Demonetization and Firm Exports*

Ritam Chaurey† Johns Hopkins University

Ryan Kim‡ Johns Hopkins University

Pravin Krishna§ Johns Hopkins University and NBER

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Abstract

On November 8, 2016, the Government of India announced the "demonetization" of large currency bills that rendered 86 percent of currency in circulation illegal tender. Using unique, high-frequency customs transaction data matched with balance sheet information of exporting firms and both event-study design and difference-in-differences specifications, we estimate the causal effects of the demonetization on firm exports. We measure exposure to demonetization across firms using differences in expected cash flow for firms' working capital needs using pre-demonetization ratio of accounts receivables to sales. We find that firms substantially reduced their export values immediately following demonetization but recovered their exports over time. We distinguish between real and nominal effects by focusing on export prices and quantities and find that export quantities drive almost all of the short-run reductions in export values. Furthermore, we find that affected firms (also in the short run) reduced the number of product lines and product destinations for their exports and material expenses, employee compensation, and inventory stock. Our results suggest that the shortage of liquidity had a significant effect on export quantities in the short run but that the economy recovered rapidly, thus allaying concerns that the temporary shock would lead to persistent supply disruptions.

Keywords: demonetization, exports, customs-transactions data, monetary policy

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†rchaurey@jhu.edu

‡rkim59@jhu.edu

§pravin_krishna@jhu.edu

1 Introduction

This paper studies the impact of liquidity shocks on export performance using a unique monetary policy episode – the "demonetization" of large currency bills suddenly announced by the Government of India on November 8, 2016, which suddenly rendered 86 percent of currency in circulation illegal tender within hours of the announcement. While the government began the process of re-monetizing the economy immediately, it took several months for this process to be complete, resulting in an interim economic environment characterized by a widespread cash shortage and considerable uncertainty about when the liquidity problems would be resolved. It was widely feared that the demonetization would impact firms negatively, both in the short run during re-monetization and in the long run if fragile supply chains linkages meant that work stoppages, loss of output, and firm bankruptcies in one part of the economy had cascading effects throughout the whole system. The effects of demonetization were also expected to be heterogeneous; depending on liquidity needs, firms faced varying levels of exposure to the policy shock.

We explore the short- and long-run effects of demonetization on exporting firms. To what extent did the currency shock affect firm-level exports in the short run? Did the effects persist after the economy was fully re-monetized? To investigate the immediate impact of demonetization and the resilience of exporting firms (and underlying production networks) in the longer term, we use unique (and previously unutilized by researchers) data sources for the first time. Specifically, we use customs transactions-level data from the years 2015-2017. These data include fine-grain detail on individual export transactions in combination with balance sheet information on the exporting firms. We use these data to explore the impact of demonetization on exporting firms with varying levels of exposure to the policy shock.

Exploiting the detailed data and the quasi-experimental conditions generated by sudden demonetization policy implementation, we use both event-study designs and difference-in-difference specifications to estimate the causal effect of demonetization on firm exports. We use the pre-demonetization (2013-15) average ratio of accounts receivable to sales (AR/S) as a measure of exposure to demonetization.[1] The underlying rationale is that firms that had high accounts receivable to sales were expected to receive more cash for their working capital needs and other transactions and, consequently, were more affected when large currency bills in circulation became illegal tender after the demonetization announcement. In both designs, we compare the evolution of outcomes for firms with varying levels of exposure to demonetization before and after the policy. We control for firm fixed effects in our empirical specifications, meaning that we essentially compare the evolution of within-firm changes in outcomes before and after demonetization. We also control for the industry-by-time fixed effects to absorb time-varying characteristics at the industry level and for observed

[1] For a comprehensive, empirically-based, discussion of trade credit, see the seminal paper by Petersen and Rajan (1997).

initial firm characteristics, such as firm age, cash holdings, interest payments, and bank borrowings. The November 8, 2016, demonetization announcement was sudden and completely unexpected by firms, thereby allaying concerns of anticipatory responses by firms (which would violate necessary assumptions about parallel pre-trends). Furthermore, using unique customs transaction data, we visually depict the month-by-month evolution of outcomes of interest both before and after the policy. We confirm that the parallel pre-trends assumption underlying both of our estimation strategies is empirically satisfied.

The high-frequency export data reveal a short-lived, negative effect of demonetization on firm-level exports. Firms with 10 percentage points higher accounts receivable to sales saw exports decline by 4 percent relative to their counterparts immediately in November 2016, after the implementation of the demonetization policy. This negative effect on exports was short-lived and mitigated over time, and by December 2017, there were no statistically significant differences across differentially exposed firms. The immediate fall in firm exports and the eventual mitigation of the negative effect after demonetization are robust across different specifications.

The observed negative effect of demonetization on exports is real, not nominal: the effect on export values is a result of a decline in export quantities, not prices, and largely arises from the reduction in the number of product lines and export destinations. We decompose the fall in exports into real and nominal effects – price versus quantity – to shed light on the mechanisms underlying the effect on exports. The short-term effect of demonetization on firm exports entirely arises from export *quantity* rather than export *prices*. Export quantities declined immediately following the announcement in November 2016 but recovered by December 2017, whereas the effect on export prices are largely muted, both before and after the episode. This distinction between price and quantity effects speaks to the difference between monetary neutrality and monetary non-neutrality. Next, we use the detailed customs data to focus on related effects on two extensive margins – the number of product lines and export destinations. We find a decline across both these margins for more affected firms following demonetization.

We corroborate our main results by showing negative effects of demonetization on other *annual*, firm-level, real and financial variables, suggesting that affected firms decrease production associated with exports. Consistent with our main findings, we find that affected firms differentially decrease their material expenses, compensation to employees, and stock of inventories. These results are in line with the notion that the firms that were more exposed to demonetization were unable to finance their labor expenses to a greater extent. Furthermore, their material input expenditures were differentially lowered either because of financial constraints or, alternately, due to a disruption in their upstream supply chains. This consequently lowered their production, inventories, and volume of exports. We also find that affected firms decreased their trade credit, bank borrowings, and interest expenses, suggesting that more-exposed firms decrease credit demand associated with lower export

production.

In sum, we find that the demonetization episode resulted in a short-lived decline in exports, driven by reduced export quantities, number of products, and number of destinations, mostly due to the inability to finance their input expenses. There are several implications of our findings. First, they demonstrate the non-neutrality of money – perhaps unsurprisingly given the empirical relevance of cash-in-advance constraints in the Indian economy. Second, our results suggest that informal credit mechanisms such as those that may have rapidly emerged following the announcement of demonetization were ineffective in smoothing the effects of the policy, at least in the short run. Finally, and most importantly, the short-lived effects of the very significant policy shock (which affected over 85 percent of the currency in circulation) suggest a significant resilience of production and production supply chains against credit shocks – which is especially noteworthy in this developing country setting where credit constraints are understood to be pervasive and where, indeed, there was considerable public concern expressed over the long run effects of the policy shock due to firm bankruptcies and closures.

To the best of our knowledge, this paper is the first to investigate the causal effect of a monetary policy shock—the demonetization episode—on real exports with a quasi-experimental setup and granular micro-level data.[2] Traditional international monetary theory has focused on household responses to monetary policy without cash in the economy (Gali and Monacelli, 2005), similar to the canonical closed-economy New Keynesian monetary model (Woodford, 2003).[3] In studying the international implications, most previous analyses have focused on international capital flows, bank behavior, and exchange rate movement. See for example, Cetorelli and Goldberg (2012); Bruno and Shin (2015); Kalemli-Özcan (2019); Brauning and Ivashina (2020); Miranda-Agrippino and Rey (2020); Schmitt-Grohé and Uribe (2022) for some recent analyses and Buch et al. (2019); Buch and Goldberg (2020); Kalemli-Özcan and Kwak (2020) for a survey of this literature. Our analyses showcase the importance of integrating the role of cash and inter-firm trade credit in understanding the monetary policy in an open economy. In the closed-economy setup, previous papers have studied the real effects of monetary policy on investment through different firm characteristics in developed economies, such as size (Gertler and Gilchrist, 1994), age (Cloyne et al., 2018), liquidity (Jeenas, 2019), and default risk (Ottonello and Winberry, 2020). In a quasi-experimental setup, our analyses show that the availability of cash (or the lack thereof) and its effect through the intra-firm trade credit (which is a critical financing option in developing countries (Allen et al., 2012, 2013)) lead to a temporary yet strong effect on firm exports.

This paper is also closely related to a growing literature studying the intersection of international

[2]See Ramey (2016) for the challenges in empirically identifying the real effect of monetary policy using a time-series approach due to the systematic changes in the monetary policy.

[3]Relatedly, Lagos and Zhang (2022) theoretically show the limitation of moneyless approach even in highly developed credit economies when there is imperfect competition in the credit market.

trade and corporate finance (see Foley and Manova (2015) for a survey). Previous papers have shown the importance of bank credit (e.g., Amiti and Weinstein 2011; Feenstra et al. 2014; Paravisini et al. 2014; Niepmann and Schmidt-Eisenlohr 2017; Bruno and Shin 2020), and external credit in general (Manova 2012), in explaining firm exports. Our paper highlights that, in addition to bank credit, the availability of cash in the economy is crucial for firms to finance inputs relevant to their exports in a cash-reliant economy like India. In studying the link between trade and finance, most existing literature has focused on financing frictions due to the slow and risky inflow of export revenues (e.g., Schmidt-Eisenlohr 2013; Antràs and Foley 2015; Ahn 2020). This paper shows that the unavailability of domestic currency, which is not directly related to export revenues, also affects international trade.

Our empirical analyses rely on the sudden nature of the demonetization episode and show its effect through the trade credit. Past studies have looked at the effects on district-level economic activity (Chodorow-Reich et al., 2019; Chanda and Cook, 2022), electoral outcomes (Bhavnani and Copelovitch, 2018; Khanna and Mukherjee, 2020), domestic agricultural trade (Aggarwal and Narayanan, 2022), digital technology adoption (Crouzet et al., 2019; Aggarwal et al., 2020), household consumption (Karmakar and Narayanan, 2020), and firm-level labor and material shares (Subramaniam, 2020).[4] In contrast, we bring in novel Custom-Prowess combined data and show the international implications of the demonetization through the inter-firm trade credit. In understanding the spillovers and propagation of the shock, numerous papers have highlighted the importance of financial frictions in production networks (Kim and Shin, 2012; Kalemli-Özcan et al., 2014; Bigio and La'O, 2020), and trade credit specifically (Jacobson and von Schedvin, 2015; Costello, 2020; Luo, 2020; Reisher, 2020; Altinoglu, 2021). Our work shows that firms that fail to receive trade credit decrease their real exports due to their inability to finance associated input expenditures.

2 Demonetization

On November 8, 2016, the Prime Minister of India, Narendra Modi, announced that the government was, with immediate effect, demonetizing "high denomination" currency notes of ₹ 500 or ₹ 1,000 (INR). These notes were immediately invalid as legal tender, but holders of the demonetized currency notes were given until December 31, 2016, to deposit their demonetized notes in their bank accounts and/or exchange demonetized currency for new notes (issued in denominations of ₹ 500 and ₹ 2,000). The rationale offered by the government for this move was that demonetization would allow the state to invalidate both undeclared income and wealth held in cash and counterfeit currency in circulation.

Since demonetized currency notes accounted for over 85 percent of currency in circulation, the implementation of the policy posed enormous logistical challenges. As Lahiri (2020) comprehensively documents, "automatic teller machines ran out of cash for long periods of time across the length and

[4] See Velde (2009) for the study of monetary non-neutrality in a similar quasi-experimental setup in France during the 18th Century.

breadth of the country including the major metropolitan cities." Further, when ATMs were supplied with new currency, it was initially mostly in the form of ₹ 2,000 denomination bills, "which was not helpful for daily transactions whose average cash value tended to be much smaller." The process of re-monetizing the economy was not helped by the fact that, in the subsequent days and weeks, the government continuously revised the conditions under which deposits of the older currency could be made, changing both the criteria for deposits of old currency and the daily limits on withdrawals of the new currency.

By the end of the first quarter of 2017, the RBI reported that nearly all of the demonetized notes had been returned and the re-monetization process was essentially complete; the main monetary aggregates (M1 and M2) were at about the same levels as they were pre-demonetization. However, re-monetizing the economy with the new currency bills proved to be slow and disruptive. In addition to the constraints faced by individuals and households in conducting daily transactions, it was widely reported that firms faced substantial challenges in their ability to pay their suppliers and workers and that demonetization had "chilled" the economy, causing significant supply chain disruptions to small-, medium-, and even large-scale enterprises (Singh 2016).

3 Data

The data combine detailed export information from the Indian Customs data with various firm-level balance sheet information available in the Prowess data. The final combined sample features approximately 4,000 firms with export information at the firm-month level and other annual financial and real firm characteristics, such as accounts receivable, bank borrowings, age, total sales, and employee compensation.

The Indian export data come from Indian Customs, which is made available by Cybex Exim Solutions. The data includes the monthly value and volume of exports by firm, destination, 8-digit HS code, and unit (e.g., Kgs, Pcs, etc.). The sample begins in 2015 and ends in 2017, covering the months before, during, and after the demonetization episode of November 2016. The export information is collected from more than 140 Indian ports and customs stations, including small Inland Container Depots (ICDS), Land Customs Stations (LCS), Sea Ports, and Air Ports. Firms in the Custom data are identified by Importer-Exporter Code (IEC), a mandatory identification number for any entity exporting from India. After cleaning and aggregating total export values by year, our export data cover approximately 70% of total exports reported in the Trademap sector-level data on average.

There are two notable advantages to using highly detailed customs data to study the effect of demonetization on exports. First, the data document high-frequency (monthly), firm-level exports. This feature is essential for our study but is rarely available in a typical annual firm-level database.

This aspect of the data allows us to distinguish the short- and long-run economic effects, getting at the heart of the debates on the Indian demonetization policy and previous literature studying the real impacts of monetary policy. Furthermore, it allows us to focus on a narrow window around the demonetization episode and to exploit the sudden nature of the demonetization episode in our identification strategy. Second, the data separately record the price and quantity of exports and document the number of destinations and products that each firm exports at a given point in time. This information helps uncover the underlying mechanism behind the reduced-form relationship between exports and demonetization, revealing how differentially exposed firms change their exports.

We combine exporting firm information with Prowess data, which is collected by the Centre for Monitoring Indian Economy Pvt. Ltd (CMIE). The data has annual balance sheet information for both listed and unlisted firms in India starting from 1989. The data coverage has increased over time and includes approximately 30,000 firms annually from 2013 to 2017, the period of analysis for our study. Leveraging this information, we study the demonetization effects on firm production and test the robustness of the results to control for pre-demonetization firm characteristics and primary industry code. We use a fuzzy matching algorithm with the firm name and location information and manually check the observations that cannot be matched precisely. (See Appendix A for more details on the merging procedure, data cleaning, and summary statistics.)

4 Empirical Strategy

To analyze the effect of demonetization on firm-level outcomes, we estimate event-study specifications of the form

$$y_{ijt} = \beta_0 + \sum_{t=-11}^{t=-2} \beta_t \left[\frac{AR_{(2013-15)}}{sales_{(2013-15)}}\right]_i + \sum_{t=0}^{t=13} \beta_t \left[\frac{AR_{(2013-15)}}{sales_{(2013-15)}}\right]_i + \lambda_i + \delta_{jt} + \epsilon_{ijt}, \tag{4.1}$$

where y_{ijt} is the outcome of interest for firm i in industry j in month t. $\left[\frac{AR_{(2013-15)}}{sales_{(2013-15)}}\right]_i$ is the *pre-demonitization* mean ratio of accounts receivable to sales between 2013-15 and is a measure of firm-level exposure to demonetization. λ_i denotes firm fixed effects that control for all time-invariant firm characteristics, and δ_{jt} denotes industry-by-year fixed effects that control for time-varying characteristics at the industry level. Finally, ϵ_{ijt} denotes the idiosyncratic error term. We cluster standard errors at the firm level. We normalize $t = 0$ to be the month (November 2016) in which the Government of India made the announcement demonetizing ₹ 500 and ₹ 1000 notes. Note that $t = -1$ is the omitted base period (October 2016, i.e., one month prior to demonetization). β_ts are the coefficients of interest and capture the differential outcomes for firms with different levels of the pre-demonetization ratio of accounts receivable to sales for each month relative to the base period. Furthermore, the inclusion of firm fixed effects means that we estimate within-firm changes

over time for firms with different levels of exposure to demonetization. This event-study design specification has two distinct advantages in our setting. First, the month-by-month coefficients (β_t) depict the dynamic evolution over time for our outcomes of interest and are of policy relevance themselves. Second, the coefficients for the months preceding demonetization help us test for the parallel pre-trends assumption central to estimating our difference-in-differences specification.

Specifically, we estimate difference-in-differences specifications of the form

$$y_{ijt} = \beta_0 + \beta_1 \left[\frac{AR_{(2013-15)}}{sales_{(2013-15)}} \right]_i \times post_t + X_{i(2013-15)} \times post_t + \lambda_i + \delta_{jt} + \epsilon_{ijt}, \tag{4.2}$$

where all the terms are similar to equation (4.1). Some outcome variables of interest are measured at an annual level, and for those regression specifications t denotes the year of observation. $post_t$ is a dummy variable that equals 1 for all months/years after demonetization (November 2016) and 0 for months preceding the event. $X_{i(2013-15)}$ consists of a set of pre-demonetization firm-level control variables (age, bank borrowing, cash holding, interest expense, total assets), measured as averages over 2013-15, interacted with $post_t$. In contrast to the event-study specification, the difference-in-differences specification provides a static estimate of the average treatment effect, captured by the coefficient β_1, on the interaction of $\left[\frac{AR_{(2013-15)}}{sales_{(2013-15)}} \right]_i$ and $post_t$. β_1 measures the average within-firm differences in outcomes for firms with different levels of exposure to demonetization before and after November 2016.

The identification strategy for equations (4.1) and (4.2) requires two key assumptions. First, exporters should not have anticipated the demonetization announcement and changed their behavior beforehand. As discussed in Section 2, the demonetization episode was indeed unexpected, as it was intended to remove undeclared wealth and counterfeit currency by suddenly invalidating the relevant currency notes. It is widely recognized that the policy was a surprise to economic entities in India (see, e.g., Lahiri 2020), consistent with the pre-trend results for various outcome variables based on equation (4.1). Second, exporters that initially had high ratios of accounts receivable to sales must not be differentially affected by other events (if any) that happened simultaneously. Focusing on the narrow window around the demonetization partially eases this concern. Other macroeconomic events at this precise moment (November 2016) are unlikely to be correlated with the exporter's initial ratio of accounts receivable to sales and exports simultaneously. In fact, controlling for important pre-demonetization characteristics that are likely to be relevant for both initial trade credit and exports—such as firm age, size, and corporate financing options, as well as industry fixed effects —does not make meaningful changes in the estimated coefficients. In addition, in the robustness exercise in Appendix C.4, we use the Redding and Weinstein (2019) taste corrected price index to account for the differential change in product demand across firms, and we find that demand shocks are unlikely to bias the estimated coefficients.

Moreover, we show that our results are robust to using the district-level demonetization shock as our treatment variable (following Chodorow-Reich et al. (2019)) or in a triple-difference specification (Appendix C.7). Our results consistently show the negative effect of demonetization on exports.

5 Results

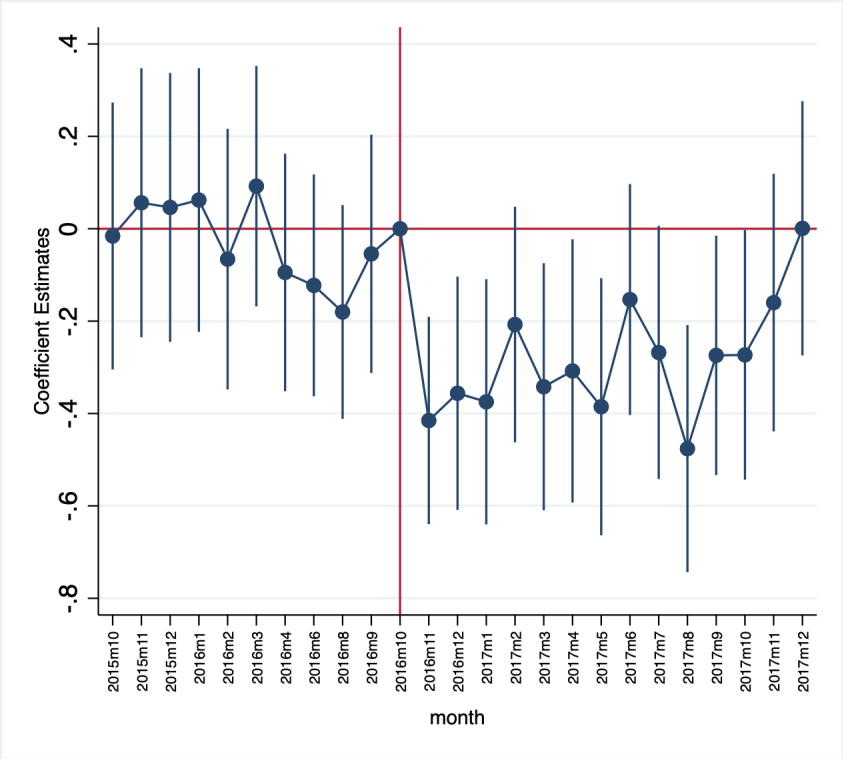


Figure 1: Exports and Demonetization: Event Study Plot

Note. Figure 1 plots the month-by-month coefficients (β_t) in Equation (4.1). Testing the null hypothesis of all pre-demonetization coefficients jointly equalling zero leads to the p-value of 0.86. A 90% confidence interval is reported for each estimated coefficient, and standard errors are clustered by firm.

Figure 1 visually depicts a strong but short-lived negative effect of the demonetization episode on firm exports. We plot the estimated month-by-month coefficients (β_t) based on equation (4.1). Before the demonetization episode, firms that initially had higher accounts receivable-to-sales (AR/S) had similar trends of export sales relative to their counterparts; the estimated coefficients are statistically indistinguishable from zero. However, in November 2016—when the demonetization policy was enacted—firms with 10 percentage points higher baseline AR/S saw a 4% decrease in exports relative to their counterparts.[5] This effect on exports gradually attenuated over time and was fully eliminated by December 2017, suggesting that the effect only lasted a little over a year.

[5]The mean and standard deviation of AR/S are .22 and .17, respectively, as shown in Table A.1.

	(1)	(2)	(3)	(4)	(5)	(6)
	Exports					
Post x AR/$S_{i,t-1}$	-0.312***	-0.313***	-0.317***	-0.321***		
	(0.105)	(0.112)	(0.112)	(0.112)		
2016 M11-12 x AR/$S_{i,t-1}$					-0.399***	-0.399***
					(0.141)	(0.141)
2017 Q1 x AR/$S_{i,t-1}$					-0.341**	
					(0.138)	
2017 Q2 x AR/$S_{i,t-1}$					-0.385***	
					(0.143)	
2017 Q3 x AR/$S_{i,t-1}$					-0.383***	
					(0.140)	
2017 Q4 x AR/$S_{i,t-1}$					-0.103	
					(0.144)	
2017 M1-2 x AR/$S_{i,t-1}$						-0.341**
						(0.145)
2017 M3-4 x AR/$S_{i,t-1}$						-0.368**
						(0.156)
2017 M5-6 x AR/$S_{i,t-1}$						-0.380***
						(0.143)
2017 M7-8 x AR/$S_{i,t-1}$						-0.468***
						(0.148)
2017 M9-10 x AR/$S_{i,t-1}$						-0.234
						(0.159)
2017 M11-12 x AR/$S_{i,t-1}$						-0.020
						(0.155)
Post x Age_i			-0.050*	-0.059**	-0.059**	-0.059**
			(0.028)	(0.029)	(0.029)	(0.029)
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
NIC4 x Post FE		✓	✓	✓	✓	✓
Other Firm Controls x Post FE				✓	✓	✓
Number of Firms	4,029	4,029	4,029	4,029	4,029	4,029
R^2	0.762	0.765	0.765	0.765	0.765	0.765
Observations	103783	103783	103783	103783	103783	103783

Table 1: Exports and Demonetization: Difference-in-Differences

Note. The dependent variable is the log value of exports at firm-month level, AR/S is 2013-2015 log mean ratio of accounts receivable to deflated sales, and *Age* is log firm age. '$Y'M$'$i-j$' is an indicator variable that equals 1 for year Y month $i-j$ (e.g., 2016 M11-12 is 1 in November and December of 2016) and is 0 otherwise. Similarly, '$Y'Q$'q' is an indicator variable that equals 1 for year Y quarter q (e.g., 2017 Q1 is an indicator variable that equals 1 in quarter 1 of 2017) and 0 otherwise. Other firm controls are 2013-2015 log mean bank borrowing, cash holdings, interest expenses, and total assets. The sample covers 2015-2017. Standard errors are clustered at the firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Columns (1)-(4) of table 1 confirm the negative effect of demonetization on firm exports using the standard difference-in-differences (DID) specification presented in equation (4.2). Column (1)

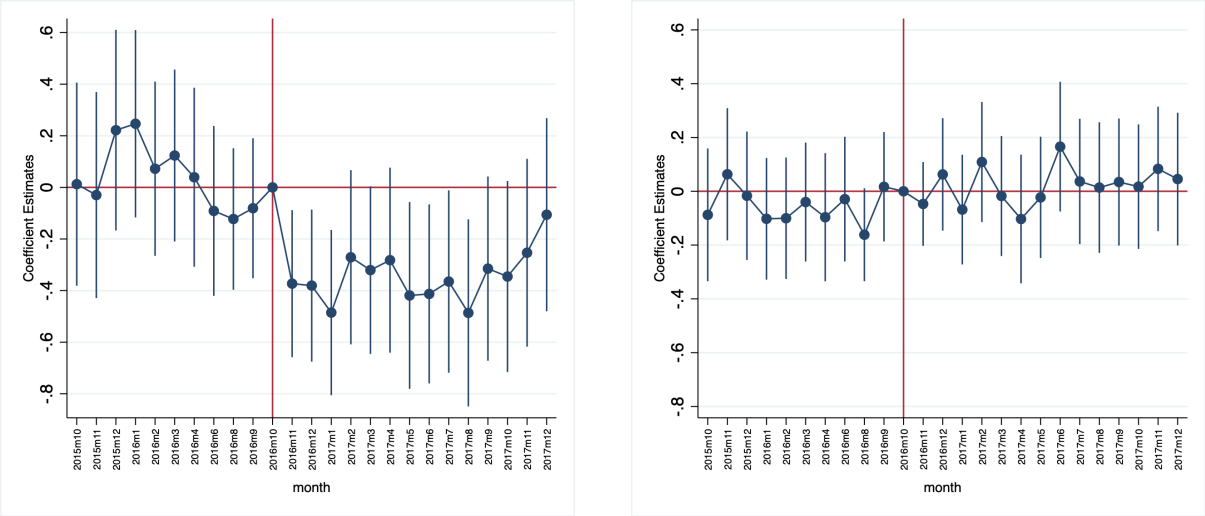
presents a baseline (DID) estimation that includes firm and month fixed effects, showing that firms with 10 percentage points higher baseline AR/S decrease their exports by 3.1 percent on average relative to the pre-demonetization period. Columns (2)-(6) show that the effect of demonetization on firm-level exports is stable across different regression specifications. Column (2) includes the interaction of the $post_t$ dummy with industry fixed effects (at the NIC 4-digit level) to compare the differential change in exports within firms in the same sector. Following recent studies that emphasize the role of firm age and its interaction with monetary policy (e.g., Haltiwanger et al. (2013); Cloyne et al. (2018)), in column (3) we further control for firm age in conjunction with firm size (sales) that was used to normalize accounts receivable. Column (4) adds additional pre-demonetization control variables, such as bank borrowings, interest payments, and cash holdings that capture alternate corporate financing options and total assets that normalize these variables. Table A.2 shows the estimated coefficients associated with the controls. After controlling for industry fixed effects, firm age, and other financing options, the negative effect of demonetization on exports remains strong. Note that older firms' exports are more sensitive to the demonetization episode, potentially because these firms are more reliant on cash than young firms.

Columns (5) and (6) confirm the temporary effect of demonetization on exports presented in Figure 1 by sub-categorizing the post period. Specifically, we replace the $Post_t$ dummy variable in equation (4.2) with an indicator variable that equals 1 for November and December 2016 and 0 otherwise to capture the short-run effect of demonetization. To further investigate the persistence of the negative effect, we include AR/S interacted with 2017 quarterly and bi-monthly dummy variables in columns (5) and (6), respectively. Our results are robust to these specifications, and the effect of demonetization on exports in the last two months of 2016 is approximately 3.9% larger for firms with 10 percentage points higher AR/S. However, the differential effect lasts for approximately one year and attenuates to zero at the end of 2017. When considered together with the results presented in figure 1, table 1 highlights the strong but temporary nature of the demonetization shock on firm exports.

Quantity vs. Price. Our primary analyses so far have focused on export sales, which is consistent with leading papers studying firm exports with firm-level data (e.g., Amiti and Weinstein (2011); Liu and Lu (2015); Barrows and Ollivier (2021)). However, the detailed custom-firm matched data allow us to further decompose firm export sales into quantity and price separately to disentangle the real and nominal effects of demonetization on exports. For simplicity, we construct and use a conventional chain-weighted Tornqvist price index at the firm level and calculate the associated quantity index by dividing the export value by the price index. Using the variety- and quality-adjusted price index following Redding and Weinstein (2019) does not change the results.[6]

By distinguishing between price and quantity effects, we are able to speak to the mechanisms

[6]See Appendix B for the construction of the price indices and Appendix C.4 for the robustness exercises.



(a) Quantity **(b)** Price

Figure 2: Exports and Demonetization: Quantity and Price

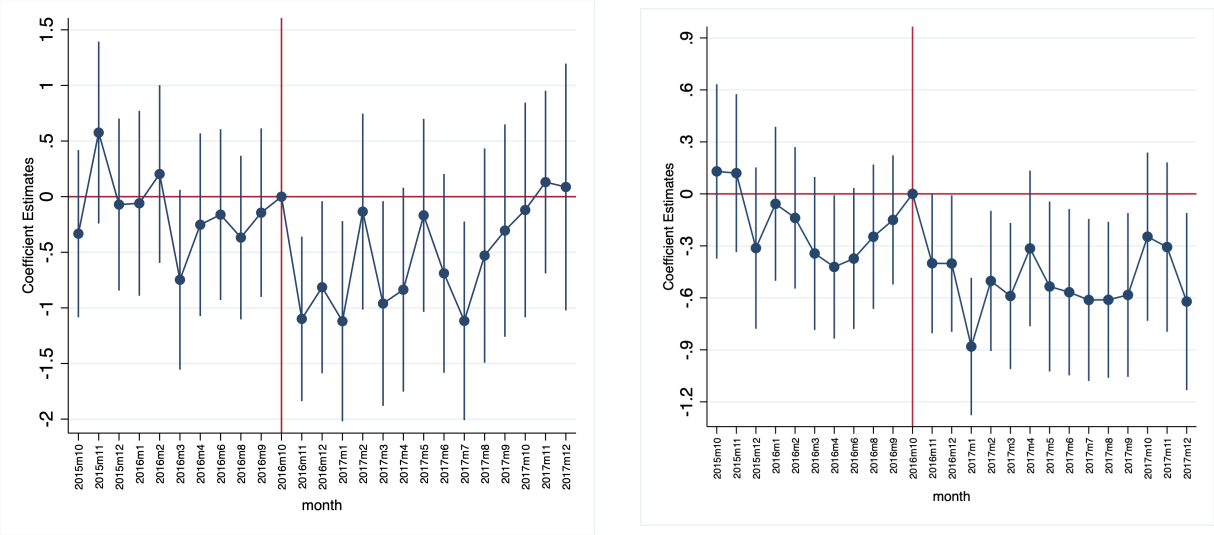
Note. Figure 2 plots the month-by-month coefficients (β_t) in equation (4.1) using log export quantity and price indices as dependent variables. The chain-weighted firm-level Tornqvist quantity and price index are used in this figure, as discussed in Appendix B. Testing the null hypothesis of all pre-demonetization coefficients jointly equalling zero leads to p-values of 0.8 and 0.73 for quantity and price, respectively. A 90% confidence interval is reported for each estimated coefficient, and standard errors are clustered at the firm level.

underlying the reduced-form effect of demonetization on export values. In essence, our analysis helps distinguish monetary neutrality versus monetary non-neutrality. On the one hand, firms with high ratios of accounts receivable to sales may be affected as cash flows dry up following demonetization, leading to lower production and volume of exports. On the other hand, affected firms may be forced to raise export prices due to the increased financial costs (e.g., Ahn et al. (2011); Haddad et al. (2011)), consequently losing their international market share. In this case, export prices rise yet value declines because of the decrease in quantity. Both of these cases would imply a short-run, real effect in terms of a quantity decline and confirm monetary non-neutrality arising from financial frictions at the firm level. Alternatively, money may be neutral and have no differential effect on real exports, leading to a reduction in nominal export values but not quantities.

Figure 2 presents the real and short-lived effect of demonetization on firm exports. The short-term demonetization effect on firm exports mainly arises from export quantity rather than export prices. The demonetization effects on export quantity shown in figure 2a closely follow the total effects on exports in figure 1. The demonetization effects on export price, however, are not significantly different from zero both in the short- and long-run, as shown in figure 2b. The estimated coefficients in the pre-demonetization period are not statistically different from zero for both quantity and price. Note that using a difference-in-differences specification (as in table 1) still shows the dominant role of an export

quantity, as presented in table A.5. Taken together, our results strongly reject the neutrality of money.

Extensive Margins. Another key advantage of using custom-firm matched data is the ability to assess the extensive margin, such as the number of products firms supply and the number of foreign markets firms serve. These variables are useful to test the real effect of demonetization. Exploiting this information, we investigate whether firms adjust their extensive margins with respect to the sudden removal of cash in the economy. We use the most granular product category available in the customs data (8-digit HS code) as a baseline analysis but find similar results using broader product categories (6-digit HS code or 4-digit HS code), as shown in Appendix C.5. Appendix C.6 highlights that the effects of demonetization on these extensive margins were as important as the effects on the intensive margins (e.g., exports per product, destination, or both).



(a) Number of Products

(b) Number of Destinations

Figure 3: Exports and Demonetization: Extensive Margins

Note. Figures 3a and 3b plot the month-by-month coefficients (β_t) in equation (4.1) using number of products and destinations as the dependent variables, respectively. The 8-digit HS code defines the product, and destination refers to the foreign country a firm exports its products to. Testing the null hypothesis of all pre-demonetization coefficients jointly equalling zero leads to p-values of 0.13 and 0.37 for the number of products and destinations, respectively. A 90% confidence interval is reported for each estimated coefficient, and standard errors are clustered at the firm level.

Figure 3 shows that more exposed exporters reduced the number of product lines and product destinations, reemphasizing the real effects of demonetization on exports. Based on figure 3a, exporters with 100 percentage points higher AR/S dropped approximately one product (8-digit HS code) after demonetization but recovered the original number of products after about a year. Given that the median exporter has three products (table A.1), the estimated coefficients point to a non-trivial effect on the extensive margin of exports. Figure 3b similarly shows that the demonetization leads to

a decrease in the number of destinations. Table A.6 shows that the results are robust to using the difference-in-differences specification.

Note that the temporary nature of demonetization is more pronounced in the number of products relative to the number of destinations. Exporters recovered their product lines after a year, but the number of export destinations remained relatively low until the end of 2017. These results are plausible because it is likely that recovering access to a new destination country is much more costly for exporters relative to producing more product lines. In order to enter a new destination country, exporters have to pay economic costs, such as transportation and tariffs, and other intangible fixed costs that allow them to overcome differences in non-economic factors, such as culture and language.

The extensive margin results provide suggestive evidence that exporters cut their expenditures associated with less profitable product lines and destinations. This empirical pattern is consistent with the idea that firms focus more on core-competence products in response to shocks. For example, Mayer et al. (2014, 2021) show that tougher competition leads exporters to focus on their best-performing products, and our analyses reveal that short-run financial frictions have a similar effect on exports. Furthermore, using the Feenstra (1994) variety correction, which considers each product's importance, we also find evidence consistent with firms continuing with their core-competence products and dropping others (results in Appendix C.4).

Suggestive mechanisms. Having established the real effect of demonetization on exports, we corroborate the underlying mechanism and the validity of the exposure variable (AR/S) by using alternative measures available in Prowess firm-level data. Although this firm-level data does not provide high-frequency information, these analyses still capture differential changes in real and financial firm-level outcomes in response to the demonetization.

Table 2 presents the effects of demonetization on various firm-level outcomes using the difference-in-differences specification in equation (4.2). Columns (1) and (2) show that firms that are more exposed to the demonetization episode decrease their material expenses and compensation to employees. These results are consistent with the notion that firms more exposed to demonetization could not finance their labor and material input expenditures and had to lower their production. Such firms also lower their inventory (column 3), potentially because these firms liquidate inventory to generate extra cash flow—which is denoted in new currency denominations that can be used after the demonetization—from the domestic market to mitigate the financing problem, consistent with Kim (2020). Column (4) confirms the negative effect on exports using the export value information available in the Prowess data. Although the Prowess data have more missing data for export values and do not record high-frequency export information, we still find a statistically significant negative effect on exports, consistent with the results presented in table 1. Finally, in columns (5) and (6), we show that affected firms lower their bank borrowings and interest expenses. These results are

	(1) Material	(2) Employee	(3) Inventories	(4) Exports	(5) Bank Borr.	(6) Interest Exp.
Post x $AR/S_{i,t-1}$	-0.193***	-0.101***	-0.184***	-0.259***	-0.146***	-0.171***
	(0.070)	(0.032)	(0.046)	(0.099)	(0.044)	(0.064)
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Number of Firms	4,528	6,011	5,601	1,765	5,215	5,778
Firm Control x Post FE	✓	✓	✓	✓	✓	✓
R^2	0.958	0.978	0.959	0.936	0.927	0.938
Observations	9056	12022	11202	3530	10430	11556

Table 2: Exports and Demonetization: Suggestive Mechanisms

Note. *Material* is log material expense, *Employee* is log compensation to employees, *Inventories* is the value of inventories, *Exports* is log firm exports, *BankBorr.* is log bank borrowings, and *InterestExp.* is the log interest expenses. AR/S is 2013-2015 log mean accounts receivable over deflated sales, and *Age* is log firm age. We control for log firm age, 2013-2015 log mean bank borrowing, cash holdings, interest expenses, and total assets. Standard errors are clustered by firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

consistent with the notion that affected firms decrease their export production and, correspondingly, decrease their credit demand from banks.

6 Conclusion

In this paper, we explore the impact of an unanticipated macroeconomic liquidity shock on the export performance of firms in India. Despite the temporary nature of the shock, the potential inability of firms in this setting to borrow (or otherwise finance themselves) against the shock raises the possibility that the policy may have persistent effects due to firm bankruptcies and closures. Supply chain network linkages in production raise the further possibility that these effects will be systemic.

We find that the effects of the policy were indeed systemic: throughout the economy, firms that were relatively heavily reliant on liquidity saw a substantial fall in exports in the period following the shock. However, our analysis of the data at a granular (customs-transactions) level suggests that these effects were only short-run. By the third quarter following the shock, the export performance effects of the shock become statistically and economically insignificant.

These findings suggest that even in developing country settings – lacking dense financial markets – firms may be able to survive temporary financial shocks and prevent long-run systemic supply chain breakdowns in the economy. The study of the exact formal or informal financial mechanisms used by these firms to ensure their survival is outside of the scope of this study but remains an important question for future research.

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Appendix A Data Description

This section describes the data cleaning, merging procedure, and summary statistics in the final sample.

Data Cleaning. We take the following steps to clean the India custom export database. First, we manually check and clean for a few misreported and inconsistent variables and duplicated observations in the customs data.[7] Second, we merge each 8-digit HS code and year available in customs data with the sector-level India export data downloaded from the Trade Map database. We generate the ratio of the aggregated sectoral export value in the customs data and the export value available in Trade Map by year and drop the sectors in the top and bottom 1% of the distribution. This filtering excludes a tiny number of observations with excessively large export values, potentially because of the misreporting/data entry errors. Third, after we aggregate export information by firm and month and combine the customs data with the Prowess data, we trim the top and bottom 1% of observations for each variable and keep firms that export for at least three consecutive months to minimize measurement error. Lastly, we exclude May and July of 2016 for the event study analyses as the mean and median export values and the number of destinations fall substantially in these months. Note that the main results are generally robust to including these months.

Combining the customs and Prowess data. After aggregating export information by firm and month, we combine the customs data with the Prowess data by using the firm name and address available in both data sets. First, we clean the company name in each database.[8] After the basic cleaning, we combine the firm names in the customs data with the "Listed Status" data downloaded from https://dgft.gov.in/sites/default/files/statusList_0.pdf. Thirty-one official firm names are duplicated within the firm identifier in the customs data (IEC). We manually check each name and pick one name that is likely to be the most informative (typically the longest name). We drop a small number of observations that have very different names within the IEC code. For both the customs and Prowess data, we further clean the firm name by using the "stnd_compname" STATA command developed by Wasi and Flaaen (2015) and check the names manually. In extracting the address information from the customs data, we only use state names to ease the merging procedure

[7] For example, "HSCODE = ########" is recorded instead of the actual HS code, Destination_Country is recorded as "GAUTEMALA" for GUATEMALA, and the same country (South Korea) is recorded as "KOREA,REPUBLIC O", "KOREA,REPUBLIC OF", and "SOUTH KOREA." The duplicated observations arise in the customs data for various reasons. For example, an error is identified by the customs officer after the shipping bill is filed (e.g., wrong export value), and the shipment is not given "LET EXPORT ORDER". The exporter then files another shipping bill with necessary changes until there is no error, leading to duplicates in the raw data.

[8] Specifically, we set space if the company name contains a comma, period, parenthesis, unknown character, the lowercase letter i (all names are in upper case), plus and minus sign, question mark, M/S, semicolon, or [MERGED]. We replace three consecutive quotes with two consecutive quotes, LIMITED to LTD, and PRIVATE to PVT. We eliminate a space at the end and at the beginning and double space.

with the Prowess data. For non-missing state variables in the customs data, we manually replace misspelled/misreported state names with formal names and double-check these names with the Prowess data. Whenever a state variable is missing, we extract state information from the city, pincode, and long address information.

In matching two databases with firm names and states, we proceed as follows. First, we identify observations that match the name and state perfectly. Second, among those unmatched observations, we use the STATA "reclink2" command developed by Wasi and Flaaen (2015) to fuzzy match the observations. We only keep the observations above the 0.99 threshold and double-check these matches by asking two research assistants to independently check the matches by searching the company names on Google and Zauba. Note that the firm is identified by the IEC code in the customs data and by the variable co_code in the Prowess data. We define a firm boundary such that there is a unique firm boundary that aggregates both IEC and co_code into one unique firm identifier.

Summary Statistics. Table A.1 reports summary statistics for the variables used in the main body of this paper. In general, there is enough heterogeneity in exports across firms and months and in trade credit across firms.

Appendix B Export Price and Quantity Index

Following Eslava and Haltiwanger (2020) and Lenzu et al. (2022), we construct the firm-time-specific chain-weighted export price index, which is defined recursively in the following way:

$$P_{ft} = P_{f,B_f} \prod_{\tau=B_f+1}^{t} \Phi_{f\tau} \tag{B.1}$$

where f denotes the firm and t denotes time (month). B_f is the first time when firm f starts to export. P_{ft} is the export price index at the firm-time level, which is the main price index we use in our analyses. P_{f,B_f} is a baseline export price index for firm f. and Φ_{ft} is the change in export price index from period $t-1$ and t for firm f. In constructing P_{ft}, we use the custom-firm combined data at the month level from January 2015 to December 2017.

Our main analyses use the conventional Tornqvist price index for simplicity, but adjusting for variety correction (Feenstra 1994) and taste correction (Redding and Weinstein 2019) do not change the main results. P_{f,B_f} is the same across all indexes, but Φ_{ft} changes with different indexes. The export-quantity index is defined as the total export value divided by the export price index.

Firm-level Baseline Export Price Index. The baseline export price index at the firm level,

Variable	Obs	Mean	Std. Dev.	P10	P50	P90
	Panel A: Exports by Firm and Month					
ln Export	160348	16.325	2.215	13.38	16.563	18.976
ln Quantity (Tornqvist)	160299	16.209	2.633	12.589	16.536	19.304
ln Price (Tornqvist)	160105	.12	1.282	-1.299	.059	1.628
Number of 8-digit HS code	161431	5.817	7.669	1	3	14
Number of 6-digit HS code	161532	5.089	6.494	1	3	12
Number of 4-digit HS code	161548	3.788	4.478	1	2	9
Number of Destinations	161513	6.407	6.915	1	4	16
	Panel B: Firm-Specific Variables					
Accounts Receivable to Sales	5960	.216	.167	.063	.181	.392
ln Age	6396	3.194	.536	2.485	3.219	3.85
ln Bank Borrowings	5093	5.195	1.92	2.676	5.279	7.583
ln Cash	5428	-.23	1.665	-2.439	-.432	2.047
ln Interest Payment	5700	3.02	2.001	.337	3.122	5.546
ln Total Assets	6110	7.016	1.704	4.889	6.995	9.242
	Panel C: Other Variables by Firm and Year					
ln Material Expenses	14079	6.099	1.951	3.477	6.264	8.469
ln Compensation to Employees	20364	4.067	1.93	1.476	4.152	6.514
ln Inventories	18576	4.732	2.19	1.831	4.886	7.392
ln Exports	8321	4.788	2.362	1.407	5.053	7.672
ln Sales	20477	6.678	1.943	4.074	6.833	9.052
ln Receivables	20105	4.911	1.95	2.289	5.075	7.317
ln Bank Borrowings	15586	5.012	2.185	2.042	5.138	7.742
ln Interest Payment	18570	2.776	2.265	-.366	2.926	5.603

Table A.1: Summary Statistics

Note. Table A.1 reports summary statistics of the variables in the combined Customs-Prowess data used in the main body of the paper. Panel A reports export information in customs data by firm and month, Panel B reports firm-specific exposure and control variables, and Panel C reports other variables reported in the Prowess data.

P_{fB_f}, is defined as follows:

$$P_{f,B_f} = P_{B_f} \prod_{p \in \Omega_{f,B_f}} \left(\frac{P_{fp,B_f}}{\bar{P}_{p,B_f}} \right)^{s_{fpB}}, \quad \bar{P}_{p,B_f} = \prod_{f} P_{fp,B_f} \tag{B.2}$$

where P_{B_f} is a baseline aggregate export price index in time B_f, P_{fp,B_f} is firm-product-level product price in time B_f, $\bar{P}_{p,B_f}$ is product-level price index in time B_f (the geometric average of P_{fp,B_f}), and s_{fpt} is the share of product p in firm f's revenue in period t. The set Ω_{f,B_f} is the collection of all products p provided by firm f in its baseline year B_f. The product is defined at the most granular level we observe in the data, which is the 8 digit HS code x unit. The 8-digit HS code reported in the India Customs data is its so-called Indian Tariff Code (ITC) number. The first six

digits are identical to the 6-digit HS code used globally, and the last two digits are added to denote more detailed product categories. For example, the 6-digit HS code "84022000 Freezers of the chest type, not exceeding 800 liters capacity" is further subdivided into the "84183010 Freezers, electrical" and "84183090 Freezers, other than electrical".

The aggregate baseline export price index, P_{B_f}, is:

$$P_{B_f} = \begin{cases} 1, & \text{if B is the first month of the sample} \\ \prod_{f'} P_{f',B-1}, & \text{if B is after the first month of the sample} \end{cases}$$

Intuitively, P_{f,B_f} is the modified version of the weighted geometric average of product-firm-specific export prices across products within the firm at time B_f, where the weight is the sales share of each product within the firm and time B_f. There are two modifications. First, it adjusts for the average product-specific export price index such that the firm-level export price index reflects what is relative to the average export price of the same product sold by other firms. Second, it combines with the aggregate export price index so that the export baseline price index for firms newly entering the market can be adjusted with the aggregate export prices.

Change in Firm-level Export Price Index: Tornqvist. Our main export price index is constructed following the conventional Tornqvist index:

$$\Phi_{ft} = \Phi_{ft}^{T} = \frac{\prod_{p \in \Omega_{ft,t-1}} (P_{fpt})^{s_{ft,t-1}}}{\prod_{p \in \Omega_{ft,t-1}} (P_{fp,t-1})^{s_{ft,t-1}}} \tag{B.3}$$

where $s_{ft,t-1} \equiv \frac{s_{f,t-1}+s_{ft}}{2}$, and $\Omega_{ft,t-1}$ is the set of products p firm f provide in both period t and $t-1$.

Change in Firm-level Export Price Index: Variety and Taste Correction. Following Redding and Weinstein (2019), Eslava and Haltiwanger (2020), and Lenzu et al. (2022), the change in price index at the firm-time level can be written as:

$$\Phi_{ft} = \Phi_{ft}^{J} \Phi_{ft}^{F} \Phi_{ft}^{RW} \tag{B.4}$$

where Φ_{ft}^{J} is an equal-weighted geometric average (a Jevons index) of the prices for all products continuing from period $t-1$ to t, Φ_{ft}^{F} is the Feenstra (1994) variety correction, and Φ_{ft}^{RW} is the Redding and Weinstein (2020) consumer taste-bias correction.

The Jevons index is defined in the following way:

$$\Phi_{ft}^{J} = \frac{\prod_{p \in \Omega_{ft,t-1}} (P_{fpt})^{\frac{1}{|\Omega_{ft,t-1}|}}}{\prod_{p \in \Omega_{ft,t-1}} (P_{fp,t-1})^{\frac{1}{|\Omega_{ft,t-1}|}}} \tag{B.5}$$

where $\Omega_{ft,t-1}$ is the set of products p firm f provides in both period t and $t-1$, and $|\Omega_{ft,t-1}|$ is the number of these continuing products provided by firm f.

The Feenstra (1994) variety correction is as follows:

$$\Phi_{ft}^{F} = \left(\frac{\sum_{p \in \Omega_{ft,t-1}} s_{fpt}}{\sum_{p \in \Omega_{ft,t-1}} s_{fp,t-1}} \right)^{\frac{1}{\sigma-1}} \tag{B.6}$$

where σ is the demand elasticity, and s_{fpt} is the share of product p in firm f's revenue at time t. This term captures the taste for variety. The intuition is that if one more variety of a product is added to the market, the share of common products must fall, leading to a smaller Φ_{ft}^{F} and price (cost of living) index. If products can be substituted easily (higher σ), this effect is lower; the variety effect is stronger if the products cannot be substituted easily.

The Redding and Weinstein (2019) taste correction is as follows:

$$\Phi_{ft}^{RW} = \left(\frac{\prod_{p \in \Omega_{ft,t-1}} \left(s_{fpt}^{*} \right)^{\frac{1}{|\Omega_{ft,t-1}|}}}{\prod_{p \in \Omega_{ft,t-1}} \left(s_{fp,t-1}^{*} \right)^{\frac{1}{|\Omega_{ft,t-1}|}}} \right)^{\frac{1}{\sigma-1}} \tag{B.7}$$

where s_{fpt}^{*} is the share of product p in firm f's revenues at time t *among all products continuing from period* $t-1$ *to* t. Thus, $\sum_{p \in \Omega_{ft,t-1}} s_{fpt}^{*} = \sum_{p \in \Omega_{ft,t-1}} s_{fp,t-1}^{*} = 1$. This term captures the utility gains from the taste shift. The intuition is as follows. If the product share is more dispersed across products within firms, Φ_{ft}^{RW} and price (cost-of-living index) fall because the geometric average of shares decreases with a higher dispersion. A more dispersed product share benefits consumers if the dispersion arises from the taste-adjusted prices. As the taste-adjusted prices are more dispersed across products within firms, households facing more dispersed prices can substitute away from high taste-adjusted price products to low taste-adjusted price products. If products can be substituted easily (higher σ), this effect is lower; the taste effect is stronger if the products cannot be substituted easily.

One practical challenge in using the price index developed by Redding and Weinstein (2019) is the unknown measure of the demand elasticity σ. For the baseline analysis, we calibrate $\sigma = 6$ and show that the results are analogous when using $\sigma = 4$ and $\sigma = 8$. These values are used to match the average import demand elasticity the Indian economy faces in exporting products. Specifically, we bring in the import demand elasticity made available by the Economic and Social Commission

for Asia and the Pacific (ESCAP). Utoktham et al. (2020) provide these elasticities by applying the estimation method developed by Feenstra (1994) and extended by Broda and Weinstein (2006) and Soderbery (2015) to the UN Commodity Trade Statistics Database (COMTRADE). The elasticities can be downloaded from https://www.unescap.org/resources/new-global-estimates-import-demand-elasticities-technical-note#. We aggregate export values in the customs data by 6-digit HS code and destination country and use this as a weight to calculate the mean and median import demand elasticity. The mean is 5.08 and the median is 6.66.

Appendix C Additional Empirical Analyses

This section presents the supplementary empirical analyses related to Figures and Tables reported in the main body of the paper.

C.1 Control Variables

Table 1 suppresses the coefficients for control variables except for firm age. This section explicitly presents the coefficient estimates associated with control variables and considers other sets of controls variables that capture corporate financing options other than trade credit.

Table A.2 shows that the estimated coefficients of the control variables are intuitive, and sequentially adding these variables only minimally changes the main estimated coefficient of the ratio of accounts receivable to sales. Columns (1) and (5) replicate columns (1) and (4) of table 1, respectively. Column (2) adds $TotalAssets$ interacted with the $Post$ dummy to control for the differential effect of demonetization on large firms and to normalize other control variables, such as cash and interest payments. The demonetization effect on exports is larger for firms with smaller total assets, potentially because larger firms rely less on cash transactions. Column (3) includes corporate financing options other than trade credit, such as cash holdings and bank borrowings. The demonetization effect is stronger for firms with high cash holdings, probably because these firms rely more on cash for their transactions. Column (4) shows that firms that had more interest payments were more negatively affected by the demonetization, potentially because these firms initially had higher financial burdens. Columns (5) and (6) consider alternative measures to control for the other financing options (cash, interest, and borrowing). The firms initially had larger cash holdings decrease their exports more during the demonetization period, consistent with the results in columns (3) and (4). Adding the interest coverage ratio and borrowing to assets ratio decreases the main coefficient on accounts receivable to sales, but these controls do not have substantial effects on exports at the conventional level of statistical significance.

	(1)	(2)	(3)	(4)	(5)	(6)
	Exports					
Post x $AR/S_{i,t-1}$	-0.312***	-0.320***	-0.328***	-0.321***	-0.330***	-0.277**
	(0.105)	(0.112)	(0.112)	(0.112)	(0.113)	(0.116)
Post x Age_i		-0.067**	-0.057*	-0.059**	-0.064**	-0.065**
		(0.030)	(0.029)	(0.029)	(0.030)	(0.030)
Post x $Total\ Assets_{i,t-1}$		0.028***	0.044***	0.061***	0.019*	0.019*
		(0.010)	(0.015)	(0.017)	(0.010)	(0.010)
Post x $Cash\ Holding_{i,t-1}$			-0.030***	-0.028***		
			(0.011)	(0.011)		
Post x $Bank\ Borrowing_{i,t-1}$			-0.003	0.030		
			(0.013)	(0.019)		
Post x $Interest\ Payment_{i,t-1}$				-0.052**		
				(0.020)		
Post x $Cash/Assets_{i,t-1}$					-6.320*	-6.360*
					(3.430)	(3.423)
Post x $Interest\ Coverage\ Ratio_{i,t-1}$						-0.000
						(0.000)
Post x $Borrowings/Assets_{i,t-1}$						-0.074
						(0.106)
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
NIC4 x Post FE		✓	✓	✓	✓	✓
Number of Firms	4,029	4,029	4,029	4,029	3,993	3,919
R^2	0.762	0.765	0.765	0.765	0.764	0.765
Observations	103783	103783	103783	103783	102910	101148

Table A.2: Exports and Demonetization: Control Variables Suppressed in Table 1

Note. Table A.2 explicitly shows the estimated coefficients associated with control variables used in table 1. $TotalAssets$ is log total assets, $CashHoldings$ is log cash holdings, $InterestPayment$ is log interest payment, and $BankBorrowing$ is log bank borrowings. $Cash/Assets$ is the cash holding to total assets ratio, $InterestCoverageRatio$ is the PBIT to Interest Payment ratio, and $Borrowings/Assets$ is the ratio of bank borrowing to total assets. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.2 Using an Indicator Variable

Our identification strategy relies on the difference-in-difference framework using account receivables to sales (AR/S) as exposure to the demonetization episode. One concern in using the AR/S is that it is a continuous variable and requires a stronger assumption to put a causal interpretation on the estimated parameter ("strong parallel trend assumption", as discussed in Callaway et al. (2021)).

As a robustness exercise, we define two different indicator variables based on the AR/S. First, we define an indicator variable equal to 1 if the AR/S is larger than 0.5 and 0 otherwise. Second,

Cutoff:	Exports					
	$AR/S_{i,t-1}$=0.5			p75 and p25 of $AR/S_{i,t-1}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Post x $D_{i,t-1}$	-0.159**			-0.109**		
	(0.077)			(0.051)		
2016 M11-12 x $D_{i,t-1}$		-0.154*	-0.153*		-0.151**	-0.151**
		(0.088)	(0.088)		(0.059)	(0.059)
2017 Q1 x $D_{i,t-1}$		-0.264***			-0.150**	
		(0.092)			(0.059)	
2017 Q2 x $D_{i,t-1}$		-0.223**			-0.141**	
		(0.099)			(0.059)	
2017 Q3 x $D_{i,t-1}$		-0.201**			-0.110*	
		(0.099)			(0.062)	
2017 Q4 x $D_{i,t-1}$		0.072			0.001	
		(0.112)			(0.063)	
2017 M1-2 x $D_{i,t-1}$			-0.230**			-0.186***
			(0.100)			(0.063)
2017 M3-4 x $D_{i,t-1}$			-0.251**			-0.122*
			(0.110)			(0.064)
2017 M5-6 x $D_{i,t-1}$			-0.251**			-0.129**
			(0.098)			(0.061)
2017 M7-8 x $D_{i,t-1}$			-0.259**			-0.154**
			(0.104)			(0.064)
2017 M9-10 x $D_{i,t-1}$			-0.052			-0.024
			(0.114)			(0.068)
2017 M11-12 x $D_{i,t-1}$			0.121			0.015
			(0.120)			(0.066)
Post x Age_i	-0.064**	-0.065**	-0.065**	-0.087**	-0.088**	-0.088**
	(0.029)	(0.029)	(0.029)	(0.044)	(0.044)	(0.044)
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
NIC4 x Post FE	✓	✓	✓	✓	✓	✓
Other Firm Controls x Post FE	✓	✓	✓	✓	✓	✓
Number of Firms	4,115	4,115	4,115	2,151	2,151	2,151
R^2	0.764	0.764	0.764	0.756	0.756	0.756
Observations	105480	105480	105480	52160	52160	52160

Table A.3: Exports and Demonetization: Using an Indicator Variable

Note. Table A.3 considers two different indicator variables defined based on $AR/S_{i,t-1}$. Columns (1)-(3) consider an indicator variable equals 1 if $AR/S_{i,t-1}$ is larger than 0.5 and 0 otherwise. Columns (4)-(6) consider an indicator variable equals 1 if $AR/S_{i,t-1}$ is larger than the 75th percentile of the distribution and 0 if $AR/S_{i,t-1}$ is smaller than the 25th percentile of the distribution. All other specifications are identical to table 1 columns (4)-(6). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

we consider an indicator variable equal to 1 if the AR/S is larger than the 75 percentile of the distribution and 0 if the AR/S is smaller than the 25 percentile of the distribution. Table A.3 reports the results using these two different indicator variables.

The temporary demonetization effects on exports through account receivables to sales remain strong regardless of using two different indicator variables. Column (1) shows that the firms with higher account receivables to sales decrease their exports more than their counterparts conditional on the demonetization episode, and columns (2) and (3) confirm the temporary nature of the effect, similar to the results using AR/S directly. Columns (4)-(6) consider an alternative indicator variable but still generate the same results.

C.3 Restrict Sectors

One potential concern related to our analyses is that our sample does not cover total exports by Indian firms. One related concern is that the extensive margin results we emphasized in Figure 3 may simply reflect that the sample coverage has decreased after the demonetization. To address this concern, we restrict the sample such that we have the same number of sectors in 2015-2017. We also use only those sectors that we can cover more than 80% of total exports. Since 80% is arbitrary, we also consider 85% and 75% cutoffs as robustness tests.

As shown in Table A.4, we still find that the demonetization effect on exports is robust across different subsectors available in our sample.

C.4 Price vs. Quantity

This section presents the results using the difference-in-differences estimation strategy for price and quantity and using Redding and Weinstein (2019) (RW) price index constructed as described Appendix B.

Table A.5 presents the real effect of demonetization on firm exports using the same specifications used in table 1. Columns (1)-(4) display the effect on export quantity, and columns (5)-(8) show the effect on export prices. Results using both the essential specification in columns (1) and (5) and the tightest specification in columns (2) and (6) are consistent. The demonetization effect on firm exports mainly arises from the export quantity rather than export prices. Columns (3) and (4) confirm the short-lived nature of the demonetization effect with the volume of exports. The real effect is strong within two months of the demonetization episode in 2016, but the effect disappears at the end of 2017 regardless of the frequency of indicators used. Columns (7) and (8) confirm that the demonetization effect on the export price, in general, is not significantly different from zero.

Figure A.1 replicates figure 2 using the Redding and Weinstein (2019) quantity and price indexes constructed following Appendix B. The results reported in figures A.1a and A.1b are more or less the same as what are reported in figure 2: there is a strong temporary effect on quantity, but the

	Exports							
	Balance Sectors			>80%			>85%	>75%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post x AR/$S_{i,t-1}$	-0.311***			-0.379***				
	(0.111)			(0.110)				
2016 M11-12 x AR/$S_{i,t-1}$		-0.399***	-0.399***		-0.494***	-0.494***	-0.450***	-0.464***
		(0.141)	(0.141)		(0.150)	(0.150)	(0.156)	(0.146)
2017 Q1 x AR/$S_{i,t-1}$		-0.321**			-0.401***			
		(0.137)			(0.140)			
2017 Q2 x AR/$S_{i,t-1}$		-0.370***			-0.431***			
		(0.141)			(0.146)			
2017 Q3 x AR/$S_{i,t-1}$		-0.374***			-0.417***			
		(0.138)			(0.138)			
2017 Q4 x AR/$S_{i,t-1}$		-0.103			-0.166			
		(0.143)			(0.143)			
2017 M1-2 x AR/$S_{i,t-1}$			-0.310**			-0.397***	-0.398**	-0.372**
			(0.145)			(0.146)	(0.160)	(0.153)
2017 M3-4 x AR/$S_{i,t-1}$			-0.357**			-0.423***	-0.349**	-0.422**
			(0.154)			(0.157)	(0.172)	(0.165)
2017 M5-6 x AR/$S_{i,t-1}$			-0.369***			-0.429***	-0.389**	-0.385***
			(0.142)			(0.150)	(0.155)	(0.145)
2017 M7-8 x AR/$S_{i,t-1}$			-0.452***			-0.446***	-0.382**	-0.444***
			(0.146)			(0.148)	(0.159)	(0.151)
2017 M9-10 x AR/$S_{i,t-1}$			-0.233			-0.349**	-0.245	-0.368**
			(0.157)			(0.161)	(0.170)	(0.167)
2017 M11-12 x AR/$S_{i,t-1}$			-0.024			-0.077	-0.013	-0.106
			(0.154)			(0.158)	(0.171)	(0.163)
Post x Age_i	-0.056*	-0.056*	-0.056*	-0.059*	-0.059*	-0.059*	-0.047	-0.068**
	(0.029)	(0.029)	(0.029)	(0.031)	(0.031)	(0.031)	(0.032)	(0.031)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
NIC4 x Post FE	✓	✓	✓	✓	✓	✓	✓	✓
Other Firm Controls x Post FE	✓	✓	✓	✓	✓	✓	✓	✓
Number of Firms	4,014	4,014	4,014					
R^2	0.767	0.767	0.767	0.772	0.772	0.772	0.770	0.772
Observations	103044	103044	103044	95747	95747	95747	92692	97547

Table A.4: Exports and Demonetization: Restrict Sectors

Note. The dependent variable is the value of exports at firm-month level, AR/S is 2013-2015 log mean ratio of accounts receivable to deflated sales, and *Age* is log firm age. '$Y'M$'$i-j$' is an indicator variable that equals 1 for year Y month $i-j$ (e.g., 2016 M11-12 is 1 in November and December of 2016) and is 0 otherwise. Similarly, '$Y'Q$'q' is an indicator variable that equals 1 for year Y quarter q (e.g., 2017 Q1 is an indicator variable that equals 1 in quarter 1 of 2017) and 0 otherwise. Other firm controls are 2013-2015 log mean bank borrowing, cash holdings, interest expenses, and total assets. The sample covers 2015-2017. Standard errors are clustered at the firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

effect on price is muted in the short- and long-run. Since conventional price, variety, and taste effects may move in opposite directions (such that the total price effect is muted), we separately analyze each component (added by $\ln P_{f,B_f}$, the log baseline firm-specific price index): $\ln \Phi^J_{ft}$, $\ln \Phi^F_{ft}$, and $\ln \Phi^{RW}_{ft}$. Figures A.1c, A.1d, and A.1e show that there is no significant effect on price regardless of using the conventional Jevon's price index, variety correction, or taste correction.

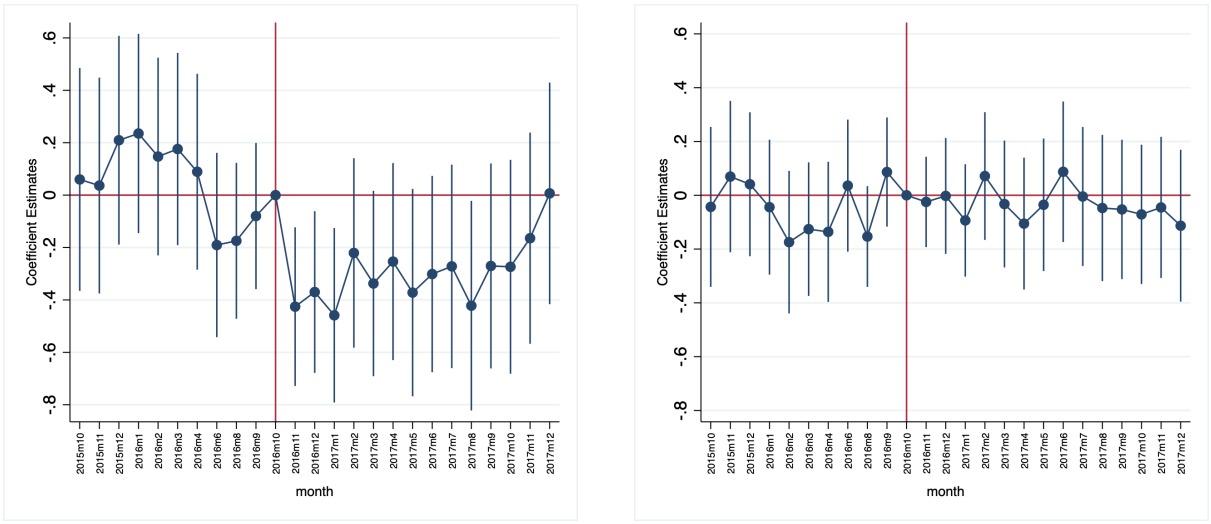
The two results using the RW price index are noteworthy. First, despite the fact that affected

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Quantity				Price			
Post x AR/$S_{i,t-1}$	-0.444***	-0.385**			0.079	0.014		
	(0.145)	(0.154)			(0.104)	(0.112)		
2016 M11-12 x AR/$S_{i,t-1}$			-0.411**	-0.411**			0.025	0.025
			(0.169)	(0.169)			(0.126)	(0.126)
2017 Q1 x AR/$S_{i,t-1}$			-0.443**				0.073	
			(0.187)				(0.132)	
2017 Q2 x AR/$S_{i,t-1}$			-0.474**				0.030	
			(0.199)				(0.138)	
2017 Q3 x AR/$S_{i,t-1}$			-0.427**				-0.024	
			(0.193)				(0.139)	
2017 Q4 x AR/$S_{i,t-1}$			-0.158				-0.037	
			(0.209)				(0.151)	
2017 M1-2 x AR/$S_{i,t-1}$				-0.497**				0.096
				(0.200)				(0.142)
2017 M3-4 x AR/$S_{i,t-1}$				-0.356*				-0.004
				(0.204)				(0.135)
2017 M5-6 x AR/$S_{i,t-1}$				-0.520**				0.061
				(0.205)				(0.141)
2017 M7-8 x AR/$S_{i,t-1}$				-0.555***				0.000
				(0.205)				(0.143)
2017 M9-10 x AR/$S_{i,t-1}$				-0.237				-0.093
				(0.212)				(0.150)
2017 M11-12 x AR/$S_{i,t-1}$				-0.076				0.003
				(0.217)				(0.160)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
NIC4 x Post FE		✓	✓	✓		✓	✓	✓
Number of Firms	4,037	4,037	4,037	4,037	4,040	4,040	4,040	4,040
Firm Control x Post FE		✓	✓	✓		✓	✓	✓
R^2	0.734	0.738	0.738	0.738	0.593	0.600	0.600	0.600
Observations	103792	103792	103792	103792	103659	103659	103659	103659

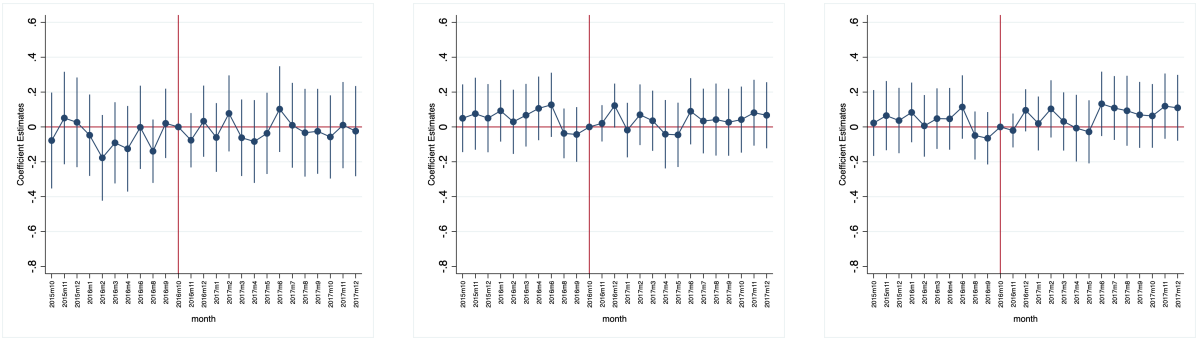
Table A.5: Exports and Demonetization: Price and Quantity, Tornqvist Index

Note. AR/S is 2013-2015 log mean accounts receivable to sales. All variables are deflated. The Tornqvist index is used for the construction of quantity and price indexes. We control for log firm age, 2013-2015 log mean bank borrowing, cash holdings, interest expenses, and total assets. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

firms reduced the number of products they export, as shown in figure 3, the Feenstra variety correction term shows no effect. This result suggests that firms temporarily drop those products that are unimportant to them, consistent with the idea of core competence: firms that are more exposed to



(a) RW Quantity Index **(b)** RW Price Index



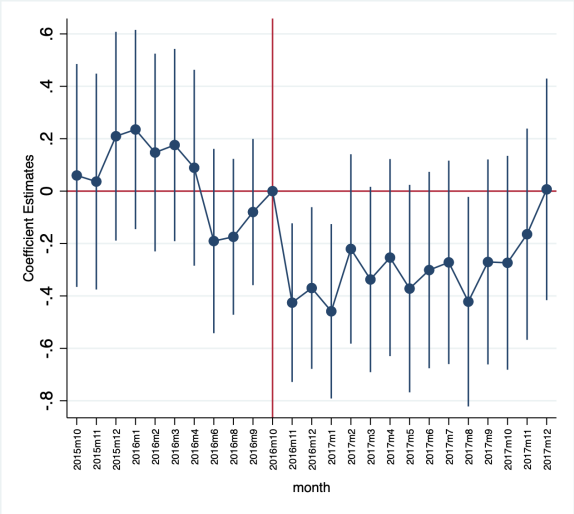
(c) Price: Jevons Index **(d)** Price: Variety Correction **(e)** Price: Taste Correction

Figure A.1: Exports and Demonetization: Price and Quantity, RW Index

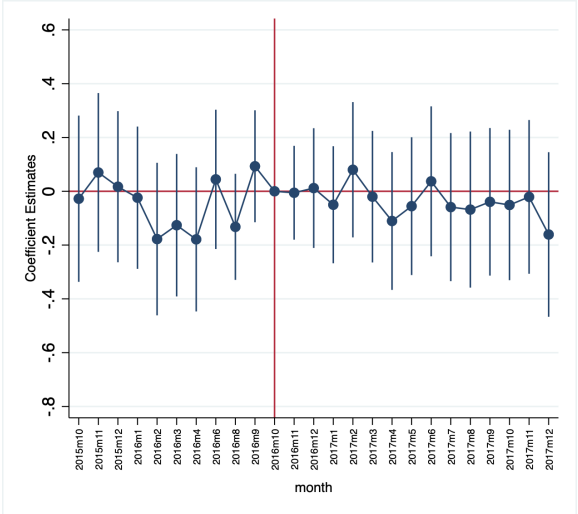
Note. Figure A.1 replicates figure 2 using the Redding and Weinstein (2019) quantity and price indexes with $\sigma = 6$, as discussed in Appendix B. Testing the null hypothesis of all the pre-demonetization coefficients jointly equalling zero leads to p-values of 0.75, 0.47, 0.68, 0.91, and 0.82 for figures A.1a-A.1e, respectively. A 90% confidence interval is reported for each estimated coefficient, and standard errors are clustered by firm.

the demonetization drop unnecessary products and focus on their most important products. Second, the results on taste correction in figure A.1e suggest that the export value effect is unlikely to be driven by the differential change in consumer demand across firms. This result is consistent with an assumption that firms with higher AR/S did not face differential demand shocks during or after the demonetization.

Figure A.2 considers alternative values for σ. The results are generally robust to using lower or higher values of σ.

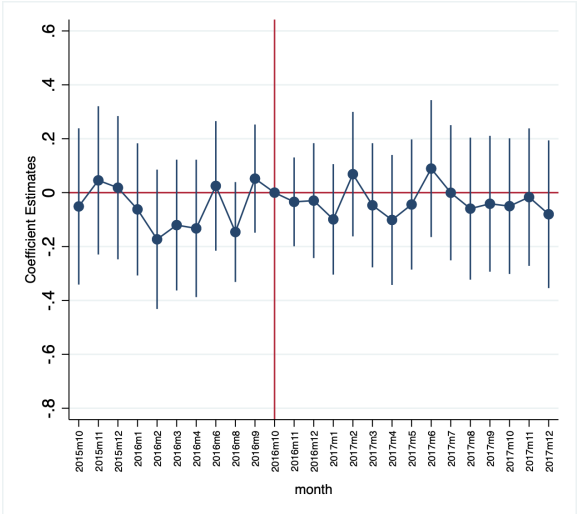


(a) RW: Quantity Index with $\sigma = 4$



(b) RW: Price Index with $\sigma = 4$

(c) RW: Quantity Index with $\sigma = 8$



(d) RW: Price Index with $\sigma = 8$

Figure A.2: Exports and Demonetization: Price and Quantity, RW Index with different σ.

Note. Figure A.2 replicates figures A.1a and A.1b using $\sigma = 4$ and $\sigma = 8$, respectively. Testing the null hypothesis of all the pre-demonetization coefficients jointly equalling zero leads to p-values of 0.8, 0.65, 0.8, and 0.55 for figures A.2a-A.2d, respectively. A 90% confidence interval is reported for each estimated coefficient, and standard errors are clustered by firm.

C.5 Extensive Margins

This section presents the results using the difference-in-differences estimation for the extensive margin, using alternative definitions of product, and the exact decomposition of the demonetization effect into intensive and extensive margin effects.

Table A.6 shows that more exposed exporters reduce their number of product lines and product

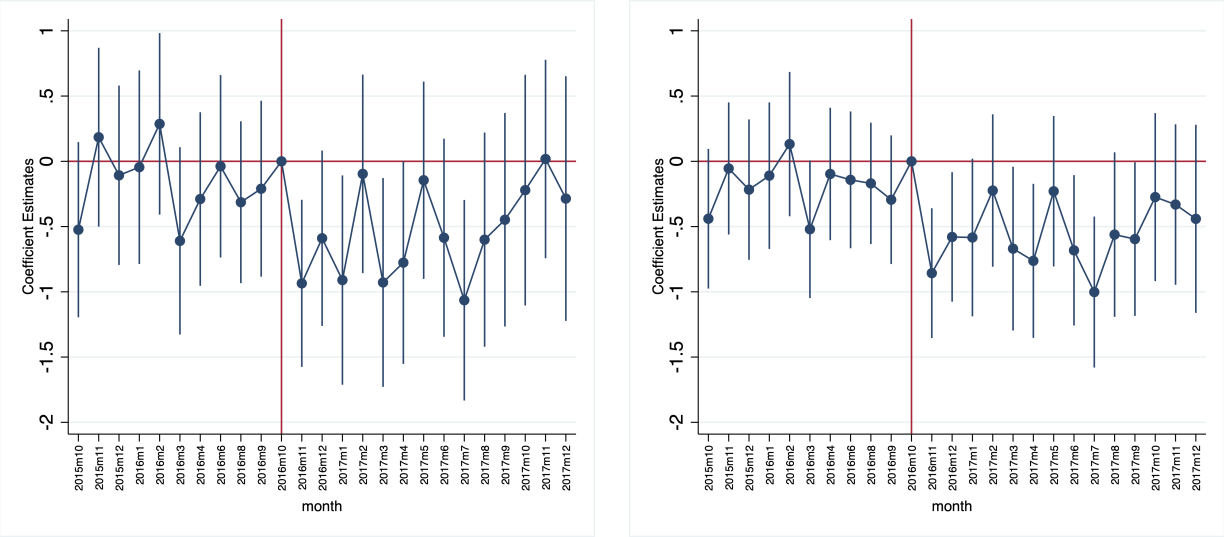
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of Products				Number of Destinations			
Post x AR/$S_{i,t-1}$	-0.735**	-0.750**			-0.747***	-0.527**		
	(0.310)	(0.330)			(0.240)	(0.254)		
2016 M11-12 x AR/$S_{i,t-1}$			-1.165***	-1.164***			-0.365	-0.365
			(0.403)	(0.402)			(0.279)	(0.279)
2017 Q1 x AR/$S_{i,t-1}$			-0.968***				-0.695**	
			(0.375)				(0.283)	
2017 Q2 x AR/$S_{i,t-1}$			-0.726*				-0.563*	
			(0.385)				(0.290)	
2017 Q3 x AR/$S_{i,t-1}$			-0.771*				-0.582*	
			(0.422)				(0.309)	
2017 Q4 x AR/$S_{i,t-1}$			-0.167				-0.373	
			(0.511)				(0.314)	
2017 M1-2 x AR/$S_{i,t-1}$				-0.919**				-0.750***
				(0.374)				(0.289)
2017 M3-4 x AR/$S_{i,t-1}$				-0.981**				-0.458
				(0.420)				(0.309)
2017 M5-6 x AR/$S_{i,t-1}$				-0.650				-0.674**
				(0.407)				(0.314)
2017 M7-8 x AR/$S_{i,t-1}$				-0.979**				-0.702**
				(0.427)				(0.309)
2017 M9-10 x AR/$S_{i,t-1}$				-0.510				-0.268
				(0.519)				(0.319)
2017 M11-12 x AR/$S_{i,t-1}$				0.105				-0.468
				(0.553)				(0.336)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓
nic4 x post FE		✓	✓	✓		✓	✓	✓
Number of Firms	4,036	4,036	4,036	4,036	4,039	4,039	4,039	4,039
Firm Control x Post FE		✓	✓	✓		✓	✓	✓
R^2	0.823	0.825	0.825	0.825	0.891	0.893	0.893	0.893
Observations	104278	104278	104278	104278	104211	104211	104211	104211

Table A.6: Exports and Demonetization: Extensive Margins

Note. AR/S is 2013-2015 log mean accounts receivable to deflated sales. Product is defined based on the 8-digit HS code, and destination is defined as the export's destination country. We control for log firm age, 2013-2015 log mean bank borrowing, cash holdings, interest expenses, and total assets. Standard errors are clustered at the firm level. * p $<$ 0.10, ** p $<$ 0.05, *** p $<$ 0.01.

destinations following demonetization, again showing the real effect of demonetization on exports. Columns (1)-(4) explore the effect on the number of products, and columns (5)-(8) show the effect on the number of destinations. As shown in columns (1), (2), (5), and (6), the demonetization leads to a larger decrease in the number of products and destinations for firms with higher accounts receivable to sales ratios regardless of inclusion (or exclusion) of control variables. Columns (3), (4), (7), and

(8) show the temporary nature of the effect.



(a) N. Products (6-digit HS) **(b)** N. Products (4-digit HS)

Figure A.3: Exports and Demonetization: Extensive Margins

Note. Figure A.3 plots the month-by-month coefficients (β_t) in equation (4.1) using the number of products as the dependent variable. Figure A.3a uses 6-digit HS codes to define products, and Figure 3b uses 4-digit HS codes. A 90% confidence interval is reported for each estimated coefficient, and standard errors are clustered by firm.

Figure A.3 shows that the results are robust to using alternative definitions of products. Instead of using an 8-digit HS code, we use a 6-digit HS code and a 4-digit HS to define products and aggregate the number of products by firm and month. Regardless of using alternative product definitions, the demonetization leads to a smaller number of products for more exposed firms.

C.6 Extensive vs. Intensive Margins

Our analyses so far emphasize the importance of the extensive margin partly because this margin shows the real effect, consistent with the effects on export quantity. This section further investigates the relative importance of the extensive and intensive margins to better understand the effect of demonetization on exports. Specifically, we decompose the value exports as follows:

$$\ln \text{Exports} = \underbrace{\ln\left(\text{Number of X}\right)}_{\text{Extensive Margin}} + \underbrace{\ln\left(\frac{\text{Exports}}{\text{Number of X}}\right)}_{\text{Intensive Margin}}$$

where X is products, destinations, and products and destinations. We regress each margin on the interaction of *Post* x AR/S along with firm and time fixed effects following equation (4.2).

Margins		Product		Destination		Product x Dest.	
	(1) Exports	(2) Ext.	(3) Int.	(4) Ext.	(5) Int.	(6) Ext.	(7) Int.
Post x $AR/S_{i,t-1}$	-0.323***	-0.143***	-0.181*	-0.115***	-0.208**	-0.197***	-0.126
	(0.111)	(0.046)	(0.098)	(0.043)	(0.088)	(0.055)	(0.084)
Firm FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
nic4 x post FE	✓	✓	✓	✓	✓	✓	✓
Number of Firms	4,000	4,000	4,000	4,000	4,000	4,000	4,000
Firm Control x Post FE	✓	✓	✓	✓	✓	✓	✓
R^2	0.757	0.784	0.714	0.833	0.667	0.824	0.675
Observations	100021	100021	100021	100021	100021	100021	100021

Table A.7: Exports and Demonetization: Extensive vs. Intensive Margins

Note. The samples are balanced across specifications such that the total effect is decomposed into the extensive and intensive margins. *Ext.* indicates the extensive margin, *Int.* indicates the intensive margin, and *Dest.* indicates product destination. Products are defined using 8-digit HS codes. We control for log firm age, 2013-2015 log mean bank borrowing, cash holdings, interest expenses, and total assets. Standard errors are clustered at the firm level. * p $<$ 0.10, ** p $<$ 0.05, *** p $<$ 0.01.

The results in table A.7 suggests that both margins are important. Columns (2) and (3) decompose exports into the number of products and the average exports per product. The effect on the intensive and extensive margin accounts for approximately 56% (=.181/.323) and 44% (=.143/323), respectively. Decomposing the export values into the destination margins, the effect within each destination is stronger. As shown in columns (4) and (5), the effect on exports per destination is approximately 64% (=.208/323), whereas the effect on the number of destinations is approximately 36% (=.115/323). If we combine both margins, the extensive margin becomes much stronger, as presented in columns (6) and (7). The effect on the number of products and destinations accounts for approximately 61%, and the effect on the average exports per product and destination becomes 39% and is not statistically significant at conventional levels.

C.7 District-level Demonetization Shock

This section additionally considers a demonetization shock that varies across Indian districts following Chodorow-Reich et al. (2019) to corroborate the empirical analyses. Specifically, Chodorow-Reich et al. (2019) utilize RBI data on the currency chest—a warehouse of currency notes—across the country and construct values for the demonetization shock that captures the variation in the replacement rate of demonetized notes. Their analyses suggest that the demonetization shock does not systematically relate to the pre-demonetization district characteristics. Although we could not access the same raw data, we extract their values for the demonetization shock across districts presented in figure 5 of

Chodorow-Reich et al. (2019). We construct an indicator variable "Demone. Shock" equalling one if the district's demonetization shock equals less than .25 and 0 otherwise. We merge this measure to our combined data using the firm location.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Exports						
				Demone. Shock=1		Demone. Shock=0	
Post x Demone. Shock$_i$	-0.118***	-0.129***	-0.124***				
	(0.038)	(0.032)	(0.033)				
Post x AR/S$_{i,t-1}$	-0.306***	-0.317***	-0.320***	-0.389***	-0.363***	-0.173	-0.268
	(0.114)	(0.100)	(0.102)	(0.143)	(0.125)	(0.152)	(0.188)
Firm FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
NIC4 x Post FE		✓	✓		✓		✓
Firm Control x Post FE			✓		✓		✓
Number of Districts	226	226	226	67	67	159	159
R^2	0.762	0.765	0.765	0.762	0.766	0.762	0.767
Observations	103747	103747	103747	64206	64206	39539	39539

Table A.8: Exports and Demonetization: Alternative Identification

Note. We control for log firm age, 2013-2015 log mean accounts receivable to sales, bank borrowing, cash holdings, interest expenses, and total assets. Standard errors are clustered by district. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.8 confirms the negative effect of demonetization on exports using the district-level demonetization shock. In addition to the accounts receivable to sales ratio used in the main body of the paper, columns (1)-(3) separately look at the district-level demonetization shock. We find that both measures independently and negatively affect exports regardless of inclusion of control variables. Columns (4)-(7) divide the sample based on the demonetization shock before estimating the regression specifications. Columns (4) and (5) show that the firms with higher AR/S were even more affected when they were located in the districts where the demonetization shock was larger, and columns (6) and (7) confirm that the effect was not significantly different from 0 if firms were located in less affected districts. All of these results are consistent with the interpretation that demonetization negatively affects exports.