predictions for the retail-level heterogeneity of pass-through. A simple market-power interpretation predicts that larger, more prominent brands with greater pricing power should pass through *less* of a cost reduction. The enforcement-and-attention channel predicts the opposite: larger, more visible brands may face greater scrutiny from consumers, media, and regulators and therefore pass through *more*.[3] The retail data show

[2]Salience and third-party enforcement have been shown to shape both consumer behavior and firm compliance in related settings; see Pomeranz (2015), Naritomi (2019), and Bernardino *et al.* (2025).

[3]During the period we study, India's National Anti-Profiteering Authority actively investigated consumer-durable firms that appeared to have retained the benefit of GST rate cuts, and the reductions we study received substantial media attention. We provide further institutional detail in Section 2.2.

that pass-through increases with brand size and product price tier: large brands and premium items transmit more of the tax cut to consumers than small brands and budget items. This pattern is more consistent with the enforcement-and-attention channel than with the simple market-power prediction.

Lastly, using the instrumental-variable approach of Zoutman *et al.* (2018) and Fajgelbaum *et al.* (2020), we estimate a wholesale demand elasticity of approximately $-2.57$ and an inverse supply elasticity that is statistically indistinguishable from zero, consistent with an approximately flat wholesale supply curve. The resulting expansion in the tax base partially offsets the mechanical revenue loss from the tax cut, but is not large enough for the reform to be self-financing. At the pre-reform tax rate of 28%, the marginal Laffer bound implies that demand elasticity would need to exceed 4.57 in absolute value for a marginal tax cut to be revenue neutral, well above our estimated wholesale elasticity.

We then combine our elasticity and retail pass-through estimates to evaluate the Marginal Value of Public Funds (MVPF). An optimistic benchmark that applies the wholesale elasticity to final consumers and assumes complete retail pass-through yields an MVPF of 2.28, meaning that each rupee of net fiscal cost would generate approximately 2.28 rupees of consumer surplus. In contrast, incorporating the observed retail pass-through rate of 56% and a plausible range of consumer demand elasticities yields a scenario-based MVPF below one. These calculations show that incomplete retail pass-through is quantitatively important: although the tax cut expands the tax base, a substantial share of the fiscal cost does not translate into consumer surplus. Instead, the share of the tax cut that is not passed on to consumers appears to be retained by retailers as higher margins.

Our paper makes four contributions to the literature on tax incidence and pass-through. First, to our knowledge, we provide the first evidence that separately identifies the wholesale and retail components of VAT pass-through within the same reform setting. A large body of work—Bachas *et al.* (2025), Benzarti & Carloni (2019), Kosonen (2015), Gaarder (2019), Fuest *et al.* (2021), Frey & Haucap (2023), Forteza *et al.* (2024), Bernardino *et al.* (2025)—estimates

pass-through primarily from retail or final-consumer data.[4] We show that incomplete pass-through to final consumers can arise predominantly at the retail stage even when upstream pass-through is essentially complete. This distinction has substantive implications for both the interpretation of incidence estimates and the design of tax policy.

Second, we provide evidence on the mechanisms underlying incomplete retail pass-through. The existing literature has emphasized market-structure frictions (Benzarti *et al.*, 2020; Weyl & Fabinger, 2013) and the salience of the tax instrument when tax-cuts are publicly discussed, e.g. during the post COVID-19 VAT reductions in Portugal online food markets (Bernardino *et al.*, 2025). We exploit the multi-stage structure of the GST and cross-brand heterogeneity in retail pass-through to distinguish the predictions of market power from those of enforcement and consumer attention. The finding that larger and more visible brands pass through more of the tax cut is more consistent with the latter channel.

Third, our estimates extend the evidence base on tax incidence beyond the relatively narrow sectoral settings that dominate much of the existing pass-through literature. Prior work has studied pass-through in French restaurants (Benzarti & Carloni, 2019), Finnish hairdressers (Kosonen, 2015; Benzarti *et al.*, 2020), Norwegian and Portuguese food markets (Gaarder, 2019; Bernardino *et al.*, 2025), menstrual hygiene products (Frey & Haucap, 2023), and pandemic-era VAT cuts in Germany, Spain, and Portugal (Fuest *et al.*, 2021; Forteza *et al.*, 2024; Bernardino *et al.*, 2025). We document near-complete wholesale pass-through across a broad cross-section of manufactured goods in a large developing economy.

Fourth, because we directly observe tax revenues at the product $\times$ state $\times$ month level—an unusually rich feature of the Indian administrative data—we can jointly study the fiscal cost of the reform and estimate demand and inverse supply elasticities using the approach of Zoutman *et al.* (2018). Our estimates of a wholesale demand elasticity of approximately

[4] Closest to our study is a recent paper by Bachas *et al.* (2025), which studies consumer-price pass-through using a supermarket scanner dataset following an earlier GST rate reduction in India and finds partial transmission of roughly 40–50% for B2C firms. We complement their paper by *decomposing* pass-through into a near-complete wholesale component and an incomplete retail component, using three separate data sources that span different stages of the supply chain and a national (rather than single-state) sample on the wholesale side.

$-2.57$ and an approximately flat inverse supply curve imply that expansion in the tax base offsets part, but not all, of the mechanical revenue loss from the tax cut, and that further retail-level pass-through would generate additional revenue recovery. These wholesale-side elasticities are larger than the consumer-side quantity elasticity of $-0.30$ estimated by Bachas *et al.* (2025). These parameters are directly relevant for the welfare and revenue evaluation of large rate rationalizations—including India's GST reforms of September 2025, which have reduced rates across more than a thousand products.[5]

# 2 Background and Data

## 2.1 Tax Changes

India adopted the Goods and Services Tax (GST) system on 1st July 2017. The GST system represented a major reform of India's indirect tax system, consolidating several central and state-level taxes, including the Value Added Tax (VAT), excise duty, service tax – all into a unified tax regime. Under this system, goods and services are taxed according to a common rate structure that applies across all states and Union Territories. These rates are deliberated and determined by the GST Council, which comprises the Union Finance Minister, the Minister of State for Finance, and the Finance Ministers of all states and Union Territories in India.

We construct a product-level dataset on GST rate changes in India for the period April 2018–March 2020. During this period, there were 13 GST Council meetings, of which four resulted in notified GST rate revisions for goods. We identify products that experienced tax rate changes using official press releases issued by the Central Board of Indirect Taxes and Customs (CBIC) and the GST Council. Across these four meetings, GST rates were revised for 70 goods.

[5]Relative to our sample period, these later rationalizations are both broader in product scope and accompanied by an explicit policy emphasis on retail transmission. The estimates we report are therefore directly relevant to predicting the distributional and fiscal consequences of that reform.

For our main analysis, we apply several restrictions to obtain a clean treatment group. We exclude products that experienced multiple tax revisions during the sample period, products that are not observed in the E-Way Bills data described below, products with incomplete monthly coverage, products that experienced tax increases, and cases where the tax revision applied only to a subset of products within a given 4-digit Harmonized System of Nomenclature (HSN) category. Appendix A provides additional details on the construction of the tax-change dataset.[6]

These restrictions yield 15 4-digit HSN product categories that experienced GST rate cuts covering the entire product category during the sample period. For the main analysis, we further restrict attention to the 13 4-digit HSN products that experienced tax rate reductions following the 28th GST Council Meeting, held on 21 July 2018, with revised rates effective from 27 July 2018. We focus on this meeting for two reasons. First, it provides the longest post-reform window within our sample, allowing us to study outcome responses over a relatively extended period after the tax cut. Second, concentrating on a single reform episode avoids the econometric complications associated with staggered treatment timing while resulting in only a small reduction in the number of treated products. In robustness checks, we also consider broader treatment definitions, including the full set of 15 4-digit HSN products that experienced category-wide tax cuts and a set of 46 4-digit HSN products for which at least a subset of products within the HSN-4 category experienced a tax decline.

## 2.2 The Enforcement Architecture of the GST

A distinctive feature of the GST is its self-enforcing invoice-matching mechanism. Under the input tax credit (ITC) system, every registered buyer can claim a credit for taxes paid on inputs against their output tax liability. Critically, this credit is available *only* if the seller has reported the transaction in their own GST return (GSTR-1). This creates a built-in

[6]Seven products experienced multiple tax revisions during this period. E-Way Bills data are unavailable for four products, while six products are excluded because data are not available for all 24 months. Of the remaining 53 products, seven experienced tax increases and 31 had revisions that applied only to a subset of products within a given 4-digit HSN product category.

incentive for buyers to monitor sellers' compliance since any underreporting or overpricing by the seller directly reduces the buyer's tax credit. The E-Way Bill system reinforces this by requiring digital documentation for all shipments above INR 50,000, creating a verifiable paper trail for goods in transit.

This enforcement architecture has two important implications for pass-through. First, it operates with full force at the *wholesale* stage of the supply chain, where both transacting parties—manufacturer and retailer—are GST-registered businesses that file returns, claim input tax credits, and face audit risk. At this stage, the buyer (retailer) has a direct financial incentive to ensure that the seller's (manufacturer's) invoice reflects the correct post-reform tax rate. Any failure to reduce the invoice price by the tax cut amount would inflate the buyer's procurement cost without a corresponding increase in their claimable ITC.

Second, this enforcement mechanism *breaks down* at the retail stage. Final consumers are not GST-registered, do not file returns, and cannot claim input tax credits. They therefore lack the institutional tool to verify whether the retailer has reduced prices commensurately with the tax cut. The only nominal anchor available to consumers is the Maximum Retail Price (MRP) printed on product packaging—a mechanism that is both costly to update and imperfectly observed by consumers. This asymmetry in enforcement intensity across the two stages of the supply chain provides a first-order institutional explanation for why wholesale pass-through may be complete while retail pass-through is not.[7]

## 2.3 Data

### 2.3.1 E-Way Bills Data

We use data on Electronic-Way (E-Way) Bills collected by the Goods and Services Tax Network (GSTN) in India during April 2018 to March 2020.[8] The GSTN implemented

[7]This mechanism is closely related to the broader literature on third-party reporting and tax compliance. Kleven *et al.* (2011) show that tax evasion is near-zero in settings with third-party information trails and substantial in settings without them. The ITC chain operates analogously: it creates a verifiable trail for B2B transactions that is absent at the retail stage.

[8]We exclude data after March 2020 due to economic disruption caused by Covid-19.

this system in April 2018. An E-Way Bill is a document carried by the party in-charge of conveyance of consignment of goods when the value of the consignment exceeds Rs 50,000 (around USD 550), irrespective of the mode of transport. Given the small threshold value, this aids in tracking goods movement across the country and serves as an important tool for monitoring compliance and limiting tax evasion. A critical feature of the E-Way Bills data is that it largely records *inter-firm* (B2B) transactions i.e., goods moving from manufacturers and wholesalers to retailers or other downstream businesses. The data therefore mainly capture transactions at the wholesale stage of the supply chain, rather than final sales to consumers.

The E-Way Bills (EWB) data provide product-level sales and tax information at the 4-digit Harmonized System of Nomenclature (HSN) level for every state and month. It records two types of sales – inter-state sales and intra-state sales – which correspond to sales of a product from a given state to buyers outside the state and within the same state, respectively. A product is reported in the dataset if it falls within the top 1,000 products for a given state and sales type. We construct domestic sales for each product by summing inter-state and intra-state sales at the state $\times$ HS-4 product $\times$ month level. Our analysis is based on a balanced panel of state $\times$ product combinations for which data on all key product outcomes - domestic sales, number of E-Way Bills, and tax revenues — are available for every month during the 24-month period from April 2018 to March 2020.

We use information on five key outcome variables for these products. First, we use Total Assessable Value (TAV), which captures the gross value of sales for a given product. Second, we use the number of E-Way Bills (EWBs) generated, which serves as a proxy for transaction quantity. Third, we use tax revenues collected on each product by the central and state governments. We compute total tax revenues by summing GST and cess collections accruing to the central and state governments.

Using these three variables, we construct two additional outcome measures: Value per EWB and Pre-tax Value per EWB. Since TAV is inclusive of taxes, dividing TAV by the

number of EWBs gives a tax-inclusive unit value measure:

$$\frac{(1+t)pq}{q} = \text{Value per EWB}$$

where $p$ denotes the pre-tax price, $q$ denotes quantity, and $t$ denotes the tax rate applicable to a given commodity transaction. Thus, Value per EWB can be interpreted as a tax-inclusive price measure.

We then construct a corresponding pre-tax unit value measure by subtracting tax revenues from TAV to obtain net sales, and then dividing net sales by the number of EWBs. This gives:

$$\frac{((1+t)pq) - tpq}{q} = \frac{pq}{q} = \text{Pre-tax Value per EWB}$$

This measure removes the tax component from gross sales and therefore provides a proxy for the pre-tax value per transaction.

We report summary statistics for the balanced panel of state × product combinations used in our main analysis, for which information on total sales, the number of E-Way Bills, and tax revenues is available in each of the 24 months from April 2018 to March 2020 (Appendix Table A.2).[9] For each state × product × month observation, we construct total domestic sales as the sum of intra-state and inter-state sales originating from that state.

Our sample contains 986 HS-4 products, of which 13 belong to the treated group and 973 to the control group. Average monthly sales are approximately INR 538 million for treated products and INR 545 million for control products. Average monthly tax revenues are approximately INR 105 million per product in both groups. Treated products generate an average of approximately 4,950 E-Way Bills per month, compared with 2,245 for products in the control category.

[9]Our main results are based on this balanced sample. The results are robust to including products observed for fewer than 24 months, as well as to restricting the sample to state × product combinations for which both intra-state and inter-state sales are reported in every month of the sample period.

### 2.3.2 ASI Data

We also use data from the Annual Survey of Industries (ASI) to examine the impact of GST rate cuts on manufacturing establishments. A key advantage of the ASI is that it provides product-level information on sales, quantities, and values at the manufacturing establishment level, allowing us to study heterogeneity in firms' responses to the tax reductions.

The ASI is the principal source of industrial statistics for India's registered manufacturing sector and is conducted annually by the National Statistical Office under the Ministry of Statistics and Programme Implementation (MoSPI). The primary unit of observation for manufacturing industries is the factory or establishment. The survey provides detailed information on establishment-level production, product sales and quantities, employment, capital, machinery, intermediate inputs, and other characteristics. The ASI sampling design consists of a census sector and a sample sector. Under the census sector, larger establishments and establishments satisfying certain state- and industry-specific criteria are surveyed every year, while the remaining establishments are selected using a stratified sampling design.[10]

These detailed establishment characteristics, combined with product-level information on values and quantities, allow us to examine both direct price responses and heterogeneity in firms' responses to GST rate reductions. Since the ASI is available only at an annual frequency, we use data from 2017–2020, covering the period following the introduction of GST in July 2017. The tax structure changed substantially with the transition from the earlier VAT-based system to GST, as discussed above. For the purposes of this paper, we use the terms *firm* and *establishment* interchangeably. This distinction is unlikely to be quantitatively important in our setting, since multi-plant establishments account for only about 5% of manufacturing plants with annual sales of at least USD 30 million in India (Chakrabati & Tomar, 2022).

[10]During our sample period, the census sector included all industrial units in seven less-industrially developed States/Union Territories, as well as establishments exceeding state-specific employment thresholds and certain other categories of units. Among the remaining establishments, samples were drawn within strata defined by state, district, sector, and industry.

We have data on 17,582 firms, of which 173 firms sold products which were subject to a rate change (Table A.3). The products in the ASI dataset are at the 4-digit Central Product Classification (CPC) level, where we have information on around 563 CPC-4 products. Of all products available, 5 underwent a rate cut. The average annual product sales of firms in the treated group is around Rs 2,300 million while it is around Rs 1,430 million for those in the control group. Additionally, we also use the ASI data to examine whether small establishments differ from large establishments with respect to the extent of pass-through. We define small establishments as those having below median gross value of sales for a given product and large as those having above median gross value of sales for a product during the pre-GST period, i.e., in 2017.

# 3 Impact on the Wholesale Market

In this section, we use administrative E-Way Bills data, which largely captures business-to-business (B2B) transactions between manufacturers/wholesalers and downstream retailers to estimate the pass-through of tax rate decreases on wholesale prices.

## 3.1 Empirical Strategy

We examine the impact of tax rate changes on a set of outcome variables using a two-way fixed effects specification.

$$ln(y_{jsmt}) = \beta\ Tax\ Cut_j \times Post_{mt} + \delta_{js} + \delta_{smt} + \delta_{HS2-mt} + \varepsilon_{jsmt} \tag{1}$$

where $y_{jsmt}$ is the outcome variable for product $j$ in state $s$ in month $m$ and year $t$. Our main outcomes of interest are: value per E-Way Bill (price), count of E-Way bills (quantity), sales, taxes, and pre-tax value per E-Way bill. $Tax\ Cut_j$ is an indicator variable which equals one

for products that undergo tax rate reduction. $Post_{mt}$ equals one from August 2018 onwards.[11]

We account for product × state level unobserved heterogeneity through $\delta_{js}$ fixed effects. We also control for fixed-effects at HSN 2-digit product × month × year level ($\delta_{HS2-mt}$). These ensure that the treated and untreated HS-4 products are compared with each other within the same broad HS-2 category over time. This fixed effect also absorbs any seasonality in outcomes at HSN 2-digit product level. $\delta_{smt}$ fixed-effects filter out any average state × month × year variation since regulatory changes at the state level were rampant during this time (Chakraborty & Mahajan, 2026). Standard errors are clustered at the HS-4 product level.

$\beta$ gives the impact of a tax rate reduction for a product on the outcome of interest. For example, when the outcome variable is sales, $\beta$ captures the percentage change in sales for product $j$ after its tax rate has been reduced compared to what it would have been if the tax rate had remained unchanged. The counterfactual for this estimation is obtained from the untreated products in the same HS-2 category as the treated product. As a robustness check, we drop the close substitutes of the treated products so that any re-allocation of expenditures across control vs treated products does not contaminate our estimates.

Our identification strategy rests on the assumption that the timing of tax rate changes was exogenous. These changes were determined in GST Council meetings, the timing of which is set by the Council. The specific goods that may be considered for a rate change are not publicly discussed beforehand, and the meeting agenda is typically released only a few days or weeks in advance. For example, the press note for the meeting held on 21 July was released on 28 June 2018.[12] Some items that were ultimately subject to rate reductions were listed in the agenda note as matters for discussion. These recommendations are made by a fitment committee, which primarily relies on two considerations: correcting an inverted duty structure and ensuring similar tax treatment for similar products. However, whether these

[11] Since the rate reduction was only applicable from 27 July 2018, for all practical purposes it only became effective from August 2018.

[12] See: Agenda Notes for Meetings.

recommendations are ultimately accepted by the GST Council remains uncertain.

We also estimate dynamic treatment effects of the tax rate cut and check for the identifying assumption of similar pre-trends directly using the below event study specification.

$$ln(y_{jsmt}) = \sum_{\tau=-K,\tau\neq-1}^{\tau=L} \beta_\tau Tax\ Cut_j^\tau + \delta_{js} + \delta_{smt} + \delta_{HS2-mt} + \varepsilon_{jsmt} \quad (2)$$

where the main variable of interest, $Tax\ Cut_j^\tau$ is an indicator variable that takes a value of one for products that undergo tax rate reduction, $\tau$ months from the change, and zero otherwise. We create bins for the endpoints of the event window based on standard event-study applications (Schmidheiny & Siegloch, 2019). We do this at event dates of -3 and 6 and normalize coefficients to event time -1. Here, $\tau$ denotes the months relative to the month of change in tax rate, e.g., $\tau = -1$ for the month before the treatment, and $mt$ is the actual calendar month-year. We use the time-period prior to treatment as the baseline to compare effects before and after the tax rate cut. Thus, $\beta_\tau$ measures the average treatment effect in the outcome variables $\tau$ periods from the treatment.

## 3.2 Impact on Wholesale Prices and Tax Collection

We estimate equation 1 and report the coefficient of interest $\beta$ in Table 1 for each outcome of interest—value per EWB (column 1), number of e-way bills (column 2), sales value (column 3) and total tax revenues (column 4). We find a significant decrease of 11 percent in value per E-Way Bill in response to tax rate reductions. The count of E-Way Bills increases by 28 percent. The fall in value per E-Way bill is lower than the increase in its count resulting in a 17 percent significant increase in sales (column 3). We find a significant reduction in tax revenues collected by 17 percent (column 4).

These results demonstrate near-complete wholesale pass-through: an average tax reduction of 10 percentage points across 13 product categories is followed by a price reduction of approximately the same magnitude in value per EWB (our proxy for wholesale price). Along

with the reduction in wholesale prices, we observe a substantial increase in the quantity of goods shipped downstream, reflected by the positive effect on count of E-way bills. Since prices and quantities move in opposite directions, the impact on aggregate sales is lower than the impact on quantity alone. Importantly, the reduced tax rate is not offset by the increase in tax revenues from higher sales volume, resulting in a significant drop in tax revenues.

To verify that the decline in value per E-way bill is attributable to the tax cuts (and not to changes in pre-tax pricing by manufacturers), we estimate equation 1 with pre-tax value per E-Way bill as the outcome variable. The results in column (5) show no significant change in pre-tax value per E-way bill, confirming that the observed price decline is driven entirely by the tax reform. This also establishes that manufacturers did not alter their pre-tax markups in response to the rate reduction, consistent with a competitive wholesale market where pre-tax prices are determined by marginal cost.

We also estimate the event study specification in equation 2 and plot the coefficients $\beta_\tau$ in Figure 1. Panel (a) shows a decline in the value per E-way bill emerging after approximately 3 months of the tax cut, which persists in the subsequent months. Panel (b) shows a significant positive effect of the tax reduction on number of E-way bills, with a steady and significant increase in quantity from the month of tax cut implementation. Panel (c) shows the aggregate sales response, which follows the pattern of quantity. The pattern for sales change is consistent with changes in tax revenues (panel d)—the months when sales increase, the decrease in tax revenues is attenuated. We observe greater stability in outcomes after 5–6 months of the tax rate reduction with a complete pass-through, rise in quantity by more than 20 percent, rise in sales by 15-20 percent and decline in tax revenues by 15-20 percent.

Notably, the temporal dynamics reveal an important pattern: wholesale quantities (E-Way Bill counts) respond *immediately* to the tax cut, while wholesale prices (value per EWB) adjust with a lag of approximately 3 months. This pattern is consistent with nominal price stickiness at the wholesale level which can be driven, for instance, by pre-existing contracts or the need to reprint Maximum Retail Price (MRP) labels on packaging followed by full

price adjustment once new production runs incorporate the revised tax rates. Crucially, even during the initial period when wholesale prices are sticky, the quantity response indicates that downstream retailers are already increasing procurement in anticipation of or response to the tax reduction.

We further report the elasticity estimates obtained by using the actual percentage point reduction in tax rate for a given product (instead of an indicator variable for tax reduction) in panel B of Table 1. We use the same specification as in equation 1, but our $Tax\ Cut$ variable is now defined as a continuous variable.[13] The coefficient estimates now denote the elasticity of outcomes with respect to the tax rate reduction. We find a negative price elasticity of approximately 1.4 with respect to the tax rate reduction. Similarly, we find a positive elasticity of 3.68 with quantity, 2.27 with total sales and negative 2.25 with tax revenues. These results are consistent in significance with the binary treatment results discussed above.

The above sample includes only treated products for which all sub-categories within the HS-4 code underwent a tax rate cut. This restriction is important for measuring true pass-through because including partially treated HS-4 categories may mechanically attenuate estimated price declines. In such cases, smaller estimated price reductions may reflect measurement error from mis-classifying an entire HS-4 category as treated, rather than incomplete pass-through. When we expand the treated sample to include HS-4 categories in which only a subset of underlying products underwent a tax reduction, the estimated effects are smaller in magnitude, as shown in Appendix Table C.3. This pattern is consistent with attenuation arising from measurement error in treatment assignment.

## 3.3 Robustness

We conduct several robustness checks on our main wholesale pass-through estimates.

[13]The rax rate change is given by:

$$\Delta Tax\ Cut = \frac{tax\ rate_{pre} - tax\ rate_{post}}{(1 + tax\ rate_{pre})} \tag{3}$$

**Synthetic Difference-in-Differences:** A potential concern with the standard two-way fixed effects estimator is that it may produce biased estimates when treatment effects are heterogeneous across units or over time (Roth *et al.*, 2022). To address this, we implement the Synthetic Difference-in-Differences (SDID) estimator proposed by Arkhangelsky *et al.* (2021). The SDID estimator reweights control units to better match the pre-treatment trajectory of treated units, providing a more credible counterfactual. Table C.1 reports the SDID estimates. We find that the value per EWB falls by 9 percent ($p < 0.01$), the count of E-Way Bills rises by 22 percent ($p < 0.01$), sales increase by 14 percent ($p < 0.01$), and tax revenues decline by 18 percent ($p < 0.01$). Reassuringly, the pre-tax value per EWB shows no significant change. The event study plots from the SDID estimation are reported in Figure 2 and confirm the absence of pre-trends and the sharp post-reform response observed in the TWFE estimates. These results also confirm almost a complete pass-through of tax rate reductions to wholesale prices.

**Excluding close substitutes:** A potential concern is that the tax rate reduction may induce consumers to substitute away from untreated close substitutes and toward treated products, thereby contaminating the control group. If such demand substitution is empirically relevant, the estimated effects on treated products would partly reflect a relative decline in outcomes for close substitutes rather than a genuine response to the tax change. To address this, we re-estimate Equation 1 after dropping 22 HS-4 products that are close demand substitutes to the 13 treated HS-4 categories. Table C.2 reports the results. The point estimates remain similar to our baseline findings. The value per E-Way Bill declines by 11 percent, the number of E-Way Bills increases by 28 percent, and aggregate sales rise by 17 percent. Tax revenues fall by 18 percent, consistent with the tax rate reduction not being fully offset by the expansion in the tax base. The stability of the estimates upon excluding close substitutes suggests that demand substitution across product categories within the control group does not bias our baseline results.

**Including 2019 tax rate cuts:** Our main estimates use 13 products from the 28th GST Council Meeting to avoid the pitfalls associated with staggered treatment (Goodman-Bacon, 2021). We now expand the treated sample to include 2 additional products from the 31st GST Council Meeting (effective January 2019), giving us 15 clean tax cuts with staggered treatment timing. The results in Appendix Table C.4 are similar to our baseline SDID results after accounting for the staggered treatment, confirming that the near-complete wholesale pass-through is not driven by the particular set of products from a single meeting.

**Measurement of prices:** A potential concern is that the E-Way Bills data do not report unit prices directly. We therefore use value per E-Way Bill as a proxy for wholesale prices. This measure captures true price changes if the average quantity shipped per E-Way Bill does not change differentially for treated products after the tax cut. However, if firms respond to higher demand after the tax reduction by shipping larger quantities per E-Way Bill, our proxy would overstate the post-reform price level for treated products. This would attenuate the estimated price decline and lead us to understate the extent of pass-through. The fact that we nevertheless estimate price declines of about 9–11 percent, close to the average tax rate reduction of roughly 10 percentage points, suggests near-complete pass-through. We further validate this interpretation using Annual Survey of Industries data, which contain direct price information, in Section 3.3.1 below.

### 3.3.1 Manufacturer-Level Evidence of Price Pass-through: ASI

We use the Annual Survey of Industries (ASI) data from 2017–2019 which has firm-level information on prices, quantities, and sales at the 4-digit Central Product Classification (CPC) product level. While ASI data is only available annually, limiting our ability to examine pre-trends around the GST rate cuts, since India followed a VAT system prior to 2017, the data provide two important advantages. First, it contains *direct* information on prices, allowing us to verify the pass-through estimates from the E-Way Bills data. Second,

it allows us to examine whether the wholesale pass-through varies by firm size, which speaks to the role of production capacity and market power at the manufacturing stage. This is also important because the E-way Bills data mostly capture large producers, given the minimum value criteria for generating it and its limit to the top 1000 products in a state.

We examine the impact of tax rate change on firm-level outcome variables in the ASI using the following specification:

$$ln(y)_{ijst} = \beta\ Tax\ Cut_j \times Post_t + \delta_{ij} + \delta_{CPC2-st} + \varepsilon_{ijst} \tag{4}$$

where $y$ is outcome for firm $i$, producing product $j$, in state $s$ in year $t$. We consider three outcomes: gross value of output, quantity sold, and gross price, where gross price is constructed by dividing gross value by quantity sold.[14] $Tax\ Cut_j$ equals one for all products that underwent a tax cut. We define products at the 4-digit Central Product Classification (CPC) level.[15] As in the E-Way Bills analysis, we restrict the treated group to clean tax-cut products, defined as products for which all underlying product categories within the relevant code experienced a GST rate reduction.[16] $Post_t$ equals one for the years 2018 and 2019. We control for unobserved variation at the firm-product level through $\delta_{ij}$ fixed-effects. We also include state varying time fixed-effects at the CPC 2-digit level $\delta_{CPC2-st}$ to compare treated and non-treated CPC 4-digit products within a given CPC 2-digit category in a state. This also absorbs any average change in outcomes at CPC 2-digit product level within a state. Standard errors are clustered at the CPC-4 product level.

To examine whether pass-through varies by firm size, we augment our baseline specification by interacting the treatment indicator with a dummy for above-median firms, defined as

[14] The ASI data do not provide information on taxes.

[15] ASI reports inputs and outputs using the National Product Classification for Manufacturing Sector (NPCMS). We first concord these NPCMS codes to the Central Product Classification (CPC) since CPC is more closely aligned with the Harmonized System of Nomenclature (HSN) classification used under GST and because concordances between NPCMS and CPC are readily available. For setting tax rates and classifying goods and services, India's GST system uses the HSN classification.

[16] Table A.3 provides additional details on the products which underwent a tax change and are observed in the ASI data. In total, 5 products from our main treated sample are available in the ASI data.

those whose total sales for a given product in the pre-reform year (2017) exceed the median. Specifically, we estimate:

$$ln(y)_{ijst} = \gamma_1 \; Tax \; Cut_j \times Post_t \times \mathbf{1}[FirmSize]_{ij} + \gamma_2 \; Tax \; Cut_j \times Post_t + \gamma_3 \; \mathbf{1}[FirmSize]_{ij} \times Post_t + \delta_{ij} + \delta_{CPC2-st} + \varepsilon_{ijst}. \tag{5}$$

where $\mathbf{1}[FirmSize]_{ij}$ equals one for firm-product pairs with above-median total sales in the pre-tax cut year, 2017. The coefficients $\gamma_1$ and $\gamma_2$ are the main coefficients of interest. The coefficient $\gamma_2$ captures the impact of the tax rate reduction on firm-level outcomes for below-median firms in that product category. The coefficient $\gamma_1$ captures the differential impact of the tax rate reduction on above-median firms relative to below-median firms. Therefore, the total effect for above-median firms is given by $\gamma_1 + \gamma_2$.

We report the estimates for equation 4 in columns 1-3 of Table 3 on three outcomes of interest: prices, quantity and firm sales. On average, we find a decrease in prices by 8% but the effect is imprecise. There is no significant effect on quantity and sales marginally decrease by 3%. Heterogeneity estimates in columns 4–6 reveal an important pattern: the pass-through is significantly concentrated among larger manufacturers. Prices charged by bigger firms differentially fall by around 11% with the overall fall at 13% ($\gamma_1 + \gamma_2$).[17] Along with a larger price reduction, these firms also witness a significant increase in quantities sold by almost 12%. We also find a significant difference in the total sales between small and large firms after tax rate reductions. While larger firms increase their sales by 8 percent, the smaller firms witness a fall in their sales revenue by 14%. The elasticity results in Table C.5 are consistent with the binary treatment results.

The ASI results corroborate the E-Way Bills evidence that, at the manufacturer/wholesale stage, tax rate reductions are substantively passed through to prices. Importantly, the ASI data records firm-level selling prices, which are wholesale or ex-factory prices paid by downstream retailers, and not final consumer prices. The pass-through documented here

[17] The average tax rate reduction for the 5 treated HSN-4 products in the ASI data is 10 percent.

therefore captures the first stage of the supply chain: from manufacturers to retailers.

# 4 Retail Price Pass-through

The preceding sections establish that wholesale pass-through of the tax rate reduction is nearly complete: manufacturers lower prices to retailers by approximately the full tax cut amount, and retailers respond by increasing procurement substantially. We now ask whether these wholesale price reductions reach final consumers. We use proprietary retail audit data from a leading market research firm, covering three consumer durable categories—refrigerators, washing machines, and electric water heaters—that underwent a 10-percentage-point GST rate reduction (from 28% to 18%) effective July 2018.

## 4.1 Data and Empirical Strategy

The retail data record item×city×channel level observations for different possible price classes at a monthly frequency from January 2017 to December 2019. An item is a specific brand-model-channel combination, city refers to a metropolitan market (Delhi or Mumbai), and channel denotes the retail outlet type—Multi-Brand Outlets (MBO) or Exclusive Brand Outlets (EBO). Price class refers to the range of price bands used by the aggregator who provides the data. If the same item for the same city-channel combination changes prices in a given month such that it moves to a different price band, then different observations will be recorded. For each observation, we observe sales units, sales value (INR), and unit price. We aggregate the data to the item×city×channel×month level using sales-weighted average prices.

We first estimate an event study specification to visualize the dynamic path of retail prices around the reform:

$$ln(p_{it}) = \sum_{\tau \neq \text{June 2018}} \beta_\tau \mathbf{1}[t = \tau] + \delta_i + \gamma_c + \varepsilon_{it} \tag{6}$$

where $p_{it}$ is the retail price of item $i$ in month $t$, $\delta_i$ are item fixed effects, and $\gamma_c$ are city fixed effects with June 2018, the last month before the rate cut took effect, serving as the base month. We use data only in 2018 as macro trends can drive long term behavior in prices. Since we observe the price pass-through by 5 months in the E-Way bills data, we should ideally see the price reduction in 2018. Heteroscedasticity-robust standard errors are reported throughout. To summarize the average post-reform price change, we estimate:

$$ln(p_{it}) = \beta \times \text{Post}_t + \delta_i + \gamma_c + \varepsilon_{it} \tag{7}$$

where $\text{Post}_t$ equals one from August 2018 onward.

## 4.2 Results

Figure 3 plots the event study coefficients from Equation (6), pooled across all three product categories. Retail prices are flat and precisely estimated at zero from February through June 2018 ($\tau = -4$ to $\tau = -1$), confirming the absence of pre-trends. Beginning in August 2018, prices decline sharply by approximately 1%. The decline deepens progressively, reaching 5–6% by October–November 2018, and stabilizes at this level through December.

Column (1) of Table 4 shows a significant average retail price decline of approximately 4.4% ($p < 0.001$). Under full pass-through with perfectly elastic supply, the prediction for the retail price change for a tax cut from 28% to 18% is $(1.18/1.28) - 1 \approx -7.8\%$. Our estimate therefore implies a pass-through rate of approximately 56% at the retail level.

When juxtaposed with the near-complete wholesale pass-through documented in Section 3, a striking pattern emerges: the tax reduction is almost fully transmitted from manufacturers to retailers (Stage 1), but only partially transmitted from retailers to final consumers (Stage 2). The approximately 44 percentage point gap between wholesale and retail pass-through represents the portion of the tax cut absorbed by the retail supply chain through higher retail margins.

**Comparison with wholesale data:** One concern is that the wholesale and retail datasets cover different products and use different empirical strategies. Hence, the divergence in findings could reflect differences in data and estimation rather than differential price pass-through. To assess this, we restrict the E-way bills data to the same consumer durable products covered in the retail data—refrigerators, washing machines, and water heaters—and replicate the retail estimation strategy by conducting a pre- versus post-period comparison using 2018 data. Table C.6 shows that value per EWB declines by approximately 7% ($p < 0.01$), with no significant change in pre-tax value per EWB, consistent with near-complete wholesale pass-through for these same products. So, for the same set of consumer durables, the decline in value per EWB is larger than the 4.4% decline observed in retail prices. This suggests that the lower estimated retail pass-through rate of about 56% is not simply an artifact of differences in product coverage across the two datasets.

# 5 Why Pass-through Differs: Wholesale vs. Retail?

The preceding sections establish that wholesale pass-through of tax rate reductions is nearly complete (approximately 100%), while retail pass-through is substantially lower (approximately 56%). In this section, we develop a framework identifying the mechanisms behind this gap and use the heterogeneity in our estimates to empirically evaluate their relevance. We organize the discussion around two broad channels—*enforcement and buyer attention*, and *market structure*—each of which operates differentially across the wholesale and retail stages of the supply chain.[18]

[18] A third channel, nominal price stickiness driven by MRP-relabeling costs, can be ruled out as a first-order explanation of the *level* of the gap. A menu-cost framework predicts convergence to full pass-through as packaging inventory turns over, yet our retail event study (Figure 3) shows pass-through stabilizing at approximately 56% through the end of 2018. Hence, menu costs cannot account for the persistent steady-state wholesale–retail gap.

## 5.1 Enforcement and Buyer Attention

The GST's invoice-matching mechanism creates asymmetric enforcement intensity across the two stages of the supply chain (Section 2.2). At the wholesale stage, the buyer (retailer) monitors the seller's (manufacturer's) pricing because the buyer's input tax credit depends on the seller's reported invoice. When the tax rate falls, the buyer has a direct financial incentive to demand a commensurate reduction in the tax-inclusive price: failing to do so would inflate the buyer's procurement cost without a corresponding increase in their claimable ITC. This "third-party enforcement" through the ITC chain disciplines wholesale pricing and pushes pass-through toward completeness.

Even absent a formal enforcement institution, pass-through depends on whether buyers notice and act on tax changes (Chetty *et al.*, 2009). B2B buyers are, by construction, highly attentive to tax rates: they file GST returns, track input tax credits, and transact in large volumes where small price misalignments accumulate to material costs. Final consumers, by contrast, may be inattentive to tax changes and do not adjust their reservation prices downward by the full tax cut. Retailers, knowing this, have limited competitive incentive to pass through the full saving. Thus, the ITC chain does not work in isolation. It works because B2B buyers are attentive to the prices on their invoices and know how to use the institution to their financial advantage. The wholesale–retail asymmetry arises because B2B markets feature *both*, a formal enforcement mechanism and attentive buyers, while B2C markets feature *neither*.

If enforcement was an important channel then, within the wholesale stage, pass-through should not vary by whether the good is intermediate or final, since both types of transactions are B2B and subject to the same ITC mechanism and buyer attentiveness, even though both can have different market structures. Firms and brands that are more *visible*—to the tax administration, to the media, and to consumers—should exhibit higher pass-through. Visibility creates informal enforcement pressure that partially substitutes for the missing ITC chain at the retail stage.

## 5.2 Market Structure

Even absent enforcement or attention differences, the standard theory of tax incidence predicts differential pass-through across market structures (Weyl & Fabinger, 2013). Under competitive supply, the full incidence of a tax change falls on the demand side—in this case, the retailer—and pass-through approaches 100% regardless of demand curvature. At the retail stage, the situation is structurally different. Retailers enjoy local market power arising from spatial differentiation and limited consumer search. With imperfect retail competition, the pass-through rate depends on the curvature of consumer demand (Weyl & Fabinger, 2013). Our estimated retail pass-through of approximately 56% is therefore consistent with a retail market characterized by near-linear demand and local market power.

The market-structure channel generates predictions for heterogeneity in retail pass-through that are distinct from those of the enforcement/attention channel. Under standard oligopoly models with log-concave demand (the empirically dominant case), firms with greater market power, proxied by higher markups, larger market shares, or more concentrated segments face flatter residual demand curves and pass through *less* of a cost change to final prices. This generates two testable predictions. First, brands with larger market shares—who exercise greater market power—should pass through *less* than smaller brands. Second, premium items, which typically command higher markups than budget items, should pass through *less* than budget items (under the standard log-concave demand assumption). Crucially, these predictions run *opposite* to the enforcement/attention channel's prediction, under which large and visible brands should pass through *more*. The retail heterogeneity estimates therefore provide a sharp test that can distinguish the two channels.

## 5.3 Enforcement vs. Market Structure

We now consolidate the heterogeneity estimates to evaluate the competing predictions of the two channels.

**Intermediate vs. final goods (wholesale).** We classify the 13 treated products as intermediate or final based on Feenstra & Jensen (2012). Five are intermediate goods (e.g. putty, paints, acids); others like refrigerators and washing machines are consumer durables (final goods). In the wholesale market captured by E-Way Bills data, the buyers of both types are firms: for intermediate goods, the buyers are other manufacturers; for final goods, the buyers are retailers. Under the GST's input tax credit mechanism, both types of buyers (regardless of a product's end-use classification) have a direct financial incentive to monitor tax rate changes and negotiate lower pre-tax prices from manufacturers. Consistent with this prediction, Appendix Table C.7 shows no significant difference in pass-through across the two types of products.

**Large vs. small brands at retail:** Large brands almost tautologically have more market power: their dominant revenue share reflects greater pricing leverage, more differentiated positioning, or stronger consumer loyalty. Under the market-structure channel, large brands should pass through *less* than small brands. Under the enforcement/attention channel, large brands should pass through *more*, because they are more visible to consumers, media, and regulators and visibility creates informal enforcement pressure that partially substitutes for the missing ITC chain. Table 4 (column 3) shows that large brands reduce prices more than small brands by 1.1%. This finding *contradicts* the market-structure prediction and *supports* the enforcement/attention channel.

**Premium vs. budget items at retail:** As discussed earlier, under the market-structure channel, premium items should pass through *less*. Under the enforcement/attention channel, premium items attract greater consumer, media, and regulatory attention and should pass through *more*. Table 4 (column 2) shows that budget items experience a $-2.2\%$ decline while premium items experience an additional $-4.1$ log-point decline, implying a total effect of approximately $-6.3\%$—closer to the full mechanical prediction. Again, the data contradict the market-structure prediction and support the enforcement/attention channel.

**Large vs. small firms at wholesale (ASI):** As reported in Table 3 (columns 4–6), wholesale pass-through is concentrated among larger manufacturers: their prices differentially fall by around 11% and quantities rise by 12%. This pattern is consistent with *both* channels. Large manufacturers are more visible to the tax administration (enforcement/attention) *and* operate with flatter supply curves due to excess installed capacity (market structure). We cannot fully separate the two channels at the wholesale-level.

The above empirical tests are suggestive of the enforcement/attention channel playing a dominant role over the market-structure channel in explaining the wholesale–retail gap in price pass-through. This does not mean market power is irrelevant. In fact, 56% retail pass-through is broadly consistent with imperfect competition and near-linear demand (Weyl & Fabinger, 2013). But within the retail stage, the cross-sectional variation in pass-through is driven more by buyer-side attention and visibility pressure than by underlying differences in market power. From a policy perspective, this implies that improving consumer-facing enforcement e.g. mandated digital receipts with tax breakdowns, consumer-awareness campaigns may be a more promising lever for increasing retail pass-through than structural reforms aimed at increasing retail competition.

# 6 Discussion

The reduced-form evidence in Section 3.2 shows near-complete pass-through at the wholesale stage, but it does not identify the demand and supply elasticities. These elasticities are important to estimate welfare gains from tax rate reductions. We now estimate these elasticities from the E-Way Bills data using the approach in Zoutman *et al.* (2018) and then estimate the welfare gains.

## 6.1 Demand and Supply Elasticities

We exploit the fact that if changes in tax rates are orthogonal to contemporaneous demand and supply shocks, then the tax rate $\tau_{jmt}$ provides exogenous variation that can be used to instrument for price in the demand equation and for quantity in the inverse supply equation. A key advantage of the E-Way Bills data is that they report both transaction value and tax collected. This allows us to recover the pre-tax price $p^*_{jsmt}$ from the tax-inclusive price $p_{jsmt}$. We estimate the following structural equations:

$$\ln q_{jsmt} = -\sigma \ln p_{jsmt} + \eta_{js} + \eta_{HS2-mt} + \eta_{smt} + \varepsilon^{q}_{jsmt}, \tag{8}$$

$$\ln p^*_{jsmt} = \omega^* \ln q_{jsmt} + \eta_{js} + \eta_{HS2-mt} + \eta_{smt} + \varepsilon^{p^*}_{jsmt}, \tag{9}$$

where $q_{jsmt}$ denotes quantity (measured as the number of E-Way Bills), $p_{jsmt}$ is the tax-inclusive price (value per E-Way Bill), and $p^*_{jsmt}$ is the pre-tax price. The parameter $\sigma$ governs demand, while $\omega^*$ is the inverse supply elasticity. The fixed effects $\eta_{js}$, $\eta_{HS2-mt}$, and $\eta_{smt}$ absorb state-product heterogeneity, HS-2 category-by-month shocks, and state-by-month shocks. We estimate both equations by IV using $\ln(1+\tau_{jmt})$ as the instrument. Appendix B provides the derivation, assumptions, and first-stage details.

Table 2 reports the results. Columns 1 and 2 show the reduced-form relationships between $\ln(1+\tau_{jmt})$ and log tax-inclusive prices and log quantities. Higher tax rates increase tax-inclusive prices and reduce quantities, consistent with the event-study evidence in Section 3.2. The first stage is strong in both cases. Column 3 implies a demand elasticity of $-2.57$, significant at the 1% level. Column 4 yields an inverse supply elasticity of $-0.07$, which is small in magnitude and statistically indistinguishable from zero.

**Retailer demand elasticity:** This demand estimate should be interpreted as a *derived* demand elasticity. The E-Way Bills data capture business-to-business transactions, so the relevant buyers are retailers and other downstream firms rather than final consumers. This

distinction matters. Downstream buyers in the GST chain observe taxes closely because invoice reconciliation and input-tax-credit claims depend on them (see Section 2.2). They are therefore likely to respond more strongly to tax-induced price changes than households do. In addition, retailers can substitute across suppliers more easily than consumers can substitute across products.

An elasticity of $-2.57$ is consistent with full pass-through at the wholesale stage. When manufacturers reduce prices following the tax cut, downstream buyers expand procurement sharply. The same interpretation helps explain the immediate quantity response in Figure 1, panel (b): informed B2B buyers adjust orders quickly once the tax change is announced, even before wholesale prices fully converge to their post-reform level.

**Supply elasticity:** The inverse supply elasticity is close to zero and imprecisely estimated, which implies a highly elastic supply curve over the margin affected by the reform. This shows that manufacturers are able to expand sales without increasing pre-tax prices. This is consistent with excess capacity, or more generally with a setting in which marginal costs do not rise much over the relevant range. With perfectly elastic supply, wholesale tax changes are passed through almost one for one to buyers.

Taken together, the two estimates rationalize the wholesale results. Demand by downstream buyers is elastic, and supply at the manufacturing stage is close to flat. This combination yields near-complete pass-through in the wholesale market. It also clarifies how to interpret the retail evidence. The incomplete retail pass-through documented in Section 4.2 implies that the elasticity relevant for final-consumer welfare is lower than the elasticity we estimate from wholesale transactions. We further discuss this distinction below.

## 6.2 Fiscal and Welfare Implications

We next use the elasticity and the retail pass-through estimates to assess the aggregate efficiency of the GST rate cuts. We focus on two measures: the elasticity required for

the reform to be revenue neutral, and the Marginal Value of Public Funds (MVPF). We distinguish the relevant behavioral response across the wholesale and retail stages.

### 6.2.1 Revenue Maximization and the Laffer Bound

A tax cut is self-financing if the induced expansion of the tax base offsets the mechanical loss in revenue. Under full pass-through, revenue is given by $R = \tau p^* q$, where $\tau$ is the ad valorem tax rate, $p^*$ is the pre-tax price, and $q$ is quantity. Differentiating revenue with respect to $\tau$ and setting the derivative to zero yields the marginal ad valorem Laffer condition:

$$|\varepsilon_D| > \frac{1+\tau}{\tau}. \tag{10}$$

At the pre-reform rate $\tau_0 = 0.28$, this threshold is

$$\frac{1+\tau_0}{\tau_0} = \frac{1.28}{0.28} \approx 4.57.$$

Our wholesale demand elasticity estimate, $|\hat{\varepsilon}_D| = 2.57$, is well below this value. Thus, the tax cut is not self-financing.[19]

The two-stage structure of the market strengthens this conclusion. The relevant response for tax revenue is final consumption, not retailer procurement. However, the elasticity we estimate from the E-Way Bills data is the elasticity of downstream buyers with respect to wholesale prices. Since consumer demand is plausibly less elastic than retailer demand, the expansion in the final tax base is likely smaller than what $|\hat{\varepsilon}_D| = 2.57$ would suggest. The retail evidence points in the same direction: only about 56% of the tax cut is passed on to

[19] Equation (10) gives the marginal Laffer condition evaluated at the pre-reform tax rate. For the finite tax reduction from $\tau_0 = 0.28$ to $\tau_1 = 0.18$, assuming constant elasticity demand and full pass-through, revenue neutrality would require

$$|\varepsilon_D| > \frac{\ln(\tau_0/\tau_1)}{\ln((1+\tau_0)/(1+\tau_1))} = \frac{\ln(0.28/0.18)}{\ln(1.28/1.18)} \approx 5.43.$$

This threshold is even higher than the marginal threshold, reinforcing the conclusion that the tax cut is not self-financing.

consumers. Thus, using the wholesale elasticity is likely to overstate the expansion of the final tax base and, if anything, understate the net fiscal cost of the reform.

### 6.2.2 The Marginal Value of Public Funds

The MVPF is the ratio of beneficiaries' willingness to pay to the net fiscal cost of the policy (Hendren & Sprung-Keyser, 2020). In our setting, these two objects depend on different margins. Consumer willingness to pay depends on the decline in retail prices, while the fiscal response depends on how much the tax base expands. We evaluate the MVPF locally at the pre-reform tax rate, $\tau_0 = 0.28$. Because we observe wholesale elasticities and retail pass-through, but do not directly estimate consumer demand, we report scenario-based bounds.

The *upper bound* treats the wholesale estimates as if they applied to final consumers. It therefore assigns both full pass-through and the retailer demand elasticity to the consumer-welfare calculation:

$$\text{MVPF}^{\text{upper}} \;=\; \frac{1}{1 - \frac{\tau_0}{1+\tau_0}\,|\hat{\varepsilon}_D|} \;\approx\; \frac{1}{1 - \frac{0.28}{1.28} \cdot 2.57} \;\approx\; 2.28. \tag{11}$$

This is the largest MVPF consistent with our estimates, but it requires assuming complete retail pass-through, an assumption our retail evidence points against.

The *lower bound* imposes the observed retail pass-through rate, $\rho = 0.56$, and replaces the retailer elasticity with a consumer elasticity $|\varepsilon_C|$ that is bounded above by $|\hat{\varepsilon}_D| = 2.57$. For $|\varepsilon_C| \in [1, 2]$,[20]

$$\text{MVPF}^{\text{lower}} \;=\; \frac{\rho}{1 - \rho\frac{\tau_0}{1+\tau_0}|\varepsilon_C|} \;\in\; [0.64,\, 0.74]. \tag{12}$$

An MVPF below one implies that, once incomplete retail pass-through is taken into account, the reform generated less than one rupee of consumer surplus per rupee of fiscal cost.

[20] The range $|\varepsilon_C| \in [1, 2]$ is consistent with estimates from the demand literature for consumer durables and packaged goods at the product-category level. Scanner-data and structural-IO studies often find category-level own-price elasticities between $-1$ and $-3$, with broader categories closer to $-1$ and narrower brand-level elasticities often above $-3$ (Hausman *et al.*, 1994; Nevo, 2001; Berry *et al.*, 1995).

The difference between the two bounds, 2.28 versus $[0.64, 0.74]$, quantifies the importance of incomplete retail pass-through. The share of the tax cut that does not reach consumers is retained by retailers as higher margins. The broader point is that incidence estimates alone are not sufficient for welfare analysis in a multi-stage market. The elasticity recovered from wholesale transactions need not coincide with the elasticity relevant for consumer surplus or tax revenue. Our decomposition makes that distinction explicit and shows that it is quantitatively important in this setting.

# 7 Conclusion

This paper uses three complementary datasets—administrative E-Way Bill records, the Annual Survey of Industries, and proprietary retail audit data—to decompose VAT pass-through into its wholesale and retail components, exploiting the rate reductions enacted under India's Goods and Services Tax in 2018–19. We find that wholesale pass-through is essentially complete: a ten-percentage-point statutory reduction lowers ex-factory and inter-firm transaction prices by approximately the full amount, with shipment volumes expanding sharply in response. Retail pass-through is substantially incomplete with only about 56% of the same statutory cut reaching final consumers. Higher retail pass-through in bigger and more premium brands is suggestive of enforcement playing an important role in increasing the pass-through, since larger, higher-visibility brands pass through *more* of the cut than smaller, less visible ones. Our results show that pass-through of tax reductions can vary along different points in the supply chain. At every stage where buyers have both the financial incentive—through the input-tax-credit chain—and the sophistication to verify and demand the price adjustment, pass-through is essentially complete. It is at the point where those frictions bind, between retailers and final consumers, that the leakage occurs.

Three policy implications follow. First, evaluating the consumer-welfare gains of a rate cut from wholesale or producer-price data alone will systematically overstate consumer benefit,

while evaluating upstream efficiency costs from retail data alone will understate manufacturer responsiveness. Second, our results imply that the welfare case for VAT rate cuts is weaker because retail pass-through is only about 56%. A substantial share of the statutory cut accrues to retailers as markup rather than to consumers as lower prices. Consumer surplus gains per rupee of forgone revenue are therefore small. Finally, the effectiveness of VAT reductions as consumer-facing stimulus depends less on the size of the rate cut than on the instruments that accompany it. Consumer labelling of reduced-rate goods, visible anti-profiteering enforcement, targeted monitoring of the most prominent firms, and sustained media attention are complements to—not substitutes for—the rate change itself. Where these are absent, the rate cut largely becomes a transfer to retailers, with limited consumer-price and quantity response.

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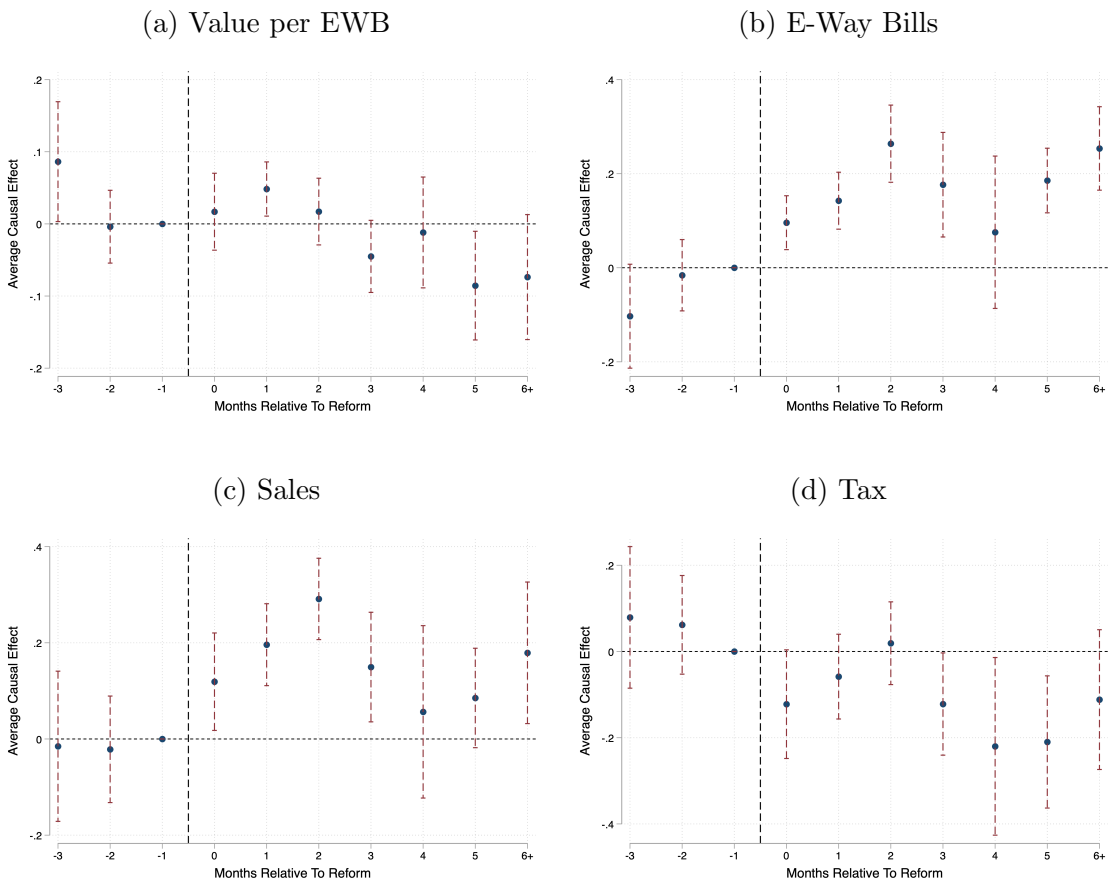
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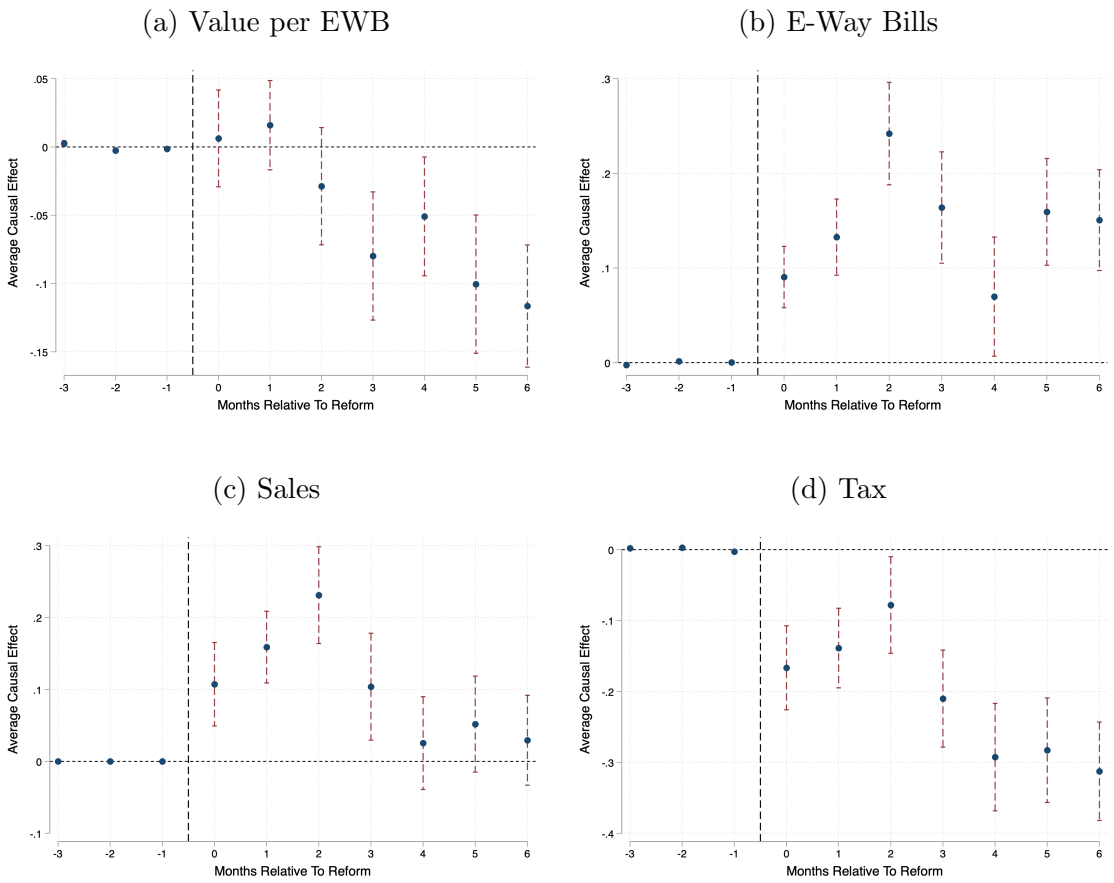
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Figure 1: Event Study: Impact of Tax Cuts on Product Outcomes



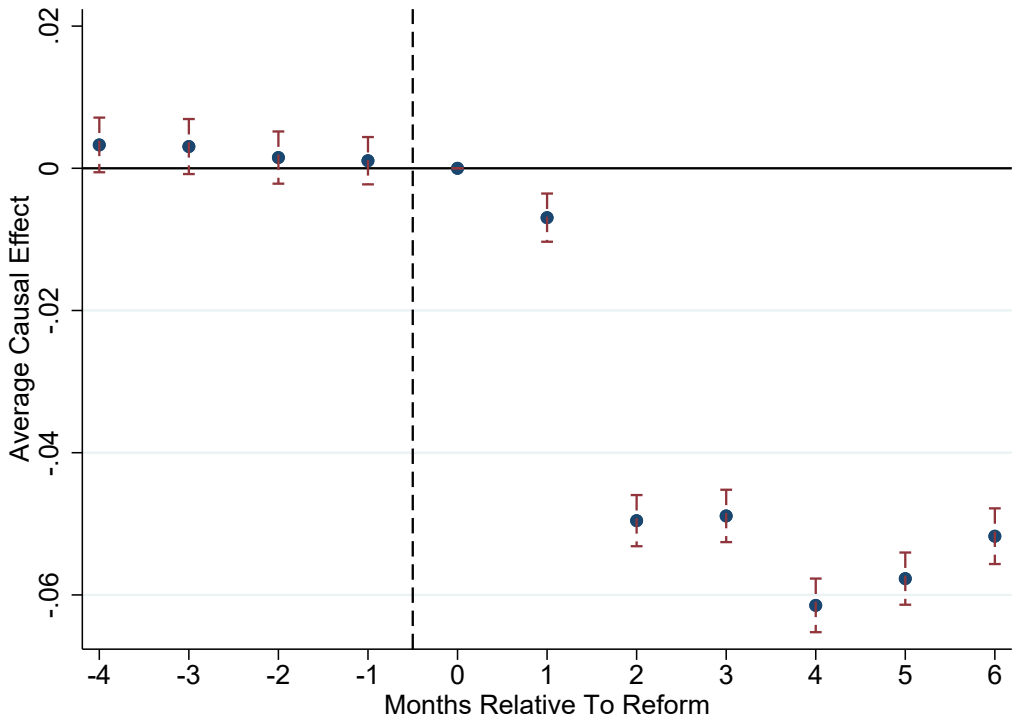

*Notes:* The figures in panels (a), (b), (c), and (d) plot the coefficient $\beta_\tau$ in Equation 2 to assess the impact of tax change for 13 tax rate changes (Clean Sample). These are the Two-Way Fixed Effects Event Study plots for the following outcome variables: Value per E-Way Bill, Count of E-Way Bills, Sales, and Tax Revenues. We have months relative to tax reform on the X-axis with one month prior to reform being the reference/base month in all the regressions. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. These regressions include the set of state × product combinations for which data on product outcomes is available for all 24 months from April 2018 to March 2020. The standard errors are clustered at HS-4 product level and 95% confidence intervals are plotted. The vertical line divides the pre-tax change period from the post-tax change period.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Figure 2: Event Study: Impact of Tax Cuts on Product Outcomes (Synthetic DID Estimates)




*Notes:* The figures in panels (a), (b), (c), and (d) plot the coefficients from SDID estimates to assess the impact of tax change for 13 tax rate changes (Clean Sample). These are the Two-Way Fixed Effects Event Study plots for the following outcome variables: Value per E-Way Bill, Count of E-Way Bills, Sales, and Tax Revenues. We have months relative to tax reform on the X-axis with one month prior to reform being the reference/base month in all the regressions. These regressions include the set of state × product combinations for which data on product outcomes is available for all 24 months from April 2018 to March 2020. The standard errors are clustered at HS-4 product level and 95% confidence intervals are plotted. The vertical line divides the pre-tax change period from the post-tax change period.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Figure 3: Event Study: Impact of Tax Cuts on Retail Prices




*Notes:* The figure plots the changes after the tax rate reduction, pooled across the three products - refrigerators, washing machines, and electric water heaters for which we obtain retail data. The base month is June 2018 (the last pre-reform month). Item and city fixed effects are included. Heteroscedasticity-robust standard errors are reported and 95% confidence intervals shown. The vertical line divides the pre-tax change period from the post-tax change period. *Source:* Retail data (January 2018 - December 2018)

Table 1: Impact of Tax Cuts on Product Outcomes

| | Value per EWB (1) | EWB (2) | Sales (3) | Tax Revenue (4) | Pre-tax Value per EWB (5) |
|---|---|---|---|---|---|
| Panel A: ATT | | | | | |
| Tax Change × Post | –0.11*** | 0.28*** | 0.17*** | –0.17*** | –0.02 |
| | (0.04) | (0.04) | (0.06) | (0.06) | (0.04) |
| R-Squared | 0.91 | 0.98 | 0.96 | 0.96 | 0.91 |
| Number of Observations | 333240 | 333240 | 333240 | 333240 | 333160 |
| Panel B: Elasticity | | | | | |
| Δ Tax Change × Post | –1.40*** | 3.68*** | 2.27*** | –2.25*** | –0.24 |
| | (0.53) | (0.46) | (0.73) | (0.79) | (0.51) |
| R-Squared | 0.91 | 0.98 | 0.96 | 0.96 | 0.91 |
| Number of Observations | 333240 | 333240 | 333240 | 333240 | 333160 |
| *Controls* | | | | | |
| HS-2 × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| HS-4 × State | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* The above table presents the coefficient $\beta$ in Equation 1 to assess the impact of tax rate reduction for 13 tax rate changes (HS-4 codes within which all products faced a reduction in tax rates, enacted in the 28th GST Council meeting). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). $TaxCut$ equals one for products that underwent tax rate reduction and $Post_t$ equals one from August 2018 onwards. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. Inclusion of HS-2 product × month-year ensures that the treated and untreated HS-4 products are compared with each other within a given HS-2 over time. These regressions include the set of state × product combinations for which domestic sales data is available for all 24 months from April 2018 to March 2020. Each column reports the effective number of observations after incorporating the included fixed effects. The standard errors are clustered at HS-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Table 2: Demand and Supply Elasticities

| | $\ln p_{igt}$ | $\ln q_{igt}$ | $\ln q_{igt}$ | $\ln p^*_{igt}$ |
|---|---|---|---|---|
| $\ln(1+\tau_{gt})$ | 1.37*** | –3.53*** | | |
| | (0.50) | (0.44) | | |
| | [0.01] | [0.00] | | |
| $\ln p_{igt}$ | | | –2.57*** | |
| | | | (0.97) | |
| | | | [0.01] | |
| $\ln q_{igt}$ | | | | –0.07 |
| | | | | (0.14) |
| | | | | [0.61] |
| Number of Observations | 230928 | 230877 | 230928 | 230877 |
| *Controls* | | | | |
| HS-2 × Month × Year | ✓ | ✓ | ✓ | ✓ |
| HS-4 × State | ✓ | ✓ | ✓ | ✓ |
| State × Month × Year | ✓ | ✓ | ✓ | ✓ |

*Notes:* The above table presents the first and second stages of Instrumental Variable regressions explained in Appendix Section B. $\ln(1+\tau_{gt})$ denotes the GST Rate for a product, $\ln q_{igt}$ denotes the count of E-Way Bills (our proxy for quantity), $\ln p^*_{igt}$ and $\ln p_{igt}$ represent Pre-Tax Value per E-Way Bill and Value per E-Way Bill which are our proxies for Pre-Tax Prices and Tax-inclusive Prices respectively. Column (3) reports the demand curve equation; the first stage is column (1). Column (4) reports the supply curve equation; the first stage is column (2). All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. These regressions include the set of state × product combinations for which domestic sales data is available for all 24 months from April 2018 to March 2020. Each column reports the effective number of observations after incorporating the included fixed effects. Outliers in the data are winsorized at 5% on both ends at the HS-4 Product x Year level. The standard errors are clustered at HS-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors. Figures in square brackets are p-values.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Table 3: Impact of Tax Cuts on Product Outcomes - ASI Data

| | Prices (1) | Quantity (2) | Sales (3) | Prices (4) | Quantity (5) | Sales (6) |
|---|---|---|---|---|---|---|
| Tax Change × Post | –0.08 | –0.00 | –0.03 | –0.02 | –0.13* | –0.14*** |
| | (0.06) | (0.07) | (0.02) | (0.07) | (0.08) | (0.03) |
| | [0.20] | [0.98] | [0.21] | [0.73] | [0.10] | [0.00] |
| Tax Change × Post × **1**[Firm Size] | | | | –0.11* | 0.25*** | 0.22*** |
| | | | | (0.06) | (0.05) | (0.03) |
| | | | | [0.09] | [0.00] | [0.00] |
| R-Squared | 0.92 | 0.93 | 0.96 | 0.92 | 0.93 | 0.96 |
| Number of Observations | 70317 | 70317 | 70317 | 70317 | 70317 | 70317 |
| Wald Test | | | | 0.06 | 0.09 | 0.00 |
| *Controls* | | | | | | |
| Firm × CPC-4 | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| CPC-2 × State × Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Firm Size × Year | | | | ✓ | ✓ | ✓ |

*Notes:* The above table presents the impact of tax rate cuts on firm-product level outcomes using the ASI data. Columns (1)-(3) show coefficient $\beta$ in Equation 4.
**1**[Firm-Size] signifies large firms where firm size is defined based on median CPC-4 product level sales (gross value) in 2017. Firm × Product combinations with higher than median product sales are classified as large firms while Firm × Product combinations with lower than median product sales are classified as small firms. All regressions include firm × CPC-4 product and CPC-2 product × state × year fixed effects. Inclusion of CPC-2 product × state × year ensures that the treated and untreated CPC-4 products are compared with each other within a given CPC-2 in a state over time. These regressions include the set of firm × product combinations for which data is available for all 4 years from 2017 to 2020. However, for the regressions, only years 2017 to 2019 are considered. Each column reports the effective number of observations after incorporating the included fixed effects. Outliers in the ASI data are winsorized at 5% on both ends at the State x Product x Year Level. The standard errors are clustered at CPC-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors. Figures in square brackets are p-values.
*Source:* Annual Survey of Industries Data (2017 to 2020)

Table 4: Impact of Tax Cuts on Retail Prices and Heterogeneity

| | (1)<br>Overall | (2)<br>Price Tier | (3)<br>Brand Share |
|---|---|---|---|
| Post | -0.044*** | -0.022*** | -0.040*** |
| | (0.001) | (0.001) | (0.001) |
| Post × Premium | | -0.041*** | |
| | | (0.002) | |
| Post × Large Brand | | | -0.011*** |
| | | | (0.002) |
| R-Squared | 0.99 | 0.99 | 0.99 |
| Number of Observations | 31,395 | 28,981 | 31,288 |
| *Controls* | | | |
| Item × City FE | ✓ | ✓ | ✓ |

*Notes:* The table reports coefficients from the specification $\ln(\text{Price}_{it}) = \beta \cdot \text{Post}_t + \delta_i + \gamma_c + \varepsilon_{it}$, where *Post* equals one from August 2018 onward. Column (1) reports the overall pooled pass-through. Column (2) adds an interaction with a premium indicator, equal to one if the item's average pre-reform (January–June 2018) price exceeds the within-product-category median. Column (3) adds an interaction with a large brand indicator, equal to one if the brand's pre-reform revenue share exceeds the within-category median. All regressions include item and city fixed effects and are restricted to year 2018. Figures in parentheses are heteroscedasticity-robust standard errors. ***, **, * denote significance at 1%, 5%, and 10%.
*Source:* Retail audit data, January–December 2018.

# ONLINE APPENDIX

## A Tax Rate Reductions

During our sample period from April 2018 to March 2020, GST rates for goods were revised in four GST Council meetings. Across these meetings, a total of 70 products experienced a tax rate change. The relevant meetings and effective dates were:

- 28th GST Council Meeting – effective 27 July 2018 (13 clean tax cuts);
- 31st GST Council Meeting – effective 1 January 2019 (2 clean tax cuts);
- 36th GST Council Meeting – effective 1 August 2019 (no clean tax cuts);
- 37th GST Council Meeting – effective 1 October 2019 (no clean tax cuts).

Our analysis focuses only on goods and on tax rate reductions. We exclude products that experienced tax increases, products that underwent multiple tax revisions during the sample period, products that are not observed in the E-Way Bills data, and products without complete coverage for all 24 months from April 2018 to March 2020. We also exclude partial tax cuts, defined as cases in which the tax revision applied only to a subset of products within a given HS-4 category rather than to the entire category.

Specifically, 7 products experienced multiple tax revisions during the sample period. E-Way Bills data are unavailable for 4 products, while an additional 6 products are excluded because they are not observed in all 24 months. Of the remaining 53 products, 7 experienced tax increases and 31 correspond to HS-4 categories in which the tax revision applied only to a subset of underlying products. These restrictions leave 15 HS-4 product categories that experienced clean tax cuts, of which 13 were affected by the 28th GST Council Meeting and 2 by the 31st GST Council Meeting.

For our main analysis, we focus on the 13 clean tax cuts implemented following the 28th GST Council Meeting. Restricting attention to this single reform episode provides the longest post-treatment window in our sample and avoids complications associated with staggered treatment timing. The remaining two clean tax cuts, implemented following the 31st GST Council Meeting, are included in robustness exercises.

The 15 clean tax-cut products span the following 7 HSN 2-digit categories:

- 28 – Gases and inorganic chemicals;
- 32 – Colouring and tanning products;

- 40 – Rubber and rubber products;
- 84 – Industrial tools and machinery;
- 85 – Electrical and electronic products;
- 87 – Motor vehicles;
- 96 – Pens, pencils, educational equipment, and smoking pipes.

Our main results are based on the 13 products affected in July 2018, for which the tax rate reduction applied to the entire HS-4 category. The results are similar when we expand the treated sample to include all 15 clean tax cuts.[1] We do not include tax increases in the analysis because they are few in number and concentrated in just 2 HS-2 categories.

[1]When all 15 tax cuts are included, corresponding to an average tax rate reduction of approximately 9.8 percentage points, Value per E-Way Bill declines significantly by approximately 11 percent, consistent with near-complete pass-through.

Table A.1: Details on Tax Changes

| Product (HS 4-digit) (1) | Product Description (2) | Tax Decline (%) (3) | Initial Tax Rate (%) (4) |
|---|---|---|---|
| 2809 | Fertilizer Grade Phosphoric Acid | 7 | 12 |
| 3208 | Paints and Varnishes dispersed in a non-aqueous medium | 10 | 28 |
| 3209 | Paints and Varnishes dispersed in an aqueous medium | 10 | 28 |
| 3210 | Other Paints and Varnishes | 10 | 28 |
| 3214 | Glaziers Putty, Grafting Putty, and other mastics | 10 | 28 |
| 8418 | Refrigerators and Freezers | 10 | 28 |
| 8450 | Washing Machines | 10 | 28 |
| 8508 | Vacuum Cleaners | 10 | 28 |
| 8510 | Shavers and Hair Clippers | 10 | 28 |
| 8516 | Electric instantaneous or storage water heaters | 10 | 28 |
| 8705 | Special Purpose Motor Vehicles | 10 | 28 |
| 8709 | Self-propelled Work Trucks used in factories | 10 | 28 |
| 9616 | Scent Sprays and toilet sprays | 10 | 28 |

*Notes:* The above table gives the HSN 4-digit products that underwent a tax decline during 2018-2020. Column (2) gives the product description corresponding to HSN 4-digit Product. Column (3) gives the magnitude of tax decline, while column (4) gives the tax rate of the HS-4 Product before the tax reform. The above list includes the set of those HSN 4-digit products where the entire product (at the HS-4 level) witnessed tax decline. Recommendations of the 28th GST Council Meeting came into effect from 27 July 2018. We denote months from August 2018 onwards as the treatment period.).
*Source:* Press Releases by GST Council and Central Board of Indirect Taxes and Customs (CBIC)

Table A.2: Summary Statistics - E-Way Bills Dataset

| | Domestic Sales | |
|---|---|---|
| | Products with Tax Changes (1) | Products without Tax Changes (2) |
| Number of Observations | 5,640 | 327,648 |
| Number of Products | 13 | 973 |
| State × Product Combinations | 235 | 13652 |
| Total Sales (INR Millions) | 537.8 (1106.7) | 544.5 (2322.8) |
| Number of E-Way Bills | 4949.98 (10394.22) | 2245.27 (16377.16) |
| Tax Revenues (INR Millions) | 102.3 (247.8) | 105.5 (13518.4) |
| Value per E-Way Bill | 0.02 (0.05) | 0.04 (0.13) |

*Notes:* The above table shows the average monthly sales, count of E-Way Bills, Tax revenues, and Value per E-Way bill for an HS-4 product. Figures in parentheses are corresponding sample standard deviations. These statistics are for the time period April 2018 to March 2020 for a balanced set of state × HS-4 product combinations, i.e., combinations which have reported data for all 24 months.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Table A.3: Summary Statistics - ASI Dataset

| | With Tax Changes (1) | Without Tax Changes (2) |
|---|---|---|
| Number of Observations | 692 | 93,504 |
| Number of Firms | 173 | 17455 |
| Number of Products | 2 | 561 |
| Firm × Product Combinations | 173 | 23376 |
| Number of Small Firms | 86 | 9362 |
| Number of Large Firms | 87 | 9763 |
| Total Sales (in INR Million) | 2,300 (5,260) | 1,430 (7,520) |

*Notes:* The above table gives the summary statistics for the Annual Survey of Industries (ASI) dataset. Mean total sales for a firm is in INR million. Figures in parentheses are corresponding sample standard deviations. Firm Size is defined based on median CPC-4 product level sales (gross value) in 2017. Firm × Product combinations with higher than median product sales are classified as large firms while Firm × Product combinations with lower than median product sales are classified as small firms.
The 2 treated products are CPC 3511 (Paints and Varnishes) and CPC 4353 (Fork-lift trucks). These 2 CPC-4 Products which underwent a tax change during the 28th GST Council meeting are mapped to 5 HS-4 Products.
*Source:* Annual Survey of Industries Data (2017 to 2020)

# B Estimation of Product Elasticities: Empirical Details

We adapt the instrumental-variable approach of Zoutman *et al.* (2018) and Fajgelbaum *et al.* (2020) to estimate product-level demand and inverse supply elasticities using variation in GST rates. The approach exploits the tax wedge between the tax-inclusive price paid by buyers, $p_{jsmt}$, and the pre-tax price received by sellers, $p^*_{jsmt}$:

$$p_{jsmt} = (1 + \tau_{jmt})p^*_{jsmt}, \tag{B.13}$$

where $\tau_{jmt}$ denotes the GST rate applicable to product $j$ in month-year $mt$. Since GST rates are uniform across states for a given product and time period, $\tau_{jmt}$ does not vary across states.

We estimate the following demand equation:

$$\ln q_{jsmt} = -\sigma \ln p_{jsmt} + \eta_{js} + \eta_{HS2-mt} + \eta_{smt} + \varepsilon^q_{jsmt} \tag{B.14}$$

where $\sigma > 0$ denotes the absolute value of the demand elasticity.

We also estimate the following inverse supply equation:

$$\ln p^*_{jsmt} = \omega^* \ln q_{jsmt} + \eta_{js} + \eta_{HS2-mt} + \eta_{smt} + \varepsilon^{p^*}_{jsmt} \tag{B.15}$$

where $\omega^*$ denotes the inverse supply elasticity. A value of $\omega^*$ close to zero corresponds to an approximately flat inverse supply curve.

The endogenous variables in equations (B.14) and (B.15) are instrumented using variation in the log gross-of-tax rate, $\ln(1 + \tau_{jmt})$. The first stage for the demand equation is:

$$\ln p_{jsmt} = \pi_p \ln(1 + \tau_{jmt}) + \eta_{js} + \eta_{HS2-mt} + \eta_{smt} + \varepsilon^p_{jsmt} \tag{B.16}$$

The first stage for the inverse supply equation is:

$$\ln q_{jsmt} = \pi_q \ln(1 + \tau_{jmt}) + \eta_{js} + \eta_{HS2-mt} + \eta_{smt} + \varepsilon^q_{jsmt} \tag{B.17}$$

Here, $q_{jsmt}$ denotes quantity, proxied by the number of E-Way Bills; $p_{jsmt}$ denotes the tax-inclusive price, measured as value per E-Way Bill; and $p^*_{jsmt}$ denotes the pre-tax price, measured as pre-tax value per E-Way Bill. We aggregate intra-state and inter-state transactions to construct these measures at the state × HS-4 product × month-year level.

All specifications include HS-4 product × state fixed effects, $\eta_{js}$, which absorb time-invariant heterogeneity at the state-product level. HS-2 × month-year fixed effects, $\eta_{HS2-mt}$, absorb common time-varying shocks within broad product categories, including category-specific seasonality. State × month-year fixed effects, $\eta_{smt}$, absorb shocks common to all products within a state in a given month.

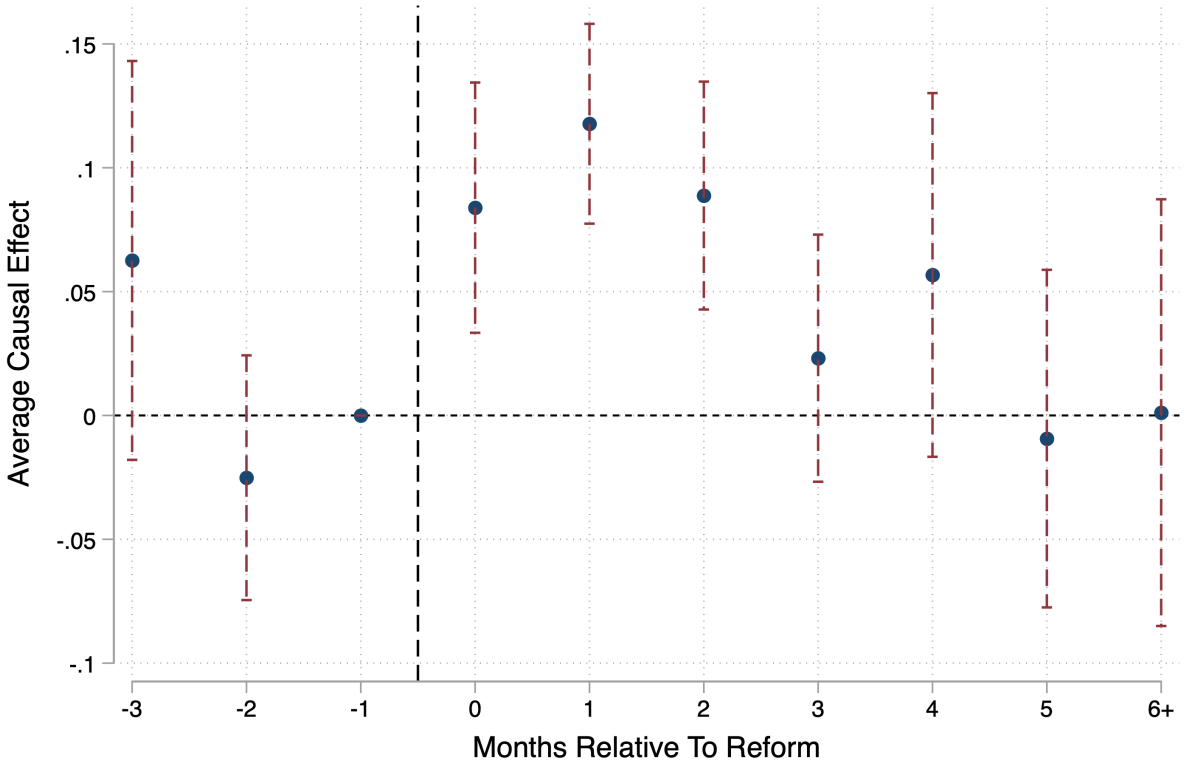
Identification follows the logic of Zoutman *et al.* (2018). First, the Ramsey Exclusion Restriction requires that buyers respond to the GST rate only through the tax-inclusive price they face. Conditional on $p_{jsmt}$, the tax rate therefore does not enter the demand equation directly. Second, the standard exclusion restriction requires that the tax rate does not directly shift the supply curve: suppliers respond to the pre-tax price $p^*_{jsmt}$, while changes in the GST rate affect supply only through the equilibrium pre-tax price. In addition, we require that, conditional on the fixed effects, changes in GST rates are uncorrelated with residual demand and supply shocks.

Under these assumptions, variation in $\ln(1 + \tau_{jmt})$ can be used as a single instrument to identify both sides of the market. To estimate the demand elasticity, we instrument the tax-inclusive price $p_{jsmt}$ with $\ln(1 + \tau_{jmt})$ using equation (B.16), and use the predicted variation in prices to estimate equation (B.14). To estimate the inverse supply elasticity, we instrument quantity $q_{jsmt}$ with $\ln(1 + \tau_{jmt})$ using equation (B.17), and use the predicted variation in quantity to estimate the inverse supply equation (B.15).

We compile product-level GST rates from official notifications of the Central Board of Indirect Taxes and Customs (CBIC). We begin with the GST rates applicable on 1 April 2018 and incorporate all subsequent rate changes occurring during the sample period from April 2018 to March 2020.

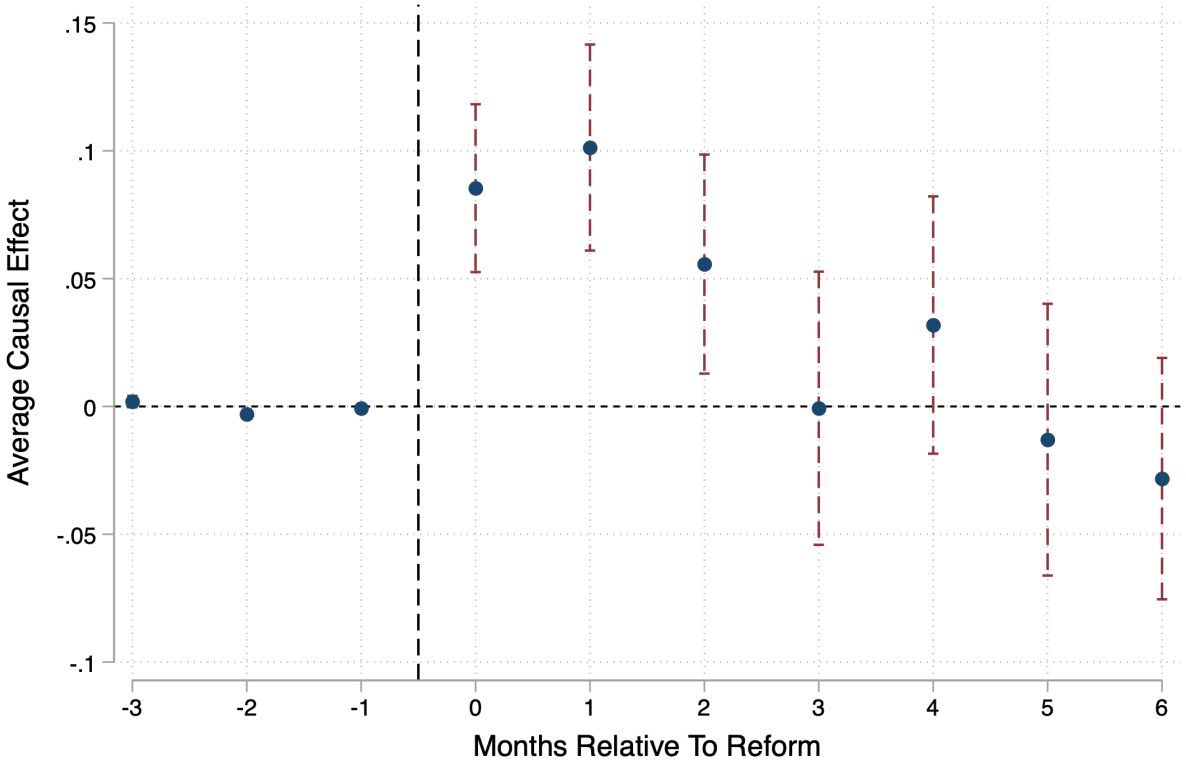
# C Additional Results

Figure C.1: Impact of Tax Cuts on Pre-tax Prices




*Notes:* The figure plots the coefficient $\beta_\tau$ in Equation 2 to assess the impact of tax change for 13 tax rate changes (Clean Sample). This figure is the Two-Way Fixed Effects Event Study plot for Pre-tax Prices. We have months relative to tax reform on the X-axis with one month prior to reform being the reference/base month in all the regressions. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. These regressions include the set of state × product combinations for which data on product outcomes is available for all 24 months from April 2018 to March 2020. The standard errors are clustered at HS-4 product level and 95% confidence intervals are plotted. The vertical line divides the pre-tax change period from the post-tax change period.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Figure C.2: Impact of Tax Cuts on Pre-tax Prices (Synthetic DiD Estimate)




*Notes:* The figure plots the coefficient $\beta_\tau$ in Equation 2 to assess the impact of tax change for 13 tax rate changes (Clean Sample). We have months relative to tax reform on the X-axis with one month prior to reform being the reference/base month in all the regressions. These regressions include the set of state × product combinations for which data on product outcomes is available for all 24 months from April 2018 to March 2020. The standard errors are clustered at HS-4 product level and 95% confidence intervals are plotted. The vertical line divides the pre-tax change period from the post-tax change period.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Table C.1: Impact of Tax Cut on Product Outcomes: SDID Estimates

| | Value per EWB (1) | EWB (2) | Sales (3) | Tax Revenue (4) | Pre-tax Value per EWB (5) |
|---|---|---|---|---|---|
| Tax Change × Post | –0.09*** | 0.22*** | 0.14*** | –0.18*** | 0.00 |
| | (0.02) | (0.03) | (0.03) | (0.03) | (0.02) |
| Number of Observations | 333288 | 333288 | 333288 | 333288 | 331536 |

*Notes:* The table shows the impact of tax rate cuts for 13 tax rate changes (HS-4 codes within which all products faced a reduction in tax rates, enacted in the 28th GST Council meeting) on product outcomes using the Synthetic DiD estimator (SDID) proposed by Arkhangelsky *et al.* (2021). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). $TaxCut$ equals one for products that underwent tax rate reduction and $Post_t$ equals one from August 2018 onwards. These regressions include the set of state × product combinations for which balanced data is available for prices for all 24 months from April 2018 to March 2020. The standard errors in parenthesis are bootstrapped at HS-4 product level with 100 replications. ***, **, * show significance at 1%, 5% and 10% levels respectively.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020).

Table C.2: Impact of Tax Cut on Product Outcomes: Robustness after dropping Close HS-4 Substitutes

| | Value per EWB (1) | EWB (2) | Sales (3) | Tax Revenue (4) | Pre-tax Value per EWB (5) |
|---|---|---|---|---|---|
| Tax Change × Post | –0.11*** | 0.28*** | 0.17*** | –0.18*** | –0.02 |
| | (0.04) | (0.04) | (0.06) | (0.06) | (0.04) |
| R-Squared | 0.91 | 0.98 | 0.96 | 0.96 | 0.91 |
| Number of Observations | 323496 | 323496 | 323496 | 323496 | 323421 |
| *Controls* | | | | | |
| HS-2 × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| HS-4 × State | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* The above table presents the coefficient $\beta$ in Equation 1 to assess the impact of tax rate reduction for 13 tax rate changes (HS-4 codes within which all products faced a reduction in tax rates, enacted in the 28th GST Council meeting). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). Here, 22 HS-4 Products which are close demand substitutes to the 13 treated HS-4 products are dropped. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. Inclusion of HS-2 product × month-year ensures that the treated and untreated HS-4 products are compared with each other within a given HS-2 over time. These regressions include the set of state × product combinations for which domestic sales data is available for all 24 months from April 2018 to March 2020. Each column reports the effective number of observations after incorporating the included fixed effects. The standard errors are clustered at HS-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020).

Table C.3: Impact of Tax Cut on Product Outcomes: Including Partial Tax Cuts

| | Value per EWB (1) | EWB (2) | Sales (3) | Tax Revenue (4) | Pre-tax Value per EWB (5) |
|---|---|---|---|---|---|
| Tax Change × Post | –0.05* | 0.20*** | 0.15*** | –0.10* | –0.00 |
| | (0.03) | (0.03) | (0.04) | (0.05) | (0.02) |
| R-Squared | 0.91 | 0.98 | 0.96 | 0.96 | 0.91 |
| Number of Observations | 340320 | 340320 | 340320 | 340320 | 340240 |
| *Controls* | | | | | |
| HS-2 × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| HS-4 × State | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* The above table presents the coefficient $\beta$ in Equation 1 to assess the impact of tax rate reduction for 34 tax rate changes (all reductions enacted in the 28th GST Council meeting). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). $TaxCut$ equals one for products that underwent tax rate reduction and $Post_t$ equals one from August 2018 onwards. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. Inclusion of HS-2 product × month-year ensures that the treated and untreated HS-4 products are compared with each other within a given HS-2 over time. These regressions include the set of state × product combinations for which domestic sales data is available for all 24 months from April 2018 to March 2020. Each column reports the effective number of observations after incorporating the included fixed effects. The standard errors are clustered at HS-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020)

Table C.4: Impact of Tax Cut on Product Outcomes: SDID Estimates (All tax cuts)

| | Value per EWB (1) | EWB (2) | Sales (3) | Tax Revenue (4) | Pre-tax Value per EWB (5) |
|---|---|---|---|---|---|
| Tax Change × Post | –0.08*** | 0.18*** | 0.10*** | –0.21*** | 0.00 |
| | (0.02) | (0.03) | (0.02) | (0.03) | (0.02) |
| Number of Observations | 334320 | 334320 | 334320 | 334320 | 332568 |

*Notes:* The table shows the impact of tax rate cuts for 15 tax rate changes (HS-4 codes within which all products faced a reduction in tax rates, enacted in the 28th GST Council meeting and the 31st GST Council meeting) on product outcomes using the Synthetic DiD estimator (SDID) proposed by Arkhangelsky *et al.* (2021). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). $TaxCut$ equals one for products that underwent tax rate reduction and $Post_t$ equals one from August 2018 onwards. These regressions include the set of state × product combinations for which balanced data is available for prices for all 24 months from April 2018 to March 2020. The standard errors in parenthesis are bootstrapped at HS-4 product level with 100 replications. ***, **, * show significance at 1%, 5% and 10% levels respectively.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020).

Table C.5: Impact of Tax Cuts on Product Outcomes - ASI Data (Elasticity)

| | Prices (1) | Quantity (2) | Sales (3) | Prices (4) | Quantity (5) | Sales (6) |
|---|---|---|---|---|---|---|
| Δ Tax Change × Post | –0.99 | –0.02 | –0.36 | –0.30 | –1.61* | –1.73*** |
| | (0.77) | (0.90) | (0.29) | (0.87) | (0.97) | (0.37) |
| | [0.20] | [0.98] | [0.21] | [0.73] | [0.10] | [0.00] |
| Δ Tax Change × Post × **1**[Firm Size] | | | | –1.39* | 3.25*** | 2.79*** |
| | | | | (0.83) | (0.66) | (0.44) |
| | | | | [0.09] | [0.00] | [0.00] |
| R-Squared | 0.92 | 0.93 | 0.96 | 0.92 | 0.93 | 0.96 |
| Number of Observations | 70317 | 70317 | 70317 | 70317 | 70317 | 70317 |
| Wald Test | | | | 0.06 | 0.09 | 0.00 |
| *Controls* | | | | | | |
| Firm × CPC-4 | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| CPC-2 × State × Year | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Firm Size × Year | | | | ✓ | ✓ | ✓ |

*Notes:* The above table presents the impact of tax rate cuts on firm-product level outcomes using the ASI data. Columns (1)-(3) show coefficient $\beta$ in Equation 4. **1**[Firm-Size] signifies large firms where firm size is defined based on median CPC-4 product level sales (gross value) in 2017. Firm × Product combinations with higher than median product sales are classified as large firms while Firm × Product combinations with lower than median product sales are classified as small firms. All regressions include firm × CPC-4 product and CPC-2 product × state × year fixed effects. Inclusion of CPC-2 product × state × year ensures that the treated and untreated CPC-4 products are compared with each other within a given CPC-2 in a state over time. These regressions include the set of firm × product combinations for which data is available for all 4 years from 2017 to 2020. However, for the regressions, only years 2017 to 2019 are considered. Each column reports the effective number of observations after incorporating the included fixed effects. Outliers in the ASI data are winsorized at 5% on both ends at the State x Product x Year Level. The standard errors are clustered at CPC-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors. Figures in square brackets are p-values.
*Source:* Annual Survey of Industries Data (2017 to 2020)

Table C.6: Impact of Tax Cuts on Product Outcomes (E-Way Bills): Robustness to Retail Products and Time Period

| | Value per EWB (1) | Pre-tax Value per EWB (2) |
|---|---|---|
| Post Tax Change | –0.07*** | 0.02 |
| | (0.02) | (0.02) |
| R-Squared | 0.85 | 0.85 |
| Number of Observations | 720 | 720 |
| *Controls* | | |
| HS-4 × State | ✓ | ✓ |

*Notes:* The table estimates change in prices in the E-way bills data for the treated products in the retail data—Refrigerators, Washing Machines, and Water Heaters using data from April 2018 to December 2018. $\ln(\text{Price}_{it}) = \beta \cdot \text{Post}_t + \delta_i + \gamma_c + \varepsilon_{it}$, where *Post* equals one from August 2018 onward. The outcomes are: Value per E-Way Bill (columns 1-2) and Pre-tax Value per E-Way Bill (columns 3-4). *Post* equals one from August 2018 onwards. All regressions include state × HS-4 product fixed effects. These regressions include the set of state × product combinations for which domestic sales data is available for all months. Each column reports the effective number of observations after incorporating the included fixed effects. The standard errors reported in paranthesis are robust to heteroscedasticity. ***, **, * show significance at 1%, 5% and 10% levels respectively.
*Source:* State × Product level E-Way Bills data (April 2018 to December 2018).

Table C.7: Impact of Tax Cut on Product Outcomes: Heterogeneity by Final vs Intermediate Good

| | Value per EWB | EWB | Sales | Tax Revenue | Pre-tax Value per EWB |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Tax Change × Post | –0.08 | 0.29*** | 0.20** | –0.09 | –0.00 |
| | (0.08) | (0.07) | (0.09) | (0.10) | (0.07) |
| Tax Change × Post × Final Good | –0.04 | –0.02 | –0.04 | –0.15 | –0.02 |
| | (0.09) | (0.08) | (0.11) | (0.12) | (0.08) |
| R-Squared | 0.91 | 0.98 | 0.96 | 0.96 | 0.90 |
| Number of Observations | 295104 | 295104 | 295104 | 295104 | 295040 |
| Wald Test | 0.00 | 0.00 | 0.02 | 0.00 | 0.50 |
| *Controls* | | | | | |
| HS-2 × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| HS-4 × State | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |
| Final Good × Month × Year | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* The above table presents the coefficient $\beta$ in Equation 1 to assess the impact of tax rate reduction for 13 tax rate changes (HS-4 codes within which all products faced a reduction in tax rates, enacted in the 28th GST Council meeting). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). $TaxCut$ equals one for products that underwent tax rate reduction and $Post_t$ equals one from August 2018 onwards. *Final Good* equals one when a given product is used for final consumption and zero otherwise. The classification of goods into intermediate vs final is based on Feenstra & Jensen (2012) with HSN-4 codes where all sub-categories are final goods being classified as the *Final Good*. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. Inclusion of HS-2 product × month-year ensures that the treated and untreated HS-4 products are compared with each other within a given HS-2 over time. These regressions include the set of state × product combinations for which domestic sales data is available for all 24 months from April 2018 to March 2020. Each column reports the effective number of observations after incorporating the included fixed effects. The standard errors are clustered at HS-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors.

*Source:* State × Product level E-Way Bills data (April 2018 - March 2020).

Table C.8: Impact of Tax Cut on Product Outcomes: Intra vs Inter State

| | Value per EWB | EWB | Sales | Tax Revenue | Pre-tax Value per EWB |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Tax Change × Post | –0.07* | 0.27*** | 0.19** | –0.15* | 0.01 |
| | (0.04) | (0.05) | (0.07) | (0.08) | (0.04) |
| Tax Change × Post × 1.Outward Sales | 0.01 | –0.10* | –0.07 | –0.06 | 0.03 |
| | (0.04) | (0.06) | (0.05) | (0.07) | (0.05) |
| R-Squared | 0.88 | 0.97 | 0.95 | 0.93 | 0.88 |
| Number of Observations | 509451 | 509451 | 509451 | 509451 | 509352 |
| *Controls* | | | | | |
| HS-2 × Month × Year × Outward Sales | ✓ | ✓ | ✓ | ✓ | ✓ |
| HS-4 × State × Outward Sales | ✓ | ✓ | ✓ | ✓ | ✓ |
| State × Month × Year × Outward Sales | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes:* The above table presents the coefficient $\beta$ in Equation 1 to assess the impact of tax rate reduction for 13 tax rate changes (HS-4 codes within which all products faced a reduction in tax rates, enacted in the 28th GST Council meeting). The outcomes are: Value per E-Way Bill (column 1), Count of E-Way Bills (column 2), Sales (column 3), Tax Revenues (column 4), and Pre-tax Value per E-Way Bill (column 5). The dependent variable in Panel A is intra-state supplies and in panel B is inter-state supplies. $TaxCut$ equals one for products that underwent tax rate reduction and $Post_t$ equals one from August 2018 onwards. All regressions include state × HS-4 product, HS-2 product × month-year, and state × month-year fixed effects. Inclusion of HS-2 product × month-year ensures that the treated and untreated HS-4 products are compared with each other within a given HS-2 over time. These regressions include the set of state × product combinations for which domestic sales data is available for all 24 months from April 2018 to March 2020. Each column reports the effective number of observations after incorporating the included fixed effects. The standard errors are clustered at HS-4 product level. ***, **, * show significance at 1%, 5% and 10% levels respectively. Figures in parenthesis are standard errors.
*Source:* State × Product level E-Way Bills data (April 2018 - March 2020).