A Few Good Masks: Evidence from Mask Manufacturing in Rwanda during the COVID-19 Pandemic*

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Abstract

Did increases in mask supply slow the spread of COVID-19? Rwanda licensed and incentivized textile manufacturers to produce high-quality masks at the start of the COVID-19 pandemic; we exploit spatial variation in exposure to mask manufacturing through textile trade networks within an event-study design using receipt-level tax data. Licensing domestic mask manufacturers conservatively reduced mask prices by 8.8% and reduced monthly growth in COVID-19 infections (proxied by demand for anti-fever medicine) by 12%. The dynamics of our results suggest that increased mask quality explains reduced infections, in a context where there was strict enforcement of mask mandates and informal markets for masks.

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1 Introduction

Masks are an important non-pharmaceutical intervention to slow the spread of COVID-19 (Abaluck et al., 2020; Howard et al., 2021), but constrained supply of high-quality masks may have generated inequities in access and accelerated the early stages of the COVID-19 pandemic. Sharp increases in global demand for masks following the rapid onset of the pandemic generated fear that available supply was insufficient, motivating widespread export restrictions on masks. Countries with limited domestic mask supply, and particularly developing countries, may have faced particularly stark constraints. Concerns over limited availability of masks even shaped public health recommendations on a global scale, with the World Health Organization avoiding recommending healthy individuals wear masks during early stages of the pandemic due to limited supply (World Health Organization, 2020). Yet, there is little evidence that mask supply affected either the availability of masks or the spread of COVID-19.

In this paper, we show increases in the supply of certifiably high-quality masks decreased mask prices and slowed the spread of COVID-19 in the early stages of the pandemic in Rwanda. We estimate these impacts within an event study design using receipt-level tax data, leveraging spatial variation in exposure to domestic mask manufacturing before and after a national policy licensing domestic textile manufacturers to produce masks. We find exposure to licensed mask manufacturing decreased mask prices, increased quantities purchased of domestically manufactured masks, and reduced COVID-19 infections; we provide evidence that increased mask quality likely explains impacts of mask manufacturing exposure on the spread of COVID-19.

To produce causal estimates of the impacts of mask supply, we leverage exogenous variation in exposure through domestic textile trade networks following the licensing of domestic mask manufacturers in Rwanda. On April 17, 2020, the Rwanda Food and Drug Administration (FDA) granted initial licenses to garment manufacturers to produce masks. This policy was accompanied by a certification process, ensuring that all formally traded masks met FDA filtration standards. Licensed mask manufacturing also received economic incentives to engage in mask production, including a Value Added Tax (VAT) exemption. To isolate exogenous variation in exposure to the policy-induced mask manufacturing, we make the observation that sub-Districts are more likely to source masks from the same sub-Districts from which they source non-mask textiles, yet non-mask textile sourcing does not predict sub-District characteristics nor pre-licensing changes in sub-District outcomes. We use this observation to construct a shift-share (Goldsmith-Pinkham et al., 2020) measure of exposure to licensed mask manufacturing, based on purchases by destination sub-Districts of non-mask textiles from origin sub-Districts with high and low mask manufacturing intensity. Our identification rests on the assumption that destination sub-Districts' sourcing of non-mask textiles is exogenous—destination sub-Districts that disproportionately source non-mask textiles from high and low mask manufacturing intensity sub-Districts would have experienced the same post-licensing changes in outcomes if there was no domestic mask manufacturing. We construct high-frequency panel data on inter-sub-District product trade, including prices and sales of masks, using the universe of timestamped transactions made through electronic billing machine software from November 2019 through August 2020. For each transaction we observe firm identifiers, product information, prices, and quantities, for sales to both final consumers and to firms. We produce four key results.

First, the increased supply of masks generated by average exposure to licensed domestic mask manufacturing persistently reduced mask prices by 8.8%. This decrease offset 13% of a large increase in mask prices in Rwanda that followed the start of the COVID-19 pandemic. These impacts on prices persisted to the end of our study period; suggesting that variation in exposure is driven by variation in domestic trade costs, which generated persistent decreases in mask prices in sub-Districts more exposed to licensed mask manufacturing. Our magnitudes support this interpretation: we show our estimates imply a 10% decrease in distance to mask manufacturing causes a 0.6% decrease in mask prices, in line with existing estimates from the trade literature (Donaldson, 2018).

Second, while exposure to licensed domestic mask manufacturing increased purchases of formally traded masks, these effects dissipate rapidly. Our estimates imply price elasticities of demand for masks of between 3 and 13 in April and May. These elasticities are larger than demand elasticities for other durable preventive health products (Berry et al., 2020), which we interpret as driven by the availability of a close substitute for formally manufactured masks in these early months of the pandemic—informally produced non-certified masks, which we do not observe in our data on formal transactions. These elasticities converge toward zero, coinciding with the June gazetting of a decree requiring that all masks sold in Rwanda meet the same quality standards as certified manufactured masks, which targeted the informal sale of non-certified masks. We therefore interpret this fade-out as driven by substitution away from informal low-quality masks in low-exposure sub-Districts to formally manufactured masks following the decree.

Third, increases in certified mask supply slowed the spread of COVID-19. As an alternative to sub-District data on COVID-19 cases, we use Electronic Billing Machine (EBM) data on purchases of fever medicine as a proxy for active COVID-19 infections. We find average manufactured mask exposure caused a 12% reduction in the monthly growth rate of infections through June, and no impacts on growth rate in July and August while level impacts on infections persisted. These implied impacts of certified mask manufacturing on the transmission of COVID-19, the first in the context of Sub-Saharan Africa, are comparable to existing work in the United States (Chernozhukov et al., 2021) and rural South Asia (Abaluck et al., 2021). Our results also contribute more broadly to a literature that estimates the impacts of preventive health interventions at scale (Miguel & Kremer, 2004; Bleakley, 2007, 2010); we find large benefits of government support for domestic mask manufacturing, with averted hospitalization costs an order of magnitude larger than fiscal costs.

Lastly, we infer from the dynamics of our results that mask quality, as opposed to increased mask use, explains the impact on COVID-19 infections. Starting April 19, Rwanda had strict enforcement of mask mandates and near- universal compliance. However, prior to the June decree, standards for mask quality were only implemented by, and enforced for, licensed mask manufacturers; masks meeting these standards display much greater aerosol filtration (Konda et al., 2020). While we do not directly observe the quality of informally traded masks, anecdotal evidence supports the notion that these did not meet FDA standards. Starting in June, all mask manufacturers had to follow these filtration standards. Consistent with quality rather than use as a channel, we no longer see impacts of early exposure to licensed manufacturing on the spread of COVID-19 after the June decree, just as the impacts on certified manufactured mask purchases fade out. In contrast, our impacts on prices persist, while we would expect a reduction in prices to drive any use channel. These results imply policies that increase the supply of certifiably high-quality masks are strongly complementary to policies that increase mask use (Abaluck et al., 2021).

The rest of the paper is structured as follows. Section 2 describes the data and the policy environment; Section 3 describes our identification and empirical strategy, and presents our estimates of impacts of domestic certified mask manufacturing; and Section 4 concludes.

2 Data and context

2.1 Data

We use administrative data from the Rwanda Revenue Authority (RRA) to study the market for masks at the outset of the pandemic in Rwanda. Our study sample begins in November 2019 and extends through August 2020, six months after the first positive COVID-19 test in Rwanda. We leverage administrative data from EBM receipts, customs, and firm registration, and complement these with census data.

EBM Our primary source of data is the universe of digitally signed and timestamped software issued EBM receipts collected by the tax authority between November 2019 and

August 2020, allowing us to track product-level sales between firms and final consumers. EBMs record receipts issued by businesses to support the collection of VAT in Rwanda, and is mandated for use by VAT taxpayers. In November 2019, these data recorded 81.8 billion Rwandan Francs (RwF) of value added, equivalent to 10.4% of monthly GDP or 16.9% of VAT declared turnover.

We leverage these receipts to construct firm-product data on prices and quantities at the transaction level. First, each EBM is identified by a unique Sales Data Controller (SDC) identifier, which we link to Taxpayer Identification Numbers (TIN) and, in turn, firm registration data to identify selling firms. When a receipt is issued for a sale to other VAT taxpayers, purchasing firms must provide their TIN. To construct the sub-District of both buyers and sellers we assume that non-VAT taxpayer final consumers are located in the same sub-District as the selling firm. For the majority of our analysis, we aggregate data to the buyer sub-District, which constitutes credible markets; the average sub-District has a population of 32,000 citizens and 1,000 registered firms. Second, for each transacted item on a receipt, we observe a product-classification UNSPSC code and a string description. We use two product classifications in our analysis—we identify textile products as having UNSPSC codes 601058, 5310, and 2312, and we identify masks as having string descriptions containing *mask* in English, French (*masque*), or Kinyarwanda (*agapfukamunwa*), and remove any misclassified products (e.g., masking tape). Masks are then classified as manufactured when sold by a firm that was issued a mask manufacturing license from the FDA. In Section 3.1.1, we use this data on mask and textile purchases and sales to construct sub-District mask manufacturing exposure; our primary analysis sample comprises the 86 sub-Districts for which we observe non-mask textile purchases necessary to construct mask manufacturing exposure.

Third, we observe a timestamp for each receipt, and prices and quantities for each transacted item. Lastly, each unique combination of selling firm and product string description identifies a firm-product. We present additional details on the coverage and construction of the EBM data in A.1.

Basic descriptive statistics from the EBM data are presented in Appendix A.1.3. We observe over 30,000 transactions covering over 2,500,000 masks, approximately 0.9 masks per adult in our primary analysis sample. On average, prices are 650 RwF (0.63 USD) per mask. Production of masks is spatially concentrated relative to consumption; aggregating to the sub-District level, the average purchasing (selling) sub-District buys (sells) from 2.4 (11.3) sub-Districts.

Customs We construct measures of border prices of masks using customs data containing the universe of imports by Rwandan firms. Just as for EBM, we identify mask imports using a combination of product codes and product string descriptions, and all imports are timestamped. We then use the combination of the point of entry and the TIN of the importing firm as identifiers to construct an equivalent of firm-product.

Firm registration data We construct firm characteristics using formal registrations of firms. Each firm is identified by a unique TIN, and the registrations contain the firm's ISIC sector classification (which we use to identify textile manufacturers) and sub-District.

Census We construct data on socioeconomic characteristics of sub-Districts using the 2012 Population and Housing Census.

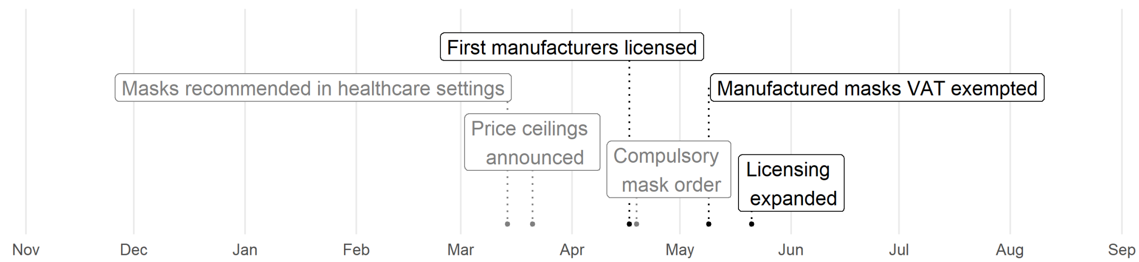
2.2 Context

Our analysis of the impacts of mask manufacturing covers the early stages of the COVID-19 pandemic in Rwanda. The Ministry of Health (MINISANTE) announced the first case of COVID-19 in Rwanda on March 14, 2020. On March 22, the Prime Minister announced a lockdown including school closures, the suspension of international travel, a work-from-home mandate, and the prohibition of non-essential movement, all with meaningful impacts on economic activity (Byrne et al., 2021). These restrictions were partially lifted on May 4, accompanied by the introduction of a curfew which then remained in place, in some form, over the remainder of our study period. Other restrictions that were lifted (and sometimes reinstated) after May 4 included the resumption of domestic travel on June 2, and the reopening of Kigali International Airport on August 1.

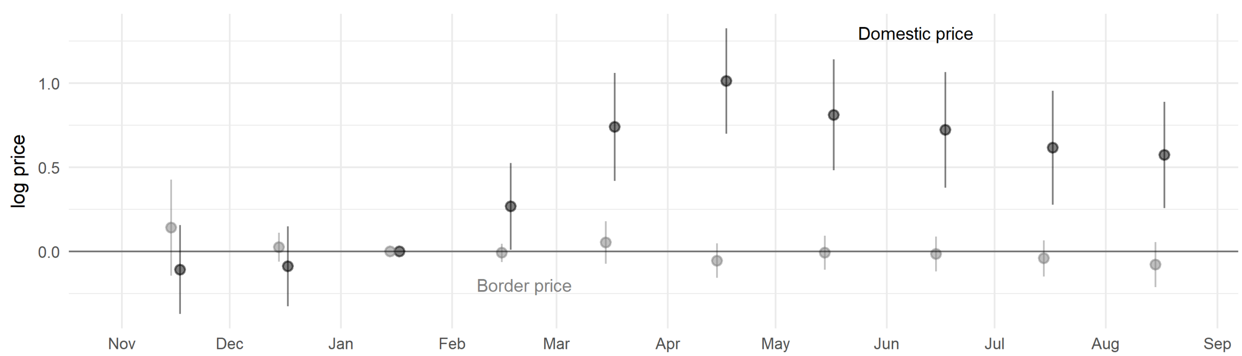
During the early months of the pandemic, Rwanda rapidly implemented recommendations and national mandates for the use of masks in public. We present the timeline of key events and policies that relate to the mask mandate during the early months of the pandemic in Rwanda in Figure 1a, leveraging fortnightly cabinet announcements and daily COVID-19 updates. On March 14, 2020, immediately following the detection of the first COVID-19 case in Rwanda, MINISANTE recommended the use of masks in health care settings. A compulsory mask mandate for all public settings was introduced on April 19, just before lockdown restrictions were partially lifted on May 4. This mandate was strictly enforced, with deterrence measures including fines in Kigali City Province, and complementary measures to promote compliance, including infomercials by the national broadcaster and education campaigns by the Rwanda National Police. These measures were effective—by early June, according to the Innovations for Poverty Action's RECOVR survey, 95% of households always wore a mask when they went out in public.

Figure 1: Timeline

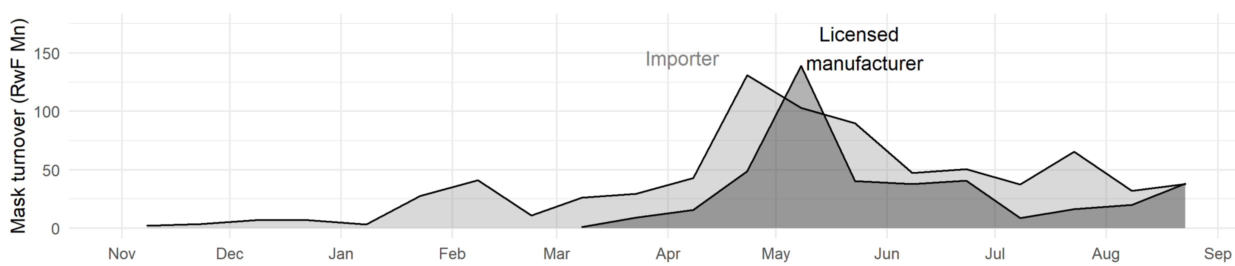
(a) Mask policy



(b) Domestic and border prices



(c) Mask turnover at mask importers and manufacturers



Notes: Figure 1a presents a timeline of mask (in black) and mask manufacturer (in pink) policies in Rwanda during our study period. Figure 1b presents a monthly time series of log changes in import (in black) and domestic (in pink) mask prices relative to January 2020. Figure 1c presents a bimonthly time series of mask turnover at importers (in black) and manufacturers (in pink).

To increase the availability of certifiably high-quality masks, the Rwanda FDA issued licenses to textile manufacturers to produce surgical and barrier masks. We present the timeline of key policy actions targeting the manufacturing of certified masks in Rwanda in Figure 1a. On March 21, 2020, in response to reported shortages of imported masks, the Ministry of Trade and Industry (MINICOM) imposed price restrictions and the FDA issued a public announcement citing concerns about the sale of substandard masks. To combat this shortage and to mitigate concerns about the sale of unregulated low-quality masks, the FDA licensed 21 garment manufacturers that had submitted applications to produce masks on April 17, two days before the national mask mandate.[1] Licensing was coupled with additional incentives to engage in mask production—MINICOM announced the intent to facilitate access to machines and raw materials, and a VAT exemption for domestically manufactured masks was granted by the Ministry of Finance and Economic Planning on April 30.

To ensure masks minimized transmission of COVID-19, quality standards were progressively scaled up, starting with licensed manufacturers and eventually covering all mask sales nationally. As part of the April 17, 2020, licensing of textile manufacturers to produce masks, the FDA enforced quality standards on manufacturers through direct assessments of manufactured mask quality. These standards included specifications for filtration, penetration, and breathability of masks, and were consistent with product characteristics known to maximize aerosol filtration efficiency (Konda et al., 2020). We present details on these requirements and specifications in B.1. Similar standards were gazetted into law in June, covering all masks. These standards constituted the benchmark for subsequent audits. Similar to enforcement in the earlier months of the pandemic, licensed manufacturers submitted their masks to quality assurance tests prior to leaving the factory. Firm audits were supple-

[1] By May 26, an additional 65 smaller firms from the textile industry had been licensed. These firms comprise only 3% of the domestic mask production in our data. In Appendix D, we find licensed firms shift production away from textiles and into masks, but do not increase profits; this is consistent with firm entry into mask production until firms earn no profits from mask production, suggesting mask demand was sufficient only to accommodate the initial manufacturers.

mented by retailer audits, and substandard masks identified by audits were removed from markets. In line with the gazetted guidelines, compliant masks were affixed with an RSB standardization mark.

2.3 Descriptive evidence on domestic manufacturing, mask prices, and markups

To motivate our analysis of the impacts of exposure to domestic mask manufacturing, we present descriptive evidence in Figure 1 that licensing and promotion increased domestic mask manufacturing and decreased mask prices in the presence of constrained international supply. To implement this analysis, we construct a time series of domestic and border prices of masks in Rwanda, and compare to total turnover by domestic mask importers and manufacturers; details on the construction of these time series are in Appendix A.4.

First, large increases in both domestic prices and turnover of masks followed mask mandates in healthcare settings and all public settings on March 14, 2020, and April 19, 2020, respectively. We interpret these policies as generating a large increase in demand for masks, which would increase both prices and quantities in the presence of upward-sloping supply of masks. However, as Rwanda is a relatively small country, one might expect it to face an exogenous international price of masks; in practice, we do not observe changes in the border price of masks. Despite this, we observe that large, statistically significant gaps emerge between domestic and border prices following mask mandates. We interpret this growing wedge between domestic and international mask prices as being potentially explained by either quantity restrictions on mask imports generated by international export restrictions on masks, or a reduced elasticity of demand for masks coupled with importer market power.

Second, domestic mask prices began to fall toward pre-COVID-19 levels starting in May, 2020, coinciding with the peak of domestic manufacturing of masks that followed initial licensing of manufacturers on April 17.[2] This coincidence provides suggestive evidence that

[2]We observe the licensing status and the imports of all firms in the EBM data. Though we cannot

domestic manufacturing decreased mask prices, but is insufficient to establish causality—alternative explanations that could have caused the observed decreases in the price of masks include increases in mask imports or decreases in demand for masks due to the reuse of masks. Establishing causality from these time series is even more challenging when COVID-19 infections are an outcome, as they were trending upward in Rwanda from March through August (just as they were globally), independent of domestic mask manufacturing. In Section 3, we therefore isolate exogenous variation in exposure to mask manufacturing across sub-Districts to estimate the impacts of mask manufacturing on purchases of domestically manufactured masks, mask prices, and COVID-19 infections.

3 Impacts of domestic mask manufacturing

3.1 Empirical strategy

3.1.1 Construction of mask manufacturing exposure

We construct a shift-share (Bartik, 1991) instrument capturing exposure to domestic mask manufacturing by combining exogenous variation in destination sub-District exposure to origin sub-Districts through textile markets with variation across origin sub-Districts in their intensity of mask manufacturing. First, we let T_{od} be the total sales of textiles *excluding masks* from November 2019 to August 2020 from origin sub-District o to destination sub-District d—we use these textile sales to construct the exposure of destination sub-Districts to mask manufacturing across origin sub-Districts. Second, we let M^s_{od} be the total sales of masks from November 2019 to August 2020 from origin sub-District o to destination sub-District d made by firms of type s. We let $s = \text{mnf}$ represent licensed textile manufacturers, and $s = \text{oth}$ represent all other firms (typically wholesalers and importers). We use these sales of masks to construct the fraction of masks sold by origin sub-District o that are

definitively rule out domestic manufacture by non-licensed EBM firms, we do not expect firms to formally record non-licensed production activity.

manufactured (its "mask manufacturing intensity"). Following Goldsmith-Pinkham et al. (2020), we exclude sales to destination sub-District d in this construction. We then construct our shift-share instrument for exposure to domestic mask manufacturing in destination sub-District d as

$$\text{Mask manufacturing exposure}_d \equiv \sum_o \underbrace{\frac{T_{od}}{\sum_{o'} T_{o'd}}}_{\text{non-mask textile share}} \underbrace{\frac{\sum_{d' \neq d} M^{\text{mnf}}_{od'}}{\sum_{d' \neq d} M^{\text{mnf}}_{od'} + M^{\text{oth}}_{od'}}}_{\text{mask manufacturing intensity}} \tag{1}$$

We plot variation in mask manufacturing exposure across sub-Districts in Figure 2a, and for comparison plot variation in mask manufacturing intensity in Figure 2b. Since mask manufacturing intensity measures exposure to certified masks, we note that our exposure measure should also be interpreted as exposure to high-grade masks. In Section 3.1.2, we argue variation in mask manufacturing exposure is exogenous.

Construction We exclude sales to destination sub-District d when constructing mask manufacturing intensity of each origin sub-District o to mitigate the concern that shocks to demand for manufactured masks in destination sub-District d could increase the mask manufacturing intensity in closely connected origin sub-Districts, increasing d's mask manufacturing exposure and generating simultaneity bias; in Appendix F.1, we show that our results are unaffected if we do not exclude sales to destination sub-District d. In addition, we note that mask manufacturing exposure is only defined for sub-Districts with non-mask textile purchases, and that we define mask manufacturing intensity to be 0 in origin sub-Districts with no mask sales to other destinations.

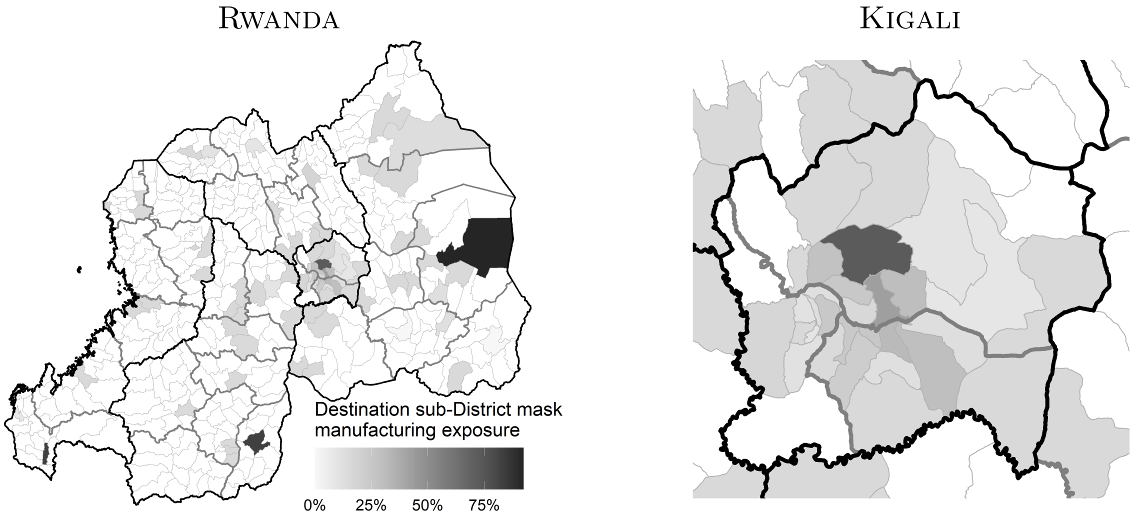
Interpretation We note that mask manufacturing exposure can be interpreted as destination sub-District d's predicted purchases of domestically manufactured masks as a share of total purchases of masks. Mask manufacturing exposure is not equal to actual purchases of domestically manufactured masks as a share of total purchases of masks for two reasons—

leave-out mask manufacturing intensity of origin o may not equal the mask manufacturing intensity of origin o's mask sales to destination d, and destination d's non-mask textile purchase share from origin o may differ from its mask purchase share from origin o.[3] Importantly, these differences exclude idiosyncratic destination d demand for masks from the construction of mask manufacturing exposure.

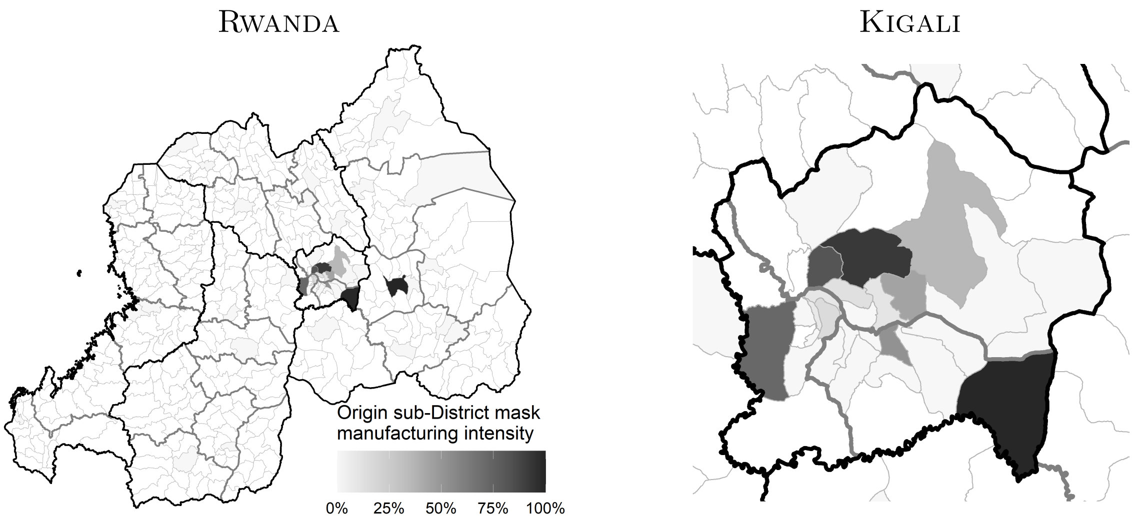
[3]Formally, $\frac{\sum_{d' \neq d} M_{od'}^{\text{mnf}}}{\sum_{d' \neq d} M_{od'}^{\text{mnf}} + M_{od'}^{\text{oth}}} \neq \frac{M_{od}^{\text{mnf}}}{M_{od}^{\text{mnf}} + M_{od}^{\text{oth}}}$ and $\frac{T_{od}}{\sum_{o'} T_{od}} \neq \frac{M_{od}^{\text{mnf}} + M_{od}^{\text{oth}}}{\sum_{o'} M_{o'd}^{\text{mnf}} + M_{o'd}^{\text{oth}}}$, respectively. In Appendix I, we show that the mask manufacturing intensity of origin sub-District mask sales is correlated across destinations, and that destination sub-Districts are more likely to source masks from the same sub-Districts from which they source non-mask textiles; as a result, mask manufacturing exposure increases the post-licensing supply of domestically manufactured masks.

Figure 2: Mask manufacturing exposure and intensity vary substantially across sub-Districts

(a) Mask manufacturing exposure$_d \equiv \sum_o \frac{T_{od}}{\sum_{o'} T_{o'd}} \frac{\sum_{d' \neq d} M_{od'}^{\text{mnf}}}{\sum_{d' \neq d} M_{od'}^{\text{mnf}} + M_{od'}^{\text{oth}}}$



(b) Mask manufacturing intensity$_o \equiv \frac{\sum_d M_{od}^{\text{mnf}}}{\sum_d M_{od}^{\text{mnf}} + M_{od}^{\text{oth}}}$



Notes: Figure 2a plots variation in destination sub-District mask manufacturing exposure. Figure 2b plots variation in origin sub-District mask manufacturing intensity. The left panels plot variation across Rwanda, while the right panels zoom in on variation within Kigali City Province.

3.1.2 Estimating the impacts of mask manufacturing exposure

We estimate the impacts of exposure to licensed mask manufacturing on availability and prices of domestically manufactured masks and the spread of COVID-19 using an event-study design. For outcome y_{dt} measured in sub-District d in month t, we estimate the following two-way fixed effects specification:

$$y_{dt} = \sum_{t=\text{Nov 2019}, t \neq \text{Mar 2020}}^{\text{Aug 2020}} \beta_t \text{Mask manufacturing exposure}_d + X_d' \delta_t + \theta_d + \gamma_t + \epsilon_{dt} \tag{2}$$

θ_d and γ_t are a set of sub-District fixed effects and month fixed effects, respectively. $X_d' \delta_t$ allows time-varying coefficients on sub-District characteristics, to control for sub-District characteristics that may be correlated with both mask manufacturing exposure and time trends (Duflo, 2001). Our coefficients of interest are β_t, the impact of mask manufacturing exposure in month t. We use March 2020, one month prior to the initial licensing of textile manufacturers to produce multi-layer masks, as the reference month. Coefficients β_t should therefore be interpreted as impacts on changes in outcomes relative to March 2020. For inference, we cluster standard errors at the sub-District-level, level at which mask manufacturing exposure varies.

Choice of supply shifter We use exposure to licensed mask manufacturing as our source of exogenous variation in mask supply. We do so, rather than mask manufacturing intensity, for two reasons. First, variation in mask manufacturing intensity is unlikely to be exogenous—one potential concern is that sub-Districts which manufacture more masks are likely to have more productive garment manufacturers, and this may be correlated with determinants of demand for masks and factors influencing the spread of COVID-19. Second, licensed mask manufacturing in a given sub-District poorly predicts the exposure of consumers in that sub-District to licensed mask manufacturing; exposure will depend ultimately on the mask manufacturing intensity in sub-Districts from which local firms source masks, which need not be located in the same sub-District.

Sufficient conditions for exogeneity Our estimates of the impacts of mask manufacturing exposure will be unbiased if mask manufacturing exposure is independent of other determinants of the availability and prices of domestically manufactured masks and the spread of COVID-19. As in Goldsmith-Pinkham et al. (2020), one sufficient condition for this assumption is that destination sub-District sourcing of other textiles is independent of other determinants of the availability and prices of domestically manufactured masks and the spread of COVID-19. In particular, we assume it is not the case that destination sub-Districts that disproportionately source other textiles from high mask manufacturing intensity origin sub-Districts would have had larger or smaller changes in demand or supply of certified masks and COVID-19 infections absent licensed domestic mask manufacturing. For this assumption to hold, it is particularly important that we have excluded masks when constructing our measures of other textile sourcing, as destination sub-Districts with high demand for domestically manufactured masks will source more of their masks from origin sub-Districts with high mask manufacturing intensity. In contrast, it is unlikely that idiosyncratic shocks to demand or supply of masks will be correlated with the sourcing of non-mask textiles (predominantly clothing).

Tests of exogeneity We test the robustness of our assumption of the exogeneity of mask manufacturing exposure in three ways, adapting suggestions from Goldsmith-Pinkham et al. (2020) for difference-in-differences with shift-share instruments to our event-study design; the results of all three tests corroborate the exogeneity of exposure to policy-induced mask manufacturing. First, in Section 3.2 we show mask manufacturing exposure is uncorrelated with sub-District characteristics, and in Section 3.3 we show our results are robust to including or excluding important sub-District characteristics X_d from Equation 2. Second, in Section 3.3 we show mask manufacturing exposure is uncorrelated with trends in outcomes prior to licensing.[4] Third, we replace our measure of leave-out mask manufacturing intensity with its residuals from regressions on origin sub-District characteristics, which changes the weights placed on destination sub-Districts' exogenous non-mask textile purchase shares across origins (Goldsmith-Pinkham et al., 2020). Our results that both leverage residualized shocks, and also include controls interacted with time fixed effects, are doubly robust; this is because either exogeneity of the non-mask textile purchase shares conditional on observables, or exogeneity of the mask manufacturing intensity shocks conditional on observables, is sufficient for consistency of our estimates of the impacts of mask manufacturing exposure.

Event studies and two-way fixed effects Equation 2 does not feature a staggered design, and therefore is not subject to recent criticisms of two-way fixed effects specifications with a staggered rollout of a binary treatment (e.g., de Chaisemartin & D'Haultfœuille (2020)). However, it is similar to specifications analyzed in recent work that demonstrates the parallel trends assumption is insufficient for identification in difference-in-difference designs with continuous treatment (in our context, mask manufacturing exposure) when treatment effect heterogeneity is correlated with treatment (Callaway et al., 2021). Here, a stronger independence assumption is sufficient for identification, and we test this assumption in Section 3.2. A similar issue emerges when controls are interacted with time fixed effects in a two-way fixed effects specification, even when treatment is binary (Borusyak et al., 2021b). In Section 3.3, we show that our results are robust to the inclusion or exclusion of controls interacted with time fixed effects.

Testing parallel trends and pre-test bias Equation 2 jointly tests for parallel trends pre-licensing and estimates impacts post-licensing, which can introduce bias from pre-testing (Roth, 2021); in Appendix F.2 we follow Borusyak et al. (2021b) and show our results are robust to an approach that eliminates this bias.

[4]We only observe data on our primary outcomes going back to November 2019; as a complement, in Appendix E.1 we also show that mask manufacturing exposure is uncorrelated with trends in VAT turnover going back to the first quarter of 2019.

Inference We cluster standard errors at the sub-District level, as opposed to inference based on variation in mask manufacturing intensity (Adao et al., 2019), as we argue exogenous variation in destination sub-District mask manufacturing exposure comes from destination sub-Districts' textile sourcing ("shares"), as discussed in Goldsmith-Pinkham et al. (2020), rather than from origin sub-Districts' mask manufacturing intensity ("shocks"), as assumed in Adao et al. (2019) and Borusyak et al. (2021a). In results available upon request, we produce results with standard errors clustered at either the District-level or Conley (1999) standard errors using a range of bandwidths of distances between sub-District centroids. In general, clustering at the sub-District level results in more conservative inference and is our preferred approach.

Outcomes We have three primary outcomes of interest in Equation 2: log buyer prices of masks, mask quantities purchased from manufacturers as a share of total mask quantities purchased, and COVID-19 infections. First, when log buyer prices of masks are the outcome of interest, we do not aggregate to the destination sub-District-month, as changes in aggregated prices would include changes in the firm-product composition of purchased masks. We instead construct log buyer prices as mean log prices at the destination sub-District-by-month-by-firm-product level, and include firm-product fixed effects in Equation 2 to control for any unobservable mask characteristics that might influence price, such as style or material.[5] Second, we use "effective," or quality-adjusted, quantities of masks to construct the share of masks purchased from manufacturers; we describe the construction of effective quantities in Appendix A.1.2, and we show our results are similar when we use sales values instead of effective quantities in Appendix F.5. Third, when COVID-19 infections are the outcome of interest, we instead use the number of purchases of paracetamol, a commonly recommended fever medicine, as a proxy for COVID-19 infections.[6] To produce

[5]In Appendix F.4, we test robustness to the level of aggregation by leaving observations at the receipt-by-firm-product level and find qualitatively similar results.

[6]We use purchases of paracetamol as a proxy because we do not have data on sub-District COVID-19 infections as COVID-19 testing in Rwanda did not become widespread until the last two months of our study period. In Appendix G, we show that increased purchases of paracetamol are associated with

estimates that are readily comparable to existing work, we estimate impacts on changes in the log number of purchases of paracetamol. As the number of purchases of paracetamol is frequently 0 in a given sub-District, we estimate Equation 2 using Poisson pseudo maximum likelihood (Wooldridge, 1999).

3.2 Balance

In Table 1, we show that mask manufacturing exposure is uncorrelated with destination sub-District characteristics. This result holds across specifications that do and do not control for Province fixed effects, and specifications that do and do not replace leave-out mask manufacturing intensity with its residuals from regressions on Province fixed effects, log population density, and log number of textile manufacturers.[7] Across 32 tests, we fail to reject the null of no correlation in just two tests at the 10% level. First, in Column 4, sub-Districts with high mask manufacturing exposure purchase significantly more inputs when we include Province fixed effects, but we do not residualize mask manufacturing intensity when constructing mask manufacturing exposure. Second, in Column 5, we reject the null of balance in the omnibus F-test when we do not include Province fixed effects, but we do residualize mask manufacturing intensity when constructing mask manufacturing exposure. We find no evidence of imbalance for specifications for which we report results in Section 3.3; these specifications are analogous to Column 3 (no controls and no residualization of mask manufacturing intensity when constructing mask manufacturing exposure) and Column 6 (controls for, and residualization of, mask manufacturing intensity when constructing mask manufacturing exposure on Province fixed effects, log textile manufacturers, and log

increased COVID-19 infections at the District-level. As COVID-19 infections are not the sole cause of fever, and therefore of purchases of paracetamol, we discuss how this impacts the interpretation of our magnitudes in Appendix C.

[7] We choose log population density and log number of textile manufacturers as these controls are the most strongly correlated with mask manufacturing intensity in Columns 1 and 2. These two characteristics plausibly affect mask manufacturing intensity—as textile manufacturers produce masks, sub-Districts with more textile manufacturers will likely produce more masks, and lower population density sub-Districts are more likely to specialize in manufacturing relative to intermediation or importing.

population density).[8]

Table 1: Mask manufacturing intensity and mask manufacturing exposure are uncorrelated with sub-District characteristics

	Dependent variable:					
	Mask manufacturing intensity		Mask manufacturing exposure			
	(1)	(2)	(3)	(4)	(5)	(6)
log EBM turnover, RwF Bn	−0.000	−0.080	−0.023	−0.026	−0.021	−0.025
	(0.076)	(0.096)	(0.015)	(0.016)	(0.017)	(0.019)
	[0.997]	[0.408]	[0.120]	[0.120]	[0.216]	[0.202]
log EBM input, RwF Bn	−0.115	0.017	0.054	0.062	0.057	0.064
	(0.124)	(0.151)	(0.033)	(0.035)	(0.039)	(0.040)
	[0.361]	[0.910]	[0.109]	[0.077]	[0.148]	[0.121]
log population density	−0.104	−0.080	−0.040	−0.046	−0.032	−0.042
	(0.068)	(0.066)	(0.039)	(0.037)	(0.045)	(0.042)
	[0.136]	[0.234]	[0.312]	[0.210]	[0.483]	[0.326]
% completed primary school	0.015	0.000	−0.001	−0.001	0.001	0.001
	(0.018)	(0.019)	(0.002)	(0.002)	(0.002)	(0.003)
	[0.392]	[0.999]	[0.684]	[0.706]	[0.703]	[0.706]
% employed	−0.012	0.009	−0.013	−0.004	−0.009	0.001
	(0.024)	(0.026)	(0.009)	(0.010)	(0.010)	(0.011)
	[0.628]	[0.732]	[0.167]	[0.719]	[0.390]	[0.962]
log textile manufacturers	0.183	0.169	−0.022	−0.016	−0.013	−0.006
	(0.131)	(0.128)	(0.037)	(0.035)	(0.044)	(0.043)
	[0.173]	[0.198]	[0.560]	[0.661]	[0.765]	[0.893]
log population	0.069	0.055	−0.016	−0.036	−0.039	−0.049
	(0.163)	(0.145)	(0.051)	(0.065)	(0.055)	(0.072)
	[0.677]	[0.708]	[0.757]	[0.577]	[0.477]	[0.497]
Province FE		X		X		X
Residualized shocks					X	X
# observations	43	43	86	86	86	86
# clusters (sub-Districts)	43	43	86	86	86	86
Omnibus F	0.629	0.993	1.123	0.999	2.124	0.942
	[0.728]	[0.454]	[0.358]	[0.439]	[0.051]	[0.480]

Notes: Columns 1 and 2 report coefficients from regressions of mask manufacturing intensity on sub-District characteristics, while Columns 3 through 6 report coefficients from regressions of mask manufacturing exposure on sub-District characteristics. Standard errors are reported in parentheses, and p-values are reported in brackets. Columns 2, 4, and 6 control for Province fixed effects, while Columns 5 and 6 residualize leave-out mask manufacturing intensity by regression on Province fixed effects, log population density, and log number of textile manufacturers before constructing mask manufacturing exposure.

[8] We report results from specifications analogous to Columns 4 and 5 in Appendix F.3, and find our estimates are qualitatively similar under these alternative approaches to introducing controls.

3.3 Results

3.3.1 Impacts of mask manufacturing exposure

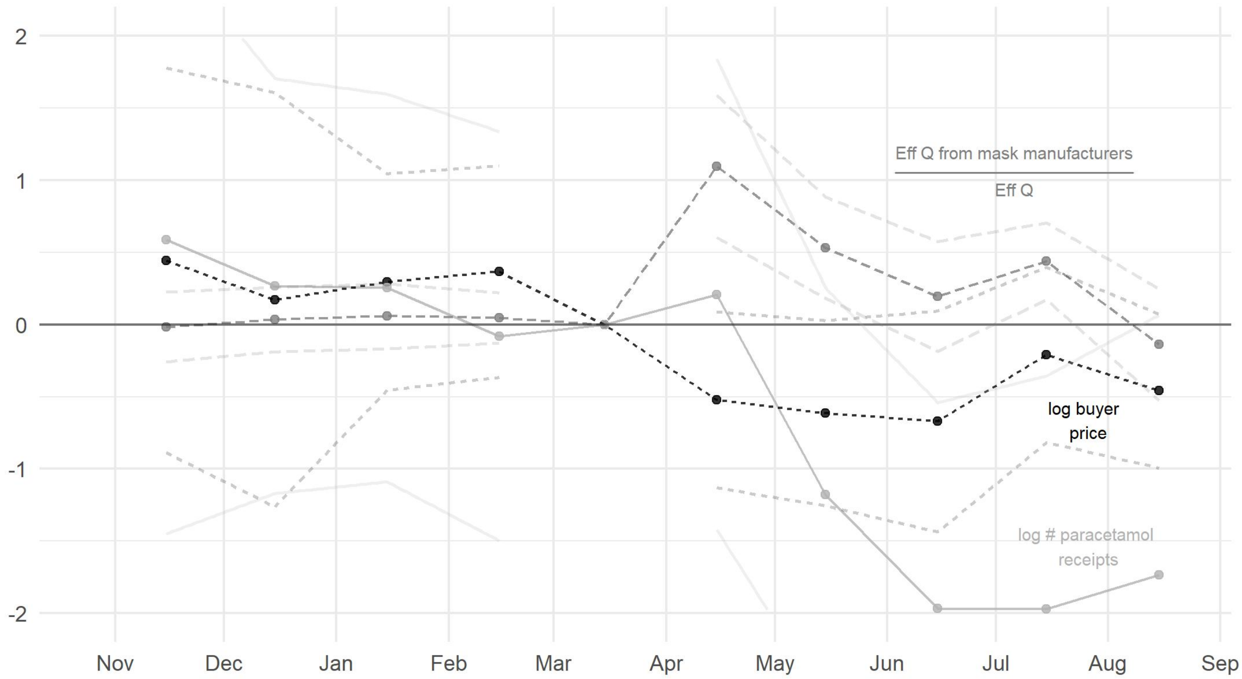
We now present our results on the impact of mask manufacturing exposure on mask prices, purchases of masks from manufacturers, and COVID-19 infections. Table 2 reports estimates of these impacts from Equation 2; we focus our discussion on estimates from Columns 1, 3, and 5 from our most parsimonious specification without controls, as estimates are quantitatively and qualitatively similar when we include controls, and we graph these estimates in Figure 3. All impacts are relative to March 2020, the month before the licensing of domestic textile manufacturers to produce high-quality masks was announced.

Table 2: Mask manufacturing exposure increases purchases of masks from manufacturers, decreases mask prices, and reduces COVID-19 infections

	Dependent variable:					
	log buyer price		$\frac{\text{Eff mask Q from manufacturers}}{\text{Eff mask Q}}$		# paracetamol receipts	
	(1)	(2)	(3)	(4)	(5)	(6)
Mask manufacturing exposure$_d$ × Nov$_t$	0.445 (0.680) [0.514]	0.263 (0.666) [0.694]	-0.016 (0.123) [0.897]	0.155 (0.202) [0.444]	0.588 (1.040) [0.572]	0.029 (0.859) [0.973]
Mask manufacturing exposure$_d$ × Dec$_t$	0.173 (0.732) [0.814]	0.199 (0.717) [0.782]	0.035 (0.114) [0.760]	0.135 (0.214) [0.531]	0.267 (0.734) [0.716]	-0.194 (0.618) [0.753]
Mask manufacturing exposure$_d$ × Jan$_t$	0.296 (0.383) [0.441]	-0.101 (0.343) [0.769]	0.057 (0.115) [0.620]	0.224 (0.211) [0.290]	0.254 (0.685) [0.711]	0.415 (0.685) [0.545]
Mask manufacturing exposure$_d$ × Feb$_t$	0.367 (0.374) [0.328]	0.393 (0.318) [0.219]	0.046 (0.089) [0.608]	0.130 (0.181) [0.475]	-0.080 (0.723) [0.912]	0.524 (0.752) [0.485]
Mask manufacturing exposure$_d$ × Mar$_t$	-	-	-	-	-	-
Mask manufacturing exposure$_d$ × Apr$_t$	-0.521 (0.311) [0.098]	-0.600 (0.300) [0.049]	1.095 (0.252) [0.000]	0.973 (0.413) [0.021]	0.208 (0.832) [0.803]	-0.393 (0.953) [0.680]
Mask manufacturing exposure$_d$ × May$_t$	-0.613 (0.327) [0.064]	-0.673 (0.290) [0.023]	0.533 (0.179) [0.004]	0.464 (0.303) [0.130]	-1.178 (0.730) [0.107]	-1.056 (0.565) [0.062]
Mask manufacturing exposure$_d$ × Jun$_t$	-0.671 (0.391) [0.089]	-0.792 (0.423) [0.065]	0.196 (0.194) [0.316]	0.513 (0.293) [0.083]	-1.968 (0.728) [0.007]	-1.669 (0.571) [0.003]
Mask manufacturing exposure$_d$ × Jul$_t$	-0.210 (0.309) [0.499]	-0.316 (0.314) [0.318]	0.438 (0.136) [0.002]	0.398 (0.262) [0.132]	-1.973 (0.825) [0.017]	-1.619 (0.753) [0.032]
Mask manufacturing exposure$_d$ × Aug$_t$	-0.460 (0.272) [0.095]	-0.606 (0.251) [0.018]	-0.138 (0.197) [0.485]	0.031 (0.311) [0.920]	-1.734 (0.918) [0.059]	-1.564 (0.862) [0.070]
Estimation method	OLS	OLS	OLS	OLS	Poisson	Poisson
Firm-product FE	X	X				
Destination sub-District FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Residualized shocks		X		X		X
Controls × Month FE		X		X		X
# observations	3,937	3,937	506	506	780	780
# clusters (sub-Districts)	86	86	86	86	78	78

Notes: Columns 1 through 6 report coefficients on mask manufacturing exposure interacted with month fixed effects from estimates of Equation 2. Standard errors are reported in parentheses, and p-values are reported in brackets. All columns include destination sub-District fixed effects and month fixed effects. Columns 1 and 2 include firm-product fixed effects, while Columns 5 and 6 estimate coefficients using Poisson pseudo maximum likelihood. Columns 2, 4, and 6 include month fixed effects interacted with controls for Province fixed effects, log population density, and log number of textile manufacturers, and residualize leave-out mask manufacturing intensity by regression on those same controls before constructing mask manufacturing exposure.

Figure 3: Mask manufacturing exposure increases purchases of masks from manufacturers, decreases mask prices, and reduces COVID-19 infections



Notes: Estimated dynamic impacts of mask manufacturing exposure from Columns 1, 3, and 5 of Table 2 with 95% confidence intervals are plotted above.

First, exposure to mask manufacturing causes persistent decreases in the price of masks through greater access to mask markets (see columns 1 & 2 in table 2). Scaling our average estimated effect on prices across months by average mask manufacturing exposure, our results imply licensing mask manufacturing decreases prices nationally by 8.8%.[9] This estimate represents a lower bound in that it does not account for across sub-District general equilibrium effects through markets for non-manufactured masks. We take two approaches to interpreting the magnitude of this effect. First, in Figure 1b, we showed that March 2020 mask prices were 109% higher in Rwanda than in January, associated with the large increase in demand for masks during the COVID-19 pandemic, while, by August, prices had fallen 17.9% below March levels. Scaling our average effect by average mask manufacturing exposure, our results imply 51.6% of the decrease in prices from March to August is explained by mask manufacturing, offsetting 13% of the January-to-March increase. Second, as mask manufacturing exposure is constructed from differences across sub-Districts in non-mask textile trade flows, we interpret these impacts as being driven by persistent differences in costs of trade between sub-Districts. Building on this interpretation, we combine our estimated impacts of mask manufacturing exposure on prices with estimates from a gravity regression of log textile trade flows on log distance which we present in Appendix H; the resulting back-of-envelope calculation implies that a 10% decrease in distance to mask manufacturing at average mask manufacturing exposure causes a 0.6% decrease in mask prices.[10]

Second, the persistent decrease in mask prices caused by exposure to mask manufacturing caused large, temporary increases in mask quantities purchased from manufacturers prior to the enforcement of quality standards nationally. As shown in Figure 3, impacts on mask quantities purchased from manufacturers are large and significant in April, but fade over time and vanish completely by August. We argue that our results in the initial months reflect substitution away from informal non-certified masks into domestically manufactured certified

[9]The average estimated effect on prices is the average of the post-March coefficients from column 1 of Table 2. Average mask manufacturing exposure is calculated across destination sub-Districts.

[10]For comparison, Donaldson (2018) estimates a 10% decrease in distance to the source of salt causes a 0.9%–1.7% price decrease in salt prices in India, statistically indistinguishable from our estimate.

masks in sub-Districts with high mask manufacturing exposure, while the declining impacts are driven by this same substitution occurring in sub-Districts with low mask manufacturing exposure following the implementation of quality standards for masks. The June gazetting of quality standards for masks sold in Rwanda coincides with these declining impacts; quality standards were already met by masks produced by licensed manufacturers, as we discuss in Section 2.2. As a result, these standards primarily targeted the informal sale of non-certified masks, while mask purchases in our data are of domestically manufactured and imported masks, which likely already met these standards. In summary, the introduction of a ban on a substitute product (informal non-certified masks) decreased the elasticity of demand for domestically manufactured masks, and particularly so when coupled with strictly enforced mask mandates, and this decreased the quantity response of certified manufactured masks to the persistent price decrease caused by exposure to mask manufacturing.

In Section 3.1.1, we noted that mask manufacturing exposure could be interpreted as predicted mask purchases from manufacturers as a share of total mask purchases. Consistent with this interpretation, a one-unit increase in mask manufacturing exposure causes an approximately one-for-one increase in mask quantities purchased from manufacturers as a share of total mask quantities purchased in April and May.

To interpret magnitudes of the impacts of mask manufacturing exposure on quantities, we calculate that our April and May estimates of impacts on prices and quantities imply a price elasticity of demand of 12.9 in April and 3.2 in May.[11] These estimated elasticities are larger than comparable estimates from Berry et al. (2020) (who find elasticities of between 0 and 4 over a range of prices) for water filters, consistent with the availability of a close substitute for certified manufactured masks (non-certified masks) in these early months.

Third, licensed domestic mask manufacturing slowed the spread of COVID-19 at early

[11]To calculate these elasticities, we divide impacts on mask quantities from manufacturers as a share of total quantities by its average value in each month (0.15 in April and 0.27 in May), and divide this by impacts on log buyer price. In results available upon request, we recover similar elasticities when we instead use Poisson pseudo maximum likelihood estimates of impacts on expected log mask quantities from manufacturers.

stages of the pandemic; however, trade frictions reduced access to certified masks in less exposed sub-Districts, temporarily increasing infection growth and persistently increasing the level of infections in less exposed sub-Districts. We scale impacts on COVID-19 infections, as proxied by the number of purchases of paracetamol, by average mask manufacturing exposure—by June, mask manufacturing had reduced COVID-19 infections nationally by 31%, corresponding to a 12% decrease in monthly infection growth.[12] Similar to estimates of price impacts, this estimate reflects a lower bound, because of both across-sub-District transmission of COVID-19 and any across sub-District general equilibrium price effects. While impacts on COVID-19 infections grew steadily through June, they remain constant after—this implies that, while the impacts of domestic mask manufacturing on infection growth persist through June, they dissipate after. This leaves persistent impacts on the level of COVID-19 infections.

3.3.2 Interpreting impacts of mask manufacturing exposure on infections

To explain the dynamics of impacts on COVID-19 infections, we argue that the local impacts of exposure to mask manufacturing that we document are mediated by substitution away from informally produced masks toward formally manufactured masks, rather than by increases in the use of masks. As discussed in Section 2.2, Rwanda had strict enforcement of mask mandates and near-universal compliance starting April 19, suggesting that we should not expect there to be differences in mask use between sub-Districts with high and low exposure to licensed mask manufacturing; there may instead be differences in the types of masks used (and, specifically, in quality). While we do not directly observe the quality of informally manufactured masks in our data, the dynamics of these impacts lend support to quality as the main mechanism driving a wedge in COVID-19 infections across high- and low-exposure sub-Districts. We use the fact that, starting in June, national standards for mask quality (which already applied to certified manufacturers) were expanded to non-licensed manufac-

[12] 31% decline calculated using the June estimate from column 5 of table 2, scaled by average mask manufacturing exposure (see Equation 1).

turers, shutting down the informal sales of masks, while licensing was extended to all formal garment manufacturers. Should quality be the main mechanism underlying our results, we should then see the impacts of mask manufacturing exposure on both manufactured mask purchases and COVID-19 infection growth fade over time. This is exactly what we observe. In addition, the persistence of our impacts on prices rules out a quantity channel.

The implied impacts of certified mask use on COVID-19 infections, based on our estimates, are consistent with existing empirical estimates of the impacts of high-quality masks in other contexts. We calculate impacts of certified mask use by scaling our June impacts on COVID-19 infections by the inverse of April impacts on mask purchases from licensed manufacturers as a share of total mask purchases. As mask mandates were strictly enforced, we interpret this scaled estimate as the effect of shifting a sub-District from not purchasing formally manufactured masks (limited use of certified masks) to exclusively purchasing formally manufactured masks (universal use of certified masks) at the early stages of the pandemic. Certified mask use reduces monthly infection growth by 37%. In Appendix C, we calculate estimates of closely related parameters from existing work; mask mandates for employees in public-facing businesses in the United States reduce monthly case growth by 35% (Chernozhukov et al., 2021), while surgical mask use reduces monthly infection growth by 18% in rural Bangladesh (Abaluck et al., 2021). The former estimate is statistically indistinguishable from ours, while the latter estimate is of impacts on villages (much smaller than sub-Districts) for which cross-village spillovers are likely to render estimates more conservative. That these three estimates are comparable is consistent with large effects of mask quality on COVID-19 transmission, in line with existing lab (Konda et al., 2020) and field (Abaluck et al., 2021) evidence.

4 Conclusion

In this paper, we show that an industrial policy aimed to increase the supply of high-quality masks slowed the spread of COVID-19 in Rwanda. We leverage the licensing of textile manufacturers to produce high-quality masks as a shock to mask production. To establish causality, we exploit the fact that sub-Districts are more likely to source masks from the same sub-Districts from which they source non-mask textiles, and yet non-mask textile sourcing does not predict sub-District characteristics nor pre-licensing changes in sub-District outcomes. Our results show licensing decreased mask prices and slowed the spread of COVID-19 in the early stages of the pandemic through increased access to formally manufactured masks. A cost-benefit calculation suggests averted hospitalization costs from estimated impacts on reduced COVID-19 infections were conservatively an order of magnitude larger than the fiscal costs of promotion of domestic mask manufacturing.[13]

We establish three key results: exogenous exposure to licensed mask manufacturing decreased mask prices, increased purchases of formally manufactured masks, and reduced COVID-19 infections. Although we do not directly observe mask quality, the dynamics of these impacts in the Rwandan context, where mask mandates were well enforced, suggest that increased quality of masks, rather than increased use, explains our results. Taken together, these results confirm the notion that constrained supply of high-quality masks accelerated the spread of COVID-19 at the early stages of the pandemic. While our results leverage sub-national variation in exposure to mask manufacturing, they suggest a similar role of access to masks in explaining international variation in the progression of the pandemic—i.e., there were not enough good masks.

[13]Details of this calculation are in Appendix J.

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A Data appendix

A.1 EBM data

We use the universe of EBM transactions made through EBM II as our original data from November 2019 through August 2020. EBM II is a software-based teller system introduced in 2018 as an alternative to traditional physical EBMs (EBM I); additional details on each are presented in Appendix A.1.1.[A1] We restrict to data in November 2019 and later, because the number of active EBM II devices in November increased by 50% relative to October, as part of a large EBM II registration drive.[A2]

Coverage EBM is mandated for use by VAT taxpayers, described in Appendix A.3. Receipts issued by registered taxpayers are transmitted via an internet connection to the RRA. In November 2019, EBM II recorded 81.8 billion RwF of value added, or 10.4% of GDP on 470,000 receipts.

Receipt data Each EBM is identified by a unique Sales Data Controller (SDC) ID, and issues receipts which are digitally signed with a unique Sales Data Controller Receipt Signature (SDCS). A fictitious EBM receipt is depicted in Appendix A.1.1. Each receipt enumerates the transacted items, and records the prices at (and quantities in) which they were sold.

EBM II receipts include both a United Nations Standard Products and Services Code (UNSPSC) classification code and detailed item descriptions for each item. We refer to the item description as the product. Since each SDC is associated with a unique Taxpayer

[A1]EBMs, or Electronic Billing Machines, are no longer exclusively physical machines. For this reason, Electronic Invoicing Systems (EIS) nomenclature is used as an umbrella term for both physical and digital/software-based receipt generating systems. To align with the colloquial usage in Rwanda, in this Appendix we refer to any official receipt generating system as EBM, and delineate versions when referring to any specific form of EBM (EBM I, EBM II). In the body of the paper, for parsimony we refer to EBM II as EBM, both because we do not use EBM I data for our analysis and also because of the relatively broad coverage of EBM II.

[A2]Although EBM I offers more coverage before November 2019, these receipts are not comprehensively stored in a machine readable format.

Identification Number (TIN), the seller is identified in all transactions. If the buyer provides their unique TIN for the transaction, both the buyer and the seller are identified by the receipt.[A3] Receipts are timestamped with the date and time of issue.[A4]

EBM location While an SDC is associated with one TIN, a firm may have multiple EBMs. Since we do not observe the location of each SDC, we assume that the EBM is located in the same sub-District as the firm. We use firm registration data to geo-locate firms (see Appendix A.3 for a description of firm registration data). In November 2019, 95.5% of EBM II taxpayers reported having only one EBM II device. We study the product market at the sub-District level, the third administrative sub-division in Rwanda. Sub-district disaggregation allows us to leverage sufficient geographic variation, while constituting a credible definition of a market; the average sub-District has a population of 32,000 citizens and 1,000 registered firms.

Since most EBM receipts identify both a buyer and a seller, we are able to map mask supply chains from the sub-District of the seller to the sub-District of the buyer. We retain a receipt-by-firm-product dataset (aggregated to a buyer sub-District-by-firm-product-by-month dataset for our main analysis) to study mask prices and construct a balanced buyer sub-District-by-month dataset to study purchases of masks and paracetamol across sub-Districts.

Administrative use of EBM The primary purpose of EBM is to support the self-enforcing design of the VAT (Pomeranz, 2015). Through the creation of a paper trail, EBM increases the information with which the RRA can validate declarations and undertake audits. In Rwanda, VAT input credits must be supported by EBM receipts, creating an incentive for the buyer to both request and associate their TIN with a purchase receipt.

[A3]Since firms require receipts to claim input tax credits, we consider unidentified buyers as final consumers.

[A4]While transmission of the receipt to RRA can be delayed, either if software is malfunctioning or if the internet connection is unstable, the RRA estimates 98% of total receipts due to be declared are transmitted within two weeks.

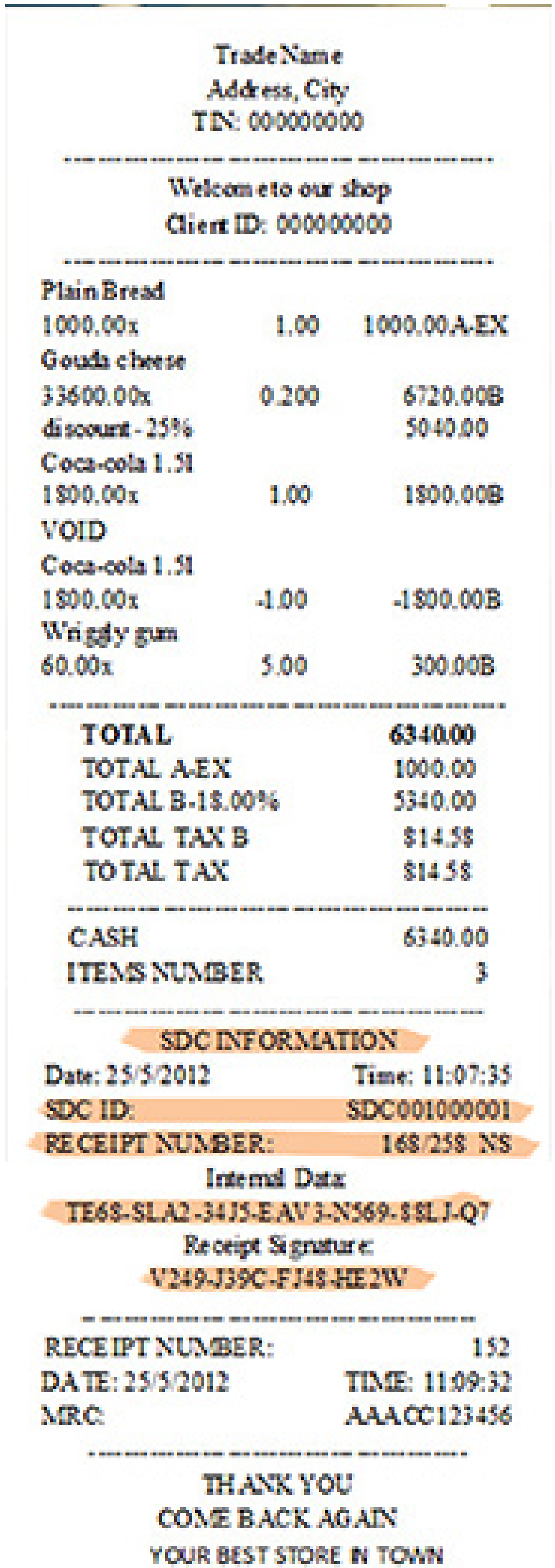
The nature of also creates sources of bias in our data; firms may seek to evade tax by over-reporting their taxable inputs or under-reporting their taxable output. Similarly, firms may under-report their turnover by neglecting to issue an EBM receipt to a (non-VAT) buyer.

A.1.1 The EBM Receipt

As described in Section 2.1, we employ electronic billing machine data from EBM devices in Rwanda to isolate product prices and sales. We present a sample EBM receipt in Figure A1. EBM II data is directly transmitted to and securely stored as data tables. We leverage the following variables from the data:

- The price of the product
- The quantity of the products sold
  - Units can be integers, or fractions (e.g., 0.200 of "Gouda cheese")
  - Negative unit values are often used by businesses to capture cancellations (note "VOID")
- A text description of the product (e.g., "Plain Bread")
- The taxpayer's identification number (TIN), and the SDC used to issue the receipt
- The purchaser's identification number (Client ID)
- The exact time and date of the sale

Figure A1: A fictitious EBM receipt



Trade Name
Address, City
TIN: 000000000

Welcome to our shop
Client ID: 000000000

Plain Bread		
1000.00x	1.00	1000.00A-EX
Gouda cheese		
33600.00x	0.200	6720.00B
discount - 25%		5040.00
Coca-cola 1.5l		
1800.00x	1.00	1800.00B
VOID		
Coca-cola 1.5l		
1800.00x	-1.00	-1800.00B
Wriggly gum		
60.00x	5.00	300.00B

TOTAL	6340.00
TOTAL A-EX	1000.00
TOTAL B-18.00%	5340.00
TOTAL TAX B	814.58
TOTAL TAX	814.58

CASH	6340.00
ITEMS NUMBER	3

SDC INFORMATION

Date: 25/5/2012	Time: 11:07:35
SDC ID:	SDC001000001
RECEIPT NUMBER:	168/258 NS

Internal Data:
TE68-SLA2-34J5-EAV3-N569-88LJ-Q7
Receipt Signature:
V249-J39C-FJ48-HE2W

RECEIPT NUMBER:	152
DATE: 25/5/2012	TIME: 11:09:32
MRC:	AAACC123456

THANK YOU
COME BACK AGAIN
YOUR BEST STORE IN TOWN

A.1.2 EBM data construction

Our analysis of the EBM data is built on a dataset of prices and quantities at the buyer sub-District-by-firm-product-by-month level. We construct the following outcomes from this data:

Product description Each item in a receipt is accompanied by a free-fill description entered by the firm (e.g., "Coca-cola 1.5l"). Firms enter these descriptions to EBM II software when they receive new stock. EBM II subsequently prints the description of each item at the point of sale.

Value and quantity To account for cancellations in the data, we aggregate over values and quantities of receipt items on the same receipt, with the same item description, the same price and the same product code. For the example in Figure A1 this creates a quantity and value of the third and fourth item will be 1 + -1 = 0 and 1800 + -1800 = 0, respectively. We remove products from our dataset which were voided or cancelled.

Price We take the following steps to improve measurement of prices. First, as described in Section 3.1.2, we employ firm-product fixed effects in all analysis of prices to ensure that we are appropriately comparing mask prices adjusting for quality differences across firm-products. Second, we note that the units in which retailers report a sale of masks within firm-product are sometimes imperfectly measured in the data. We therefore winsorize prices within firm-product at the 5th and 95th percentiles.

We identify products using string descriptions. To identify masks and paracetamol, we string match masks and paracetamol, respectively, in English, French, and Kinyarwanda (e.g., mask, masque, agapfukamunwa) in product descriptions. We subsequently remove products from this data, which satisfy the matching algorithm, but are not face masks. This includes, for example, packaging for masks ("Mask Envelope") or masks for nebulizers.

Effective mask quantity For some analysis, we construct aggregate "effective", or quality-adjusted, mask quantities. To do so, we multiply observed mask quantities by a firm-product specific measure of quality. To estimate this measure, we let p_{rft} be the price of firm-product f on receipt r from month t, and estimate

$$\log p_{rft} = \tau_t + \alpha_f + \epsilon_{rft}$$

We then construct effective mask quantity as $Q_{rft} \equiv \exp(\alpha_f) Q_{rft}^{\text{observed}}$, where $Q_{rft}^{\text{observed}}$ is the observed quantity of firm-product f on receipt r, and α_f are the estimated firm-product fixed effects from the log price regression above. In our main analysis, we construct effective mask quantity from firms of type s by summing effective quantity within buyer sub-District-by-month across firms of type s.

Product UNSPSC codes In order to determine the tax status of a product, firms classify their inventory using United Nations Standard Products and Services Code (UNSPSC) codes. We identify textile products (a product grouping) using UNSPSC codes 601058, 5310 and 2312. We ensure that the string descriptions conform to the UNSPSC product descriptions for each code.

A.1.3 EBM descriptive statistics

Table A1: Masks in EBM

	Manufacturer (1)	Retailer/Trader (2)	Importer (3)
# sub-Districts	44	137	91
# mask purchasing sub-Districts	44	135	91
# mask selling sub-Districts	14	52	15
# firms	410	1,408	1,109
# of mask purchasing firms	393	1,240	1,083
# of mask selling firms	23	285	51
# receipts	965	25,864	5,041
# of mask sales	1,025	26,261	5,112
# of masks	1,070,200	1,080,615	482,186
Mask sales, RwF	415,697,270	507,994,654	795,044,037

Notes: Summary statistics, either counts or total values, on mask sales in EBM during our study period are reported in this table. Column 1 reports statistics on sales by manufacturers, Column 2 reports statistics on sales by non-manufacturers excluding mask importers, and Column 3 reports statistics on sales by non-manufacturer mask importers.

Table A2: Mask supply chains in EBM

	Sales to (destination) sub-District (1)	Purchases from (origin) sub-District (2)
sub-Districts	2.43 (2.36)	11.29 (11.32)
Firms	3.00 (3.75)	29.57 (40.58)
Receipts	14.85 (32.86)	68.93 (105.35)

Notes: Average counts on mask sales in EBM during our study period are reported in this table, with standard deviations in parentheses. Column 1 reports counts for sales to each destination sub-District (excluding destination sub-Districts with no purchases), while Column 2 reports counts for sales from each origin sub-District (excluding origin sub-Districts with no sales).

A.2 Customs data

To compare the local and global mask markets we complement domestic EBM II data with similarly granular international trade data.

Coverage Customs data collected by the RRA contains the universe of tax-registered importing firms based in Rwanda. Importing firms are identified by the same Taxpayer Identification Number (TIN) which identifies a firm for domestic declarations (including VAT).

Import data The data includes the imported value, weight, product unit, Harmonized System (HS) product codes, an import origin and a timestamp at the point of entry. In addition, two free-text string fields are populated with product descriptions by customs officers. We identify masks using a combination of HS product codes, and product string descriptions. Just as in EBM data, we exclude items outside the scope of our study (e.g., oxygen masks).

A.2.1 Customs data construction

Product descriptions At import, product descriptions are captured by RRA customs officers at the border. We identify imports exclusively containing masks using string descriptions. We subsequently remove products from customs data, which satisfy the matching algorithm, but are not face masks.

Price The most salient difference for our research, between customs and EBM data is that no natural price (per unit) variable exists in the international trade data. The data does include the total imported value, as measured by the Cost, Insurance, Freight (CIF), and several quantity variables including *product quantity* and Net Weight. As discussed in Appendix A.1 units of quantity may vary. We use the import value and weight to construct a mask price per unit of weight for transaction r made at the firm-border post combination f in month t.

$$p_{rft}^{\text{imp}} = \frac{\text{CIF}_{rft}}{\text{KG}_{rft}}$$

We winsorize border prices at the 5th and 95th percentile.

A.3 Additional appendix data sources

VAT Firms with turnover equal to or in excess of RwF 20 million ($20,000) annual turnover (or the RwF 5 million ($5,000), for three consecutive quarters) are required to file VAT. Among those filing VAT, small firms may file quarterly, while large are required to declare by the 15th of each month. Firms who file VAT are required to report their sales using an EBM device. EBM II data is subsequently used to verify VAT declarations.

A VAT declaration reports the total (taxable and non-taxable) sales of a firm. Since this includes both EBM I and EBM II, VAT is a more comprehensive measure of turnover. Since a VAT liability is determined by taxable outputs and inputs and the statutory tax rate (18%), a VAT declaration requires a firm to report total tax paid on inputs, only.[A5] VAT does not require declarations of non-taxable input, as well as inputs from non-VAT registered vendors. Consequently, EBM is often a more complete measure of a firm's aggregate input.

In Appendix E.1, we use VAT turnover data to test the exogeneity of mask manufacturing exposure. In Appendix D, we use VAT turnover data to to measure firm growth caused by licensing.

PAYE Firms with employees are required to declare tax withholding on behalf of their employees. Large firms declare PAYE on a monthly basis. With permission, small firms declare PAYE on a quarterly basis. Employers who compensate an employee by 30,000 RwF (30 USD) or more per month are required to declare and pay PAYE on behalf of the employee.[A6]

A PAYE declaration aggregates the total pay, allowances and benefit-in-kind (BIK) provided to the firm's employees. In an annex to the declaration, firms enumerate each employee, and report the compensation paid to each individual.

[A5] The tax code in Rwanda also provides for exemptions and a 0% rate for some products. See the VAT law (https://www.rra.gov.rw/index.php?id=36) for more detail.

[A6] Additionally, firms who declare pension or other benefits on behalf of their employees may submit a joint declaration, including PAYE. In cases where the income of the employee is less than 30,000 RwF (30 USD) per month this will not give rise to a tax withholding liability.

In Appendix D we use firm reported employment and wage bill to understand the impact of licensing on firm growth.

Firm registration Firm registration data contains firm-level details including the ISIC sector classification, and the Province, District and sub-District in which the firm operates. This data is collected when the firm is formally registered at the Rwanda Development Board (RDB) and continuously monitored and updated by the RRA.

A.4 Time series of mask prices

To construct our time series of mask prices, we estimate the following equations to document changes in both domestic prices and border prices of masks in Rwanda during the COVID-19 pandemic. For both domestic and imported masks, we consider transactions between November 2019 and August 2020.

Domestic mask prices To account for the composition of domestic mask sales, we construct prices as mean log prices at the month-by-firm-product level. We let p_{ft} be the mean log price of firm-product f sold domestically in month t. We then estimate

$$\log p_{ft} = \sum_{t=\text{Nov 2019}, t\neq \text{Jan 2020}}^{\text{Aug 2020}} \tau_t + \alpha_f + \epsilon_{ft} \tag{A1}$$

where τ_t captures the change in log mask prices relative to January 2020, and α_f is a firm-product fixed effect that controls for any changes in the composition of masks sold domestically. We report standard errors clustered at the sub-District of the firm. As discussed in Section 2.2, starting April 2020 sales of domestically manufactured multi-layer masks grew relative to sales of imported medical masks.

Border mask prices For border prices, we similarly construct mean log prices per unit at the month-by-firm-border post level. Since unit mask prices are not naturally identified in the customs data, we prices per unit of weight, net of freight weight.[A7] Further, as we are unable to consistently identify products in the customs data, we use firm-border post fixed effects to account for changes in the composition of mask imports. We let p_{ft}^{imp} be the mean log price of imported masks in customs data for firm-border post f in month t.

$$\log p_{ft}^{\text{imp}} = \sum_{t=\text{Nov 2019}, t \neq \text{Jan 2020}}^{\text{Aug 2020}} \tau_t^{\text{imp}} + \alpha_f + \epsilon_{ft}^{\text{imp}} \tag{A2}$$

where τ_t^{imp} captures the change in log border prices of masks relative to January 2020, and α_f is a firm-border post fixed effect that controls for any changes in the composition of imported masks, assuming that variation in quality of imported masks and other unobservable determinants of price does not systematically vary over time within firm-border post. We report standard errors clustered at the firm level.

B Mask specifications and enforcement

B.1 Definition of mask quality

Guidelines for the manufacture of barrier masks were released by the Rwanda FDA[A8] and the Rwanda RSB[A9] on April 17 and April 24, respectively. The guidelines released by the RSB were gazetted in Rwandan Law in June 2020.[A10] The guidelines include references to:

- Performance: ISO specifications for penetration (for solid and liquid particles), air permanence (315-1265 μm/Pa.s), and mass per unit area (120-250 g/m^2)
- Materials: cotton, viscose, polyester; multiple layers preferred

[A7] See Appendix A.2 for additional details on this construction.
[A8] https://rwandafda.gov.rw/web/index.php?id=36
[A9] https://www.rsb.gov.rw/fileadmin/user_upload/files/pdf/new_stds/RS_433-2_2020.pdf
[A10] https://www.rsb.gov.rw/fileadmin/user_upload/files/pdf/new_stds/National_Standards_May_2020.pdf

- Size: Four adult size specifications and three child specifications detailed (e.g., a small adult mask should measure 280mm-306mm x 104mm-111mm)
- Labelling: The manufacturer's name, the constituent material, recommended use period, handling instructions
- Packaging: Masks should be packaged to protect masks from contamination or damage

B.2 Enforcement

Objective ISO testing metrics are outlined in the RSB's standards guidelines, which were gazetted into law in June 2020. Individual firms subsequently report subjecting their masks to quality checks. As a supporting anecdote, Chillington Rwanda (with support from the RSB) provided masks to the FDA for approval.[A11] As another, UFACO & VLISCO NL LTD reported inspections and compliance with the RSB standards before the masks were leave the factory gate.[A12]

In an interview on May 7 with Kigali Today, the Quality Control Division Manager at the RSB and the Director of Engineering Standards at the FDA discussed the issue of facemask quality at markets, and underscored their role as quality auditors; masks found to be non-compliant with standards were removed from markets, packaging should comply with the RSB standards:[A13]

> *After the policies were published* [April 17] *we started the inspection which makes me think that all face masks being manufactured now meet all the required standards*
>
> Director of Engineering Standards, FDA

[A11] https://www.msdhub.org/blog/learning-story-spotlight-chillington-rwanda-pathways-for-adaptation-anc
[A12] http://expressnews.rw/are-face-masks-safe-with-health-standards-for-users/
[A13] https://www.youtube.com/watch?v=en8RdSauF2g

C Comparison to existing estimates of impacts of masks on COVID-19 infections

To compare our estimates of the impacts of mask manufacturing exposure on purchases of anti-fever medicine to existing estimates of the impacts of mask wearing on COVID-19 infections, we first provide assumptions under which purchases of anti-fever medicine are a valid proxy for COVID-19 infections. To interpret purchases of anti-fever medicine as a proxy for COVID-19 infections, we make two key assumptions, closely related to assumptions in Abaluck et al. (2021) under which symptomatic seroprevalence for COVID-19 is a valid proxy for symptomatic seroconversions. First, we assume that the percentage impacts of mask manufacturing exposure on use of anti-fever medicine caused by COVID-19 are equal to the percentage impacts of mask manufacturing exposure on COVID-19 infections. That is, mask manufacturing exposure proportionally decreases COVID-19 infections that do and do not result in use of anti-fever medicine. Second, we assume that the percentage impacts of mask manufacturing exposure on use of anti-fever medicine caused by COVID-19 are equal to the percentage impacts of mask manufacturing exposure on the use of anti-fever medicine. This would be true if, for example, all use of anti-fever medicine is caused by flu-like illnesses, and mask manufacturing exposure causes identical percentage decreases in COVID-19 infections and other flu-like illnesses. In Appendix G, we show that increases in purchases of anti-fever medicine are associated with increases in confirmed COVID-19 cases, but our estimates are noisy due to relatively infrequent testing during our study period. If both of these assumptions are true, then the percentage decrease in sales of anti-fever medicine caused by mask manufacturing exposure is equal to the percentage decrease in COVID-19 infections.

Next, we scale our estimates of impacts of mask manufacturing exposure for comparability. We focus on our estimates without controls, and in particular of our estimate of the impacts of mask manufacturing exposure on June sales of anti-fever medicine as a proxy for

COVID-19 infections. We divide this estimate by 3 to recover impacts on monthly growth rates in infections – an increase in mask manufacturing exposure from 0 to 1 causes the monthly growth rate of COVID-19 infections (defined as the ratio of COVID-19 infections in the current month to COVID-19 infections in the previous month) to fall by 48%. We then take two approaches to scaling this estimate. First, to evaluate the overall effect of mask manufacturing in Rwanda, we multiply our estimates by the average mask manufacturing exposure in our primary analysis sample (0.187), and interpret this as the effect of setting mask manufacturing exposure equal to zero nationally. Alternatively, we divide our estimate by the April impact on share of mask purchases from manufacturers (1.095), which we interpret as the effect of households exclusively sourcing masks from manufacturers. Applying the former, domestic mask manufacturing reduced the monthly growth rate of COVID-19 infections nationally by at least April through June. Applying the latter, shifting from no manufactured masks to complete adoption of manufactured masks causes the monthly growth rate of COVID-19 infections to fall by 45%.

We then compare this estimate to results from Chernozhukov et al. (2021) and Abaluck et al. (2021). Chernozhukov et al. (2021) find that mandating employees wear masks holding fixed behavior causes a 10% decrease in weekly case growth, which scales to a 35% decrease in monthly case growth; this is statistically indistinguishable from our estimate of the impacts of adoption of manufactured masks. Abaluck et al. (2021) find that promotion of masks led to a 29pp increase in mask wearing in public, which over 8 weeks was associated with an 11.2% reduction in symptomatic infections when surgical masks were provided. Scaling this estimate by the inverse of their estimated impacts on mask adoption yields an 19% decrease in monthly infection growth. These estimates suggest larger impacts of masks on the spread of COVID-19 in the United States and in urban and peri-urban Rwanda than in rural Bangladesh; alternatively, the source of variation in mask policy in Bangladesh is at a much narrower geography (village) than in Rwanda and in the United States, suggesting across-village spillovers may reduce estimated impacts of masks on the spread of COVID-19 in Bangladesh.

D Impacts of licenses to produce masks on firms

D.1 Data

Sample As discussed in Section 2.2, the FDA provided licenses to garment manufacturers for the production of barrier masks. We use the published list of licensed manufacturers to identify 38 licensed manufacturers and identify an additional 43 garment manufacturers using a combination of firm registration data and EBM product descriptions. Firm names, as published by the FDA, were matched to TINs by RRA staff.

Data We construct a balanced panel of firms and aggregate outcomes to the firm-by-quarter level. We construct outcomes from VAT, PAYE, and EBM data, and also use customs data to construct pre-period firm characteristics.

D.2 Empirical strategy

To avert concerns pertaining to the impact of several heterogeneous economic shocks at the pandemic's outset, we study the impact of licensing on firms within the garment manufacturing sector, only. For a discussion of the sectoral heterogeneity underlying the COVID-19 shock in Rwanda, see Byrne et al. (2021).

We consider two approaches to identifying the impact of licensing on firms. Firstly, we assume parallel trends in outcomes for licensed and non-licensed textile manufacturers absent licensing. Under this assumption, we estimate the impact of licensing on firm i in quarter q, with firm (θ_i) and quarter (γ_q) fixed effects:

$$y_{iq} = \sum_{q=2019Q4, q \neq 2020Q1}^{2020Q2} \beta_q D_i + \theta_i + \gamma_q + \epsilon_{iq} \tag{A3}$$

where y_{iq} is an observable business outcome and D_i is an indicator that firm i was licensed to manufacture masks. We produce estimates by either Poisson pseudo maximum likelihood, to flexibly handle zeros in our main firm outcomes, or OLS. All impacts are relative to 2020Q1 (January 2020 through March 2020), the quarter before the April licensing of domestic textile manufacturers.

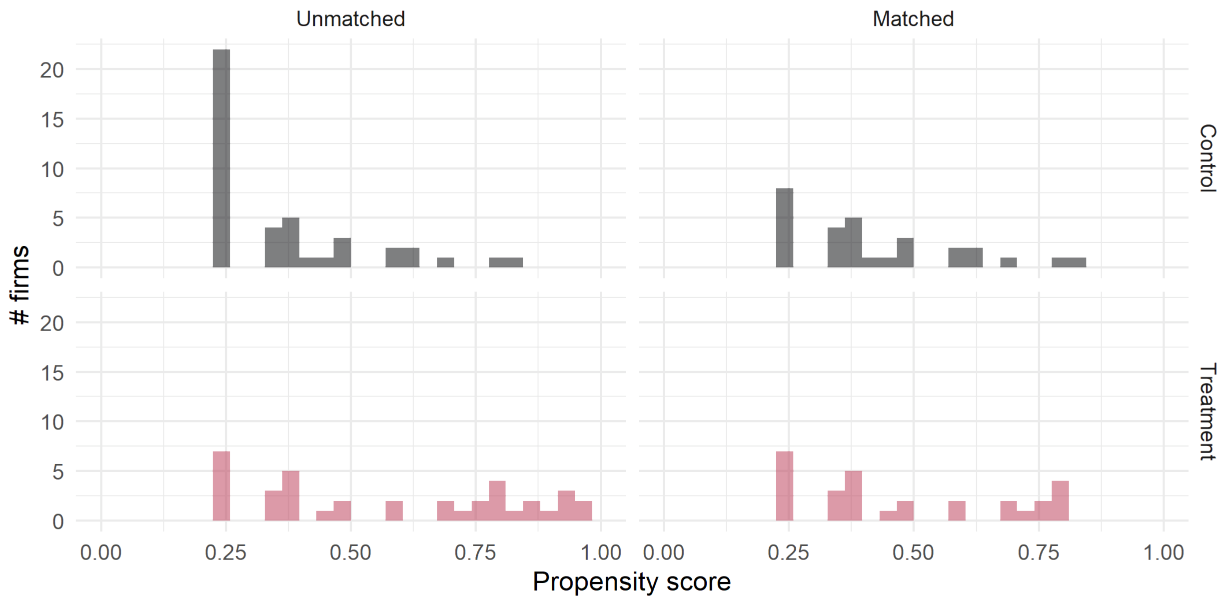
However, the parallel trends assumption required to causally interpret β may not hold. As noted in Section 2.2, firm licenses were issued following applications to the Rwanda FDA; one possible concern is that larger firms with better access to inputs may be more likely to apply for licenses. To address concerns the licensed firms may be larger or have better access to inputs, we instead relax our parallel trends assumption, assuming it only holds conditional on observable baseline firm characteristics. Specifically, we use log employment and an importer indicator to construct a propensity matched sample of licensed and non-licensed firms. We follow Ho et al. (2007) and create a balanced sample across licensed and non-licensed firms, and enforce common support through trimming (see Cunningham (2021) for a discussion of trimming in propensity score designs). We then follow Sant'Anna & Zhao (2020) and estimate a doubly robust difference-in-difference specification, controlling for the matching covariates X_i for firm i, while also restricting to the propensity score matched sample.

$$y_{iq} = \sum_{q=2019Q4, q \neq 2020Q1}^{2020Q2} \beta_q D_i + X_i' \eta_q + \theta_i + \gamma_q + \epsilon_{iq} \tag{A4}$$

D.3 Balance

The matched and unmatched kernel densities of the estimated propensity score are displayed in Figure A2. After matching, the two distributions appear more similar, with fewer low propensity score non-licensed observations and high propensity score licensed observations.

Figure A2: Distribution of propensity scores across non-licensed and licensed firms are more similar after matching



Next, we demonstrate that matched licensed and non-licensed firms have similar baseline observable characteristics in Table Appendix A3.

Table A3: Propensity matched licensed and non-licensed textile manufacturers exhibit statistically similar baseline observables

	Mask Manufacturers			Textile Manufacturers			Difference
	Mean	Std. dev	# obs	Mean	Std. dev	# obs	p value
Turnover (VAT)							
Inputs (RWF Mn)	4.850	6.758	29	7.402	10.191	29	0.267
Sales (RWF Mn)	43.002	101.186	29	22.136	35.881	29	0.302
Transactions (EBM)							
Receipts	54.870	91.001	23	193.963	719.514	27	0.328
Buyers	15.783	32.072	23	67.259	216.772	27	0.233
Sellers	14.793	15.363	29	14.759	15.652	29	0.993
Products	22.652	33.793	23	32.889	33.578	27	0.290
Textile sale share	0.319	0.433	23	0.206	0.350	27	0.321
Labour (PAYE)							
Employees	6.822	9.373	29	3.437	6.628	29	0.119
Labor Cost (RWF Mn)	4.744	8.964	29	1.152	2.440	29	0.045
International Trade (Customs)							
Imports (RWF Mn)	13.044	44.093	29	7.059	18.733	29	0.505
Imports (KG 000)	3.441	11.501	29	3.000	9.221	29	0.872

D.4 Results

We present the impacts of licenses to manufacture masks on textile manufactures in Table A4. We report estimates using both Equations A3 (with no matching and no additional controls) and A4 (with matching and additional controls).

First, we find no evidence of pre-licensing trends in outcomes across specifications with and without matching. We interpret this as consistent with our assumption of parallel trends absent licensing. Second, we estimate that licensing increases mask sales as a fraction of turnover in the first three months of licensing by 42pp. This "first stage" estimate demonstrates that licensed firms substantively increased their production of masks. Third, we find suggestive evidence that firms reduced their non-mask turnover. This result is less robust and more imprecise; however, we interpret it as consistent with licensed garment manufacturers substituting away from production of other textiles, and into the production of masks, potentially due to input adjustment frictions. Lastly, we fail to find evidence of impacts of licensing on firm scale for any other business outcomes, including total turnover, remuneration, and employment. This lack of impacts on firm scale can be explained by garment manufacturers making competitive entry decisions into mask manufacturing.

Table A4: Licensing causes firms to shift into mask production without firm growth

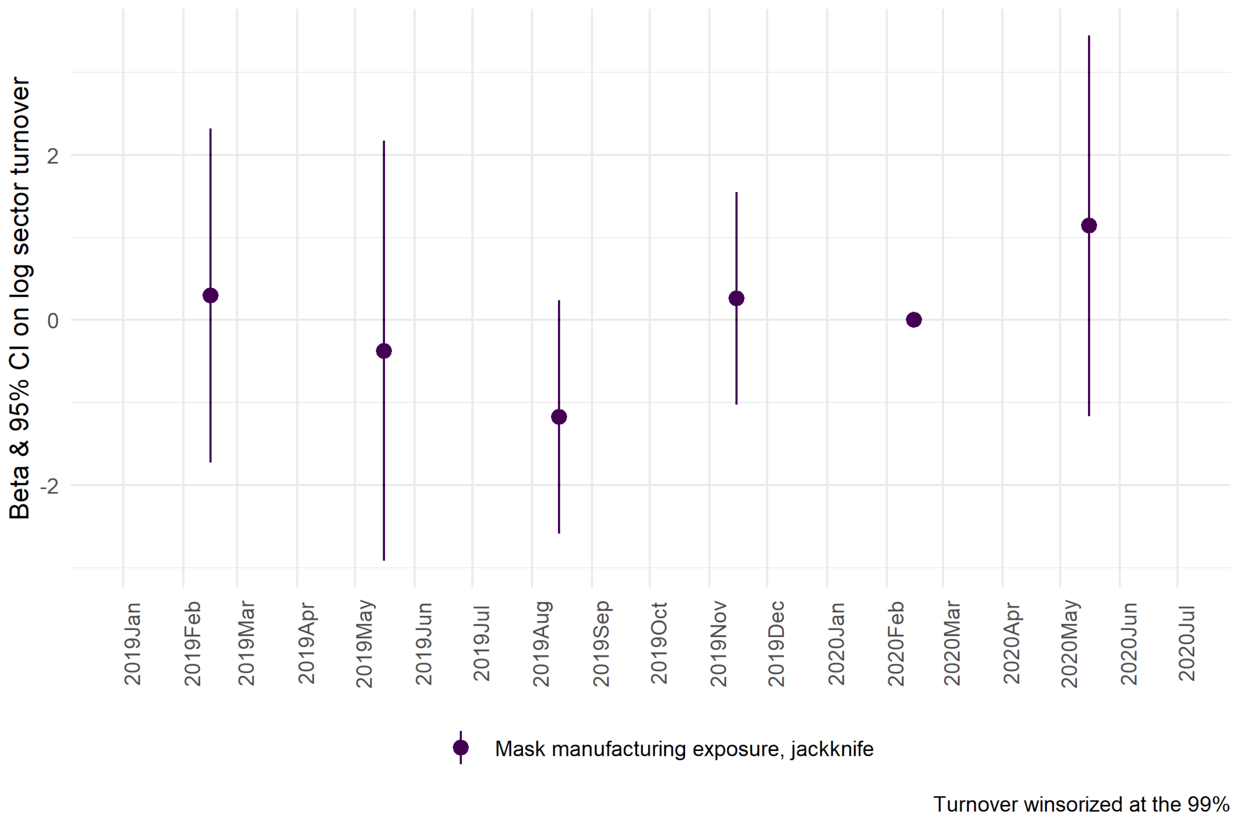
	Dependent variable:					
	Mask Turnover Share	Non Mask Turnover	Turnover	Turnover	Labor Pay	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: Unmatched						
	-0.038	-0.306	-0.323	0.022	0.206	0.181
Treat x Q4 (u)	(0.036)	(0.317)	(0.313)	(0.139)	(0.176)	(0.204)
	[0.298]	[0.334]	[0.302]	[0.871]	[0.240]	[0.375]
Treat x Q1 (u)	-	-	-	-	-	-
	0.420	-0.422	0.080	0.137	0.358	-0.003
Treat x Q2 (u)	(0.098)	(0.238)	(0.258)	(0.209)	(0.291)	(0.193)
	[0.000]	[0.076]	[0.756]	[0.513]	[0.219]	[0.989]
# observations (u)	134	150	153	234	117	117
Controls x Quarter FE (u)						
Panel II: Matched						
	-0.008	0.281	0.146	-0.168	0.081	0.473
Treat x Q4 (m)	(0.039)	(0.338)	(0.318)	(0.171)	(0.340)	(0.502)
	[0.846]	[0.406]	[0.646]	[0.327]	[0.812]	[0.346]
Treat x Q1 (m)	-	-	-	-	-	-
	0.428	-0.423	-0.210	0.273	0.052	-0.013
Treat x Q2 (m)	(0.124)	(0.305)	(0.256)	(0.192)	(0.454)	(0.306)
	[0.001]	[0.166]	[0.411]	[0.155]	[0.909]	[0.965]
# observations (m)	96	108	111	165	90	90
Controls x Quarter FE (m)	X	X	X	X	X	X
Data	EBM	EBM	EBM	VAT	PAYE	PAYE
Estimation method	OLS	Poisson	Poisson	Poisson	Poisson	Poisson
Firm FE	X	X	X	X	X	X
Quarter FE	X	X	X	X	X	X

E Additional placebo checks

E.1 Impacts of manufactured mask exposure on turnover in VAT

As discussed in Appendix A.1, our primary outcomes in our analysis of the impacts of mask manufacturing exposure in Section 3 are only observable in EBM II, which lacks sufficient coverage prior to November 2019. As a complement, we leverage value added tax data (described in Appendix A.3) on total sub-District turnover that we extend back to the first quarter of 2019 to test for parallel trends prior to the availability of EBM II data. We estimate Equation 2, our primary estimating equation for the impacts of mask manufacturing exposure, over quarters instead of months and with the first quarter of 2020 as the omitted quarter, and present estimated quarterly coefficients in Figure A3. We find no evidence that mask manufacturing exposure had impacts on sub-District turnover up to one year prior to licensing, although our confidence intervals include relatively large impacts of mask manufacturing exposure on turnover. We interpret this result as consistent with our assumption of the exogeneity of mask manufacturing exposure.

Figure A3: No impacts of mask manufacturing exposure on turnover before licensing



F Robustness of estimated impacts of manufactured mask exposure

F.1 Without jackknife

In Section 3.1.1, we constructed our measure of destination sub-District mask manufacturing exposure in Equation 1 by calculating average mask manufacturing intensity across origin sub-Districts weighted by the destination sub-Districts share of non-mask textiles sourced from each origin sub-District *except* the destination sub-District. This "jackknife" procedure is intended to alleviate the following endogeneity concern: if sub-Districts source a large share of their textiles locally, including masks, then a large mask demand shock could cause an increase in local mask manufacturing intensity, causing mask manufacturing exposure to be endogenous to mask demand. Although this concern would not affect our results in Table 2 for which we use our jackknife construction of mask manufacturing exposure, we nonetheless test for this possibility by replicating our results on balance with respect to and impacts of mask manufacturing exposure in Tables 1 and 2, respectively, constructing mask manufacturing exposure without excluding non-mask textiles sourced from the destination sub-District. We report our results without jackknife on balance and impacts in Tables A5 and A6, respectively. The patterns in the results we describe in Sections 3.2 and 3.3 are unaffected by constructing mask manufacturing exposure without jackknife.

Table A5: Mask manufacturing exposure without jackknife remains uncorrelated with sub-District observables

	Dependent variable:					
	Mask manufacturing intensity		Mask manufacturing exposure			
	(1)	(2)	(3)	(4)	(5)	(6)
log EBM turnover, RwF Bn	−0.000	−0.080	−0.025	−0.027	−0.019	−0.020
	(0.076)	(0.096)	(0.015)	(0.016)	(0.017)	(0.019)
	[0.997]	[0.408]	[0.098]	[0.096]	[0.266]	[0.278]
log EBM input, RwF Bn	−0.115	0.017	0.058	0.067	0.050	0.053
	(0.124)	(0.151)	(0.033)	(0.034)	(0.038)	(0.039)
	[0.361]	[0.910]	[0.084]	[0.056]	[0.194]	[0.180]
log population density	−0.104	−0.080	−0.039	−0.041	−0.035	−0.050
	(0.068)	(0.066)	(0.039)	(0.037)	(0.045)	(0.042)
	[0.136]	[0.234]	[0.326]	[0.268]	[0.437]	[0.244]
% completed primary school	0.015	0.000	−0.001	−0.002	0.001	0.002
	(0.018)	(0.019)	(0.002)	(0.002)	(0.002)	(0.003)
	[0.392]	[0.999]	[0.715]	[0.418]	[0.559]	[0.514]
% employed	−0.012	0.009	−0.013	−0.004	−0.008	−0.002
	(0.024)	(0.026)	(0.009)	(0.009)	(0.010)	(0.011)
	[0.628]	[0.732]	[0.175]	[0.669]	[0.416]	[0.867]
log textile manufacturers	0.183	0.169	−0.028	−0.024	−0.015	−0.007
	(0.131)	(0.128)	(0.036)	(0.036)	(0.044)	(0.044)
	[0.173]	[0.198]	[0.445]	[0.507]	[0.734]	[0.874]
log population	0.069	0.055	−0.010	−0.033	−0.037	−0.042
	(0.163)	(0.145)	(0.051)	(0.064)	(0.055)	(0.071)
	[0.677]	[0.708]	[0.849]	[0.611]	[0.501]	[0.560]
Province FE		X		X		X
Residualized shocks					X	X
# observations	43	43	88	88	88	88
# clusters (sub-Districts)	43	43	88	88	88	88
Omnibus F	0.63	0.99	1.1	0.97	1.55	0.83
	[0.728]	[0.454]	[0.373]	[0.460]	[0.163]	[0.569]

Table A6: Mask manufacturing exposure without jackknife increases purchases of masks from manufacturers, decreases mask prices, and reduces COVID-19 infections

	Dependent variable:					
	log buyer price		$\frac{\text{Eff mask Q from manufacturers}}{\text{Eff mask Q}}$		# paracetamol receipts	
	(1)	(2)	(3)	(4)	(5)	(6)
Mask manufacturing exposure$_d$ × Nov$_t$	0.296 (0.646) [0.648]	0.287 (0.630) [0.649]	-0.011 (0.106) [0.914]	0.142 (0.194) [0.467]	0.196 (0.883) [0.824]	-0.092 (0.781) [0.906]
Mask manufacturing exposure$_d$ × Dec$_t$	0.221 (0.725) [0.761]	0.319 (0.740) [0.667]	0.033 (0.097) [0.736]	0.121 (0.208) [0.562]	-0.083 (0.623) [0.894]	-0.277 (0.549) [0.614]
Mask manufacturing exposure$_d$ × Jan$_t$	0.057 (0.403) [0.887]	-0.097 (0.333) [0.771]	0.019 (0.100) [0.853]	0.256 (0.209) [0.223]	0.077 (0.624) [0.902]	0.363 (0.647) [0.574]
Mask manufacturing exposure$_d$ × Feb$_t$	0.396 (0.342) [0.249]	0.423 (0.299) [0.161]	0.018 (0.079) [0.823]	0.138 (0.181) [0.448]	0.012 (0.700) [0.986]	0.529 (0.741) [0.476]
Mask manufacturing exposure$_d$ × Mar$_t$	-	-	-	-	-	-
Mask manufacturing exposure$_d$ × Apr$_t$	-0.484 (0.303) [0.113]	-0.566 (0.288) [0.053]	1.165 (0.226) [0.000]	0.985 (0.399) [0.015]	0.104 (0.818) [0.899]	-0.445 (0.953) [0.641]
Mask manufacturing exposure$_d$ × May$_t$	-0.545 (0.311) [0.083]	-0.637 (0.280) [0.025]	0.578 (0.158) [0.000]	0.437 (0.299) [0.147]	-1.107 (0.641) [0.084]	-1.091 (0.578) [0.059]
Mask manufacturing exposure$_d$ × Jun$_t$	-0.617 (0.382) [0.110]	-0.719 (0.397) [0.074]	0.135 (0.177) [0.446]	0.511 (0.289) [0.080]	-1.798 (0.706) [0.011]	-1.639 (0.583) [0.005]
Mask manufacturing exposure$_d$ × Jul$_t$	-0.184 (0.301) [0.543]	-0.255 (0.292) [0.386]	0.403 (0.143) [0.006]	0.414 (0.256) [0.109]	-1.719 (0.760) [0.024]	-1.536 (0.725) [0.034]
Mask manufacturing exposure$_d$ × Aug$_t$	-0.470 (0.269) [0.084]	-0.550 (0.231) [0.019]	-0.088 (0.189) [0.643]	0.028 (0.308) [0.927]	-1.459 (0.828) [0.078]	-1.473 (0.835) [0.078]
Estimation method	OLS	OLS	OLS	OLS	Poisson	Poisson
Firm-product FE	X	X				
Destination sub-District FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Residualized shocks		X		X		X
Controls × Month FE		X		X		X
# observations	3,942	3,942	511	511	800	800
# clusters (sub-Districts)	88	88	88	88	80	80

F.2 Parallel pre-trends pre-test robustness

In Table 2, we both test for parallel pre-trends with respect to mask manufacturing exposure and estimate impacts of mask manufacturing exposure; when analysis is reported conditional on passing tests for parallel pre-trends, estimates of impacts and inference may be biased (Roth, 2021). However, Borusyak et al. (2021b) demonstrate that that under homoskedasticity, this concern is eliminated in specifications that pool across pre-treatment periods to estimate a counterfactual for post-treatment periods. Specifically, we modify Equation 2 and estimate

$$y_{dt} = \sum_{t=\text{Apr 2020}}^{\text{Aug 2020}} \beta_t \text{Mask manufacturing exposure}_d + X_d' \delta_t + \theta_d + \gamma_t + \epsilon_{dt} \tag{A5}$$

Equation A5 differs from Equation 2 in that it imposes parallel trends prior to treatment, separating tests of parallel trends from estimation. We report estimates of Equation A5 in Table A7. The patterns in the results we describe in Section 3.3 are unaffected by imposing parallel trends prior to treatment.

Table A7: Mask manufacturing exposure increases purchases of masks from manufacturers, decreases mask prices, and reduces COVID-19 infections when parallel pre-trends are imposed

	Dependent variable:					
	log buyer price		$\frac{\text{Eff mask Q from manufacturers}}{\text{Eff mask Q}}$		# paracetamol receipts	
	(1)	(2)	(3)	(4)	(5)	(6)
Mask manufacturing exposure$_d$ × Pre$_t$	-	-	-	-	-	-
Mask manufacturing exposure$_d$ × Apr$_t$	-0.768 (0.171) [0.000]	-0.678 (0.214) [0.002]	1.076 (0.220) [0.000]	0.853 (0.353) [0.018]	0.047 (1.150) [0.967]	-0.244 (1.083) [0.822]
Mask manufacturing exposure$_d$ × May$_t$	-0.925 (0.190) [0.000]	-0.753 (0.229) [0.001]	0.511 (0.161) [0.002]	0.370 (0.231) [0.113]	-1.338 (1.029) [0.193]	-1.473 (0.995) [0.139]
Mask manufacturing exposure$_d$ × Jun$_t$	-0.890 (0.257) [0.001]	-0.870 (0.364) [0.019]	0.178 (0.174) [0.309]	0.393 (0.223) [0.081]	-2.127 (1.076) [0.048]	-1.892 (0.942) [0.045]
Mask manufacturing exposure$_d$ × Jul$_t$	-0.441 (0.222) [0.051]	-0.395 (0.244) [0.110]	0.423 (0.126) [0.001]	0.278 (0.185) [0.136]	-2.132 (1.142) [0.062]	-1.853 (0.987) [0.061]
Mask manufacturing exposure$_d$ × Aug$_t$	-0.735 (0.139) [0.000]	-0.684 (0.153) [0.000]	-0.156 (0.184) [0.401]	-0.088 (0.248) [0.724]	-1.894 (1.312) [0.149]	-1.668 (1.094) [0.127]
Estimation method	OLS	OLS	OLS	OLS	Poisson	Poisson
Firm-product FE	X	X				
Destination sub-District FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Residualized shocks		X		X		X
Controls x Month FE		X		X		X
# observations	3,937	3,937	506	506	780	780
# clusters (sub-Districts)	86	86	86	86	78	78

F.3 Controls

In Section 3.3, we presented impacts of mask manufacturing exposure using two specifications: first, one that omits controls interacted with month fixed effects and does not residualize mask manufacturing intensity on controls before constructing mask manufacturing exposure, and second, one that includes controls interacted with month fixed effects and residualizes mask manufacturing intensity on controls before constructing mask manufacturing exposure. These specifications correspond to the balance tests in Columns 3 and 6 of Table 1. In Table A8, we also present impacts of mask manufacturing exposure with two additional specifications corresponding to the balance tests in Columns 4 and 5 of Table 1: first, one the includes controls interacted with month fixed effects but does not residualize mask manufacturing intensity on controls before constructing mask manufacturing exposure, and second, one that does not include controls interacted with month fixed effects but does residualize mask manufacturing intensity on controls before constructing mask manufacturing exposure. In general, the patterns in the results we describe in Section 3.3 are robust to these two alternate specifications; we note that impacts on mask purchases from manufacturers become larger in specifications that residualize mask manufacturing intensity on controls before constructing mask manufacturing exposure, and become smaller in specifications that include controls interacted with month fixed effects, and as a result specifications that either do neither or both of these have similar effects, but this does not qualitatively affect our results.

Table A8: Mask manufacturing exposure increases purchases of masks from manufacturers, decreases mask prices, and reduces COVID-19 infections across alternative approaches to including controls

	Dependent variable:											
	log buyer price				$\frac{\text{Eff mask Q from manufacturers}}{\text{Eff mask Q}}$				# paracetamol receipts			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Mask manufacturing exposure$_d$ × Nov$_t$	0.445 (0.680) [0.514]	0.437 (0.640) [0.496]	0.219 (0.654) [0.738]	0.263 (0.666) [0.694]	-0.016 (0.123) [0.897]	0.256 (0.198) [0.200]	-0.088 (0.156) [0.573]	0.155 (0.202) [0.444]	0.588 (1.040) [0.572]	-0.021 (0.961) [0.983]	0.611 (0.957) [0.524]	0.029 (0.859) [0.973]
Mask manufacturing exposure$_d$ × Dec$_t$	0.173 (0.732) [0.814]	-0.601 (0.734) [0.415]	0.293 (0.648) [0.653]	0.199 (0.717) [0.782]	0.035 (0.114) [0.760]	0.281 (0.202) [0.168]	-0.042 (0.142) [0.770]	0.135 (0.214) [0.531]	0.267 (0.734) [0.716]	-0.182 (0.690) [0.792]	0.132 (0.612) [0.829]	-0.194 (0.618) [0.753]
Mask manufacturing exposure$_d$ × Jan$_t$	0.296 (0.383) [0.441]	0.107 (0.349) [0.760]	0.060 (0.390) [0.878]	-0.101 (0.343) [0.769]	0.057 (0.115) [0.620]	0.237 (0.208) [0.259]	0.034 (0.143) [0.815]	0.224 (0.211) [0.290]	0.254 (0.685) [0.711]	0.366 (0.760) [0.630]	0.058 (0.558) [0.917]	0.415 (0.685) [0.545]
Mask manufacturing exposure$_d$ × Feb$_t$	0.367 (0.374) [0.328]	0.273 (0.330) [0.410]	0.482 (0.337) [0.156]	0.393 (0.318) [0.219]	0.046 (0.089) [0.608]	0.189 (0.185) [0.308]	-0.008 (0.114) [0.947]	0.130 (0.181) [0.475]	-0.080 (0.723) [0.912]	0.413 (0.857) [0.629]	-0.175 (0.615) [0.775]	0.524 (0.752) [0.485]
Mask manufacturing exposure$_d$ × Mar$_t$	-	-	-	-	-	-	-	-	-	-	-	-
Mask manufacturing exposure$_d$ × Apr$_t$	-0.521 (0.311) [0.098]	-0.574 (0.321) [0.078]	-0.606 (0.289) [0.039]	-0.600 (0.300) [0.049]	1.095 (0.252) [0.000]	1.377 (0.285) [0.000]	0.697 (0.356) [0.054]	0.973 (0.413) [0.021]	0.208 (0.832) [0.803]	0.110 (0.888) [0.901]	-0.174 (0.809) [0.829]	-0.393 (0.953) [0.680]
Mask manufacturing exposure$_d$ × May$_t$	-0.613 (0.327) [0.064]	-0.611 (0.292) [0.040]	-0.772 (0.344) [0.027]	-0.673 (0.290) [0.023]	0.533 (0.179) [0.004]	0.724 (0.276) [0.010]	0.404 (0.221) [0.071]	0.464 (0.303) [0.130]	-1.178 (0.730) [0.107]	-0.689 (0.584) [0.238]	-1.405 (0.752) [0.062]	-1.056 (0.565) [0.062]
Mask manufacturing exposure$_d$ × Jun$_t$	-0.671 (0.391) [0.089]	-0.723 (0.418) [0.088]	-0.792 (0.396) [0.049]	-0.792 (0.423) [0.065]	0.196 (0.194) [0.316]	0.581 (0.349) [0.099]	0.212 (0.215) [0.328]	0.513 (0.293) [0.083]	-1.968 (0.728) [0.007]	-1.561 (0.626) [0.013]	-1.824 (0.698) [0.009]	-1.669 (0.571) [0.003]
Mask manufacturing exposure$_d$ × Jul$_t$	-0.210 (0.309) [0.499]	-0.295 (0.319) [0.357]	-0.396 (0.317) [0.215]	-0.316 (0.314) [0.318]	0.438 (0.136) [0.002]	0.656 (0.262) [0.014]	0.237 (0.188) [0.211]	0.398 (0.262) [0.132]	-1.973 (0.825) [0.017]	-1.668 (0.841) [0.047]	-1.785 (0.749) [0.017]	-1.619 (0.753) [0.032]
Mask manufacturing exposure$_d$ × Aug$_t$	-0.460 (0.272) [0.095]	-0.663 (0.284) [0.022]	-0.498 (0.251) [0.050]	-0.606 (0.251) [0.018]	-0.138 (0.197) [0.485]	0.174 (0.315) [0.583]	-0.318 (0.253) [0.212]	0.031 (0.311) [0.920]	-1.734 (0.918) [0.059]	-1.531 (1.007) [0.129]	-1.600 (0.785) [0.041]	-1.564 (0.862) [0.070]
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	Poisson	Poisson	Poisson	Poisson
Firm-product FE	X	X	X	X								
Destination sub-District FE	X	X	X	X	X	X	X	X	X	X	X	X
Month FE	X	X	X	X	X	X	X	X	X	X	X	X
Residualized shocks		X		X		X		X		X		X
Controls × Month FE			X	X			X	X			X	X
# observations	3,937	3,937	3,937	3,937	506	506	506	506	780	780	780	780
# clusters (sub-Districts)	86	86	86	86	86	86	86	86	78	78	78	78

F.4 Price aggregation

In Section 3.3, we found that mask manufacturing exposure causes persistent decreases in mask prices after licensing. Because comparing prices across distinct products is challenging, we used as outcomes mean log buyer prices at the firm-product-by-month-by-destination (buyer) sub-District level, and included firm-product fixed effects in our analysis of impacts on prices. This construction of log buyer price is different from our other outcomes, for which we simply aggregated to the month-by-destination sub-District level. To test robustness of this construction, we instead leave the outcome log buyer price at the firm-product-by-receipt level (rather than aggregating), and present impacts on this outcome in Table A9. The patterns in the results we describe in Section 3.3 are robust to this alternative approach to constructing prices; although the magnitudes of impacts are often somewhat larger when we do not aggregate, they are also less precisely estimated.

Table A9: Mask manufacturing exposure decreases mask prices across multiple approaches to constructing prices

	Dependent variable:			
	log mask price		Avg log mask price	
	(1)	(2)	(3)	(4)
Mask manufacturing exposure$_d$ × Nov$_t$	0.444	0.272	0.445	0.263
	(1.059)	(0.898)	(0.680)	(0.666)
	[0.677]	[0.763]	[0.515]	[0.694]
Mask manufacturing exposure$_d$ × Dec$_t$	−0.245	0.181	0.173	0.199
	(0.940)	(0.940)	(0.732)	(0.717)
	[0.796]	[0.848]	[0.814]	[0.783]
Mask manufacturing exposure$_d$ × Jan$_t$	0.359	0.235	0.296	−0.101
	(0.520)	(0.416)	(0.383)	(0.343)
	[0.492]	[0.574]	[0.442]	[0.770]
Mask manufacturing exposure$_d$ × Feb$_t$	−0.545	−0.568	0.367	0.393
	(0.616)	(0.551)	(0.374)	(0.318)
	[0.380]	[0.306]	[0.329]	[0.220]
Mask manufacturing exposure$_d$ × Mar$_t$	-	-	-	-
Mask manufacturing exposure$_d$ × Apr$_t$	−0.475	−0.839	−0.521	−0.600
	(0.514)	(0.433)	(0.311)	(0.300)
	[0.358]	[0.056]	[0.098]	[0.049]
Mask manufacturing exposure$_d$ × May$_t$	−0.783	−1.149	−0.613	−0.673
	(0.788)	(0.644)	(0.327)	(0.290)
	[0.324]	[0.079]	[0.065]	[0.023]
Mask manufacturing exposure$_d$ × Jun$_t$	−1.069	−1.402	−0.671	−0.792
	(0.812)	(0.637)	(0.391)	(0.423)
	[0.192]	[0.031]	[0.090]	[0.065]
Mask manufacturing exposure$_d$ × Jul$_t$	−0.561	−1.003	−0.210	−0.316
	(0.798)	(0.647)	(0.309)	(0.314)
	[0.484]	[0.125]	[0.499]	[0.319]
Mask manufacturing exposure$_d$ × Aug$_t$	−0.890	−1.144	−0.460	−0.606
	(0.805)	(0.702)	(0.272)	(0.251)
	[0.273]	[0.107]	[0.096]	[0.018]
Destination sub-District FE	X	X	X	X
Aggregated log price			X	X
Firm-product FE	X	X	X	X
Controls × Month FE		X		X
Resizualized shocks		X		X
# clusters (sub-Districts)	86	86	86	86
# of observations	32,110	32,110	3,937	3,937

F.5 Measurement of mask purchases from manufacturers

In Appendix A.1.2 we describe the construction of effective mask quantities which account for heterogeneous quality (and other unobservable determinants of price) across masks. In Table A10, we demonstrate that when constructing the outcome share of masks purchased from manufacturers, using either effective quantities or expenditures delivers statistically indistinguishable results. While expenditures are simpler to construct, estimated impacts on share of effective mask quantities purchased from manufacturers can be used to more directly recover price elasticities of demand, and as a result our preferred estimates use effective mask quantities.

Table A10: Mask manufacturing exposure decreases mask prices across multiple approaches to constructing prices

	Dependent variable:			
	Eff mask Q from manufacturers / Real mask Q		Mask purchases from manufacturers / Mask purchases	
	(1)	(2)	(3)	(4)
Mask manufacturing exposure$_d$ × Nov$_t$	-0.016 (0.123) [0.897]	0.155 (0.202) [0.444]	-0.030 (0.156) [0.850]	0.098 (0.249) [0.696]
Mask manufacturing exposure$_d$ × Dec$_t$	0.035 (0.114) [0.760]	0.135 (0.214) [0.531]	0.026 (0.148) [0.861]	0.116 (0.258) [0.653]
Mask manufacturing exposure$_d$ × Jan$_t$	0.057 (0.115) [0.620]	0.224 (0.211) [0.290]	0.053 (0.148) [0.723]	0.202 (0.251) [0.424]
Mask manufacturing exposure$_d$ × Feb$_t$	0.046 (0.089) [0.608]	0.130 (0.181) [0.475]	0.027 (0.114) [0.812]	0.115 (0.210) [0.584]
Mask manufacturing exposure$_d$ × Mar$_t$	-	-	-	-
Mask manufacturing exposure$_d$ × Apr$_t$	1.095 (0.252) [0.000]	0.973 (0.413) [0.021]	0.999 (0.281) [0.001]	0.896 (0.465) [0.057]
Mask manufacturing exposure$_d$ × May$_t$	0.533 (0.179) [0.004]	0.464 (0.303) [0.130]	0.504 (0.204) [0.015]	0.446 (0.324) [0.173]
Mask manufacturing exposure$_d$ × Jun$_t$	0.196 (0.194) [0.316]	0.513 (0.293) [0.083]	0.254 (0.223) [0.258]	0.518 (0.314) [0.103]
Mask manufacturing exposure$_d$ × Jul$_t$	0.438 (0.136) [0.002]	0.398 (0.262) [0.132]	0.514 (0.148) [0.001]	0.448 (0.275) [0.107]
Mask manufacturing exposure$_d$ × Aug$_t$	-0.138 (0.197) [0.485]	0.031 (0.311) [0.920]	-0.111 (0.221) [0.617]	-0.017 (0.341) [0.960]
Estimation method	OLS	OLS	OLS	OLS
Firm-by-product FE				
Destination sub-District FE	X	X	X	X
Month FE	X	X	X	X
Residualized shocks		X		X
Controls × Month FE		X		
# observations	506	506	506	506
# clusters (sub-Districts)	86	86	86	86

G Number of paracetamol receipts as a proxy for COVID-19 infections

In Section 3.1.2, we used number of purchases of paracetamol, a commonly recommended fever medicine, as a proxy for COVID-19 infections, absent data on sub-District COVID-

19 infections and given limited testing capacity during the early months of the COVID-19 pandemic. In this section, we leverage data on COVID-19 infections at the District level to test the validity of number of purchases of paracetamol as a proxy for COVID-19 infections. Specifically, we estimate the association between increases in number of purchases of paracetamol and COVID-19 infections, and test whether a 1% increase in number of purchases of paracetamol is associated with a 1% increase in COVID-19 infections. We fail to reject this null in our preferred specifications, although we have limited statistical power due to relatively limited testing during our study period.

Data We gather daily confirmed District-level COVID-19 case counts from the Rwanda Biomedical Centre's COVID-19 operations dashboard.[A14] We aggregate confirmed COVID-19 case counts to the District-by-month level. For comparison, we aggregate our sub-District-by-month data on number of purchases of paracetamol (used as an outcome in Table 2) to the District-by-month level.

Empirical strategy To estimate the association between number of purchases of paracetamol and COVID-19 infections, we estimate by Poisson pseudo maximum likelihood

$$\log \mathbf{E}[\text{COVID-19 cases}_{dt} | \log \# \text{ paracetamol receipts}_{dt}, X_{dt}] = \beta \log \# \text{ paracetamol receipts}_{dt} + X_{dt}'\delta \quad \text{(A6)}$$

where X_{dt} is a vector of control variables; we include either a constant, destination District fixed effects, month fixed effects, or destination District fixed effects, month fixed effects, and controls interacted with month fixed effects. As in Table 2, the controls interacted with month fixed effects are Province fixed effects, log population density, and log number of textile manufacturers. Our preferred specification includes both sets of fixed effects along with controls interacted with month fixed effects, as this mirrors our specification with full

[A14] https://gis.rbc.gov.rw/portal/apps/opsdashboard/index.html#/59872985985446bbaf8c394ad857c5cd controls in Table 2.

Table A11: Increases in number of paracetamol receipts are associated with increased COVID-19 cases

	Dependent variable:			
	Confirmed COVID-19 cases			
	(1)	(2)	(3)	(4)
log # paracetamol receipts	0.496 (0.175) [0.005]	1.397 (0.792) [0.078]	0.482 (0.169) [0.004]	0.552 (0.738) [0.455]
Estimation method	Poisson	Poisson	Poisson	Poisson
Controls × Month FE				X
Month FE			X	X
Destination District FE		X		X
# observations	158	140	158	124
# clusters (Districts)	29	24	29	24

Results We present estimates of Equation A6 in Table A11. Across specifications, we find a 1% increase in the number of paracetamol receipts is associated with between a 0.5% and 1.4% increase in COVID-19 cases; this is relative to the 1% increase we would expect if number of paracetamol receipts were exactly proportional to COVID-19 infections. All of our point estimates are positive and of expected magnitude; however, many of these estimates are noisy, and in our preferred specification we can neither reject that a 1% increase in the number of paracetamol receipts is associated with a 0% nor a 1% increase in the number of COVID-19 cases, due to relatively limited testing during our study period that motivates our use of a proxy.

H Trade cost estimation

Empirical strategy As in Section 3.1.1, we aggregate non-mask textile trade values to total bilateral flows T_{od} from origin sub-District o to destination sub-District d. We then measure distances between sub-Districts using sub-District centroids, and remove intra-sub-District trade from our data. We follow Silva & Tenreyro (2006) and estimate a gravity equation by Poisson pseudo maximum likelihood.

$$\log \mathbf{E}[T_{od} | \log \text{distance}_{od}, \theta_o, \gamma_d, X_{od}] = \beta \log \text{distance}_{od} + X'_{od}\delta + \theta_o + \gamma_d \qquad \text{(A7)}$$

We estimate specifications with no controls, and controlling for an indicator that origin sub-District o and destination sub-District d are located in the same Province.

Results We present estimates of Equation A7 in Table A12. We find a 10% increase in distance between sub-Districts decreases non-mask textile trade by 6.3%-6.8%, with 6.8% our preferred point estimate from a specification without controls. We use this to interpret a 10% increase in distance to mask manufacturing as generating a 6.8% decrease in mask manufacturing exposure. Multiplying this by the average mask manufacturing exposure in our primary analysis sample (0.188), a 10% increase in distance to mask manufacturing generates a 0.013 decrease in mask manufacturing exposure. We use this value to interpret our estimates of impacts of mask manufacturing exposure on prices in Section 3.3.1.

Table A12: Non-mask textile trade between sub-Districts is decreasing in distance

	Dependent variable:	
	$\frac{\text{Non-mask textile purchases}_{od}}{\sum_o \text{Non-mask textile purchases}_{od}}$	
	(1)	(2)
$\log \text{distance}_{od}$	-0.682 (0.095) [0.000]	-0.632 (0.183) [0.001]
Estimation method	Poisson	Poisson
Intra-provincial trade dummy		X
Origin (sub-District) FE	X	X
Destination (sub-District) FE	X	X
# observations	27,881	27,881
# clusters (sub-Districts)	329	329

I Mask manufacturing exposure and purchases of masks from manufacturers

Interpretation of mask manufacturing exposure In Section 3.1.1, we noted that mask manufacturing exposure can be interpreted as predicted purchases of domestically manufactured masks as a share of total purchases of masks.

$$
\begin{aligned}
\text{Mask manufacturing exposure}_d \equiv \sum_o & \underbrace{\frac{T_{od}}{\sum_{o'} T_{o'd}}}_{\text{non-mask textile purchase share}} \times \underbrace{\frac{\sum_{d' \neq d} M^{\text{mnf}}_{od'}}{\sum_{d' \neq d} M^{\text{mnf}}_{od'} + M^{\text{oth}}_{od'}}}_{\text{jackknife mask manufacturing intensity}} \\
\approx \sum_o & \underbrace{\frac{M^{\text{mnf}}_{od} + M^{\text{oth}}_{od}}{\sum_{o'} M^{\text{mnf}}_{o'd} + M^{\text{oth}}_{o'd}}}_{\text{mask purchase share}} \times \underbrace{\frac{M^{\text{mnf}}_{od}}{M^{\text{mnf}}_{od} + M^{\text{oth}}_{od}}}_{\text{own mask manufacturing intensity}} \\
= & \frac{\sum_o M^{\text{mnf}}_{od}}{\sum_o M^{\text{mnf}}_{od} + M^{\text{oth}}_{od}}
\end{aligned}
$$

Mask manufacturing exposure does not equal domestically manufactured masks as a share of total purchases of masks for two reasons. First, the share of destination sub-District d's non-mask textiles purchased from origin sub-District o does not equal the share of destination sub-District d's masks purchased from origin sub-District o. Second, origin sub-District o's mask manufacturing intensity leaving out destination sub-District d does not equal its destination sub-District d-specific mask manufacturing intensity. In Section 3.1.1, we argued that mask manufacturing exposure is exogenous. However, for mask manufacturing exposure to impact purchases of masks, it should be the case that non-mask textiles purchases from origin sub-District o predict masks purchased from origin sub-District o and origin sub-District o's mask manufacturing intensity leaving out destination sub-District d predicts destination sub-District d-specific mask manufacturing intensity.

Empirical strategy We test whether non-mask textiles purchased from origin sub-District o predict masks purchased from origin sub-District o, and whether origin sub-District o's mask manufacturing intensity leaving out destination sub-District d predicts destination sub-District d-specific mask manufacturing intensity. We estimate

$$\frac{M_{od}^{\text{mnf}} + M_{od}^{\text{oth}}}{\sum_{o'} M_{o'd}^{\text{mnf}} + M_{o'd}^{\text{oth}}} = \beta \frac{T_{od}}{\sum_{o'} T_{o'd}} + X_{od}'\delta + \theta_o + \gamma_d + \epsilon_{od} \tag{A8}$$

$$\frac{M_{od}^{\text{mnf}}}{M_{od}^{\text{mnf}} + M_{od}^{\text{oth}}} = \beta \frac{\sum_{d' \neq d} M_{od'}^{\text{mnf}}}{\sum_{d' \neq d} M_{od'}^{\text{mnf}} + M_{od'}^{\text{oth}}} + X_o'\delta + \gamma_d + \epsilon_{od} \tag{A9}$$

Equation A8 tests whether the share of destination sub-District d's non-mask textiles purchased from origin sub-District o predicts the share of destination sub-District d's masks purchased from origin sub-District o. We include both origin sub-District and destination sub-District fixed effects, analogous to our gravity specification in H. In robustness specifications, we include as controls either log bilateral distance (described in H) and a bilateral same-Province indicator, origin Province-by-destination Province fixed effects, or both. Equation A9 tests whether origin sub-District o's mask manufacturing intensity leaving out destination sub-District d predicts its destination sub-District d-specific mask manufacturing intensity. We include destination sub-District fixed effects. In a robustness specification, we include origin Province fixed effects.

We note that specifications of Equation A8 and A9 that include origin Province-by-destination Province and origin Province fixed effects respectively, can be interpreted as testing whether predictive power remains after residualizing regressors on Province fixed effects. In Section 3.3.1, we showed our results are robust to residualizing jackknife mask manufacturing intensity on Province fixed effects, suggesting that predictive power remains.

Results First, we present estimates of Equation A8 in Table A13. We find that higher fractions of non-mask textiles in sub-District d sourced from origin sub-District o are associated with higher fractions of masks in sub-District d sourced from origin sub-District o. This finding is robust to the inclusion of additional controls, including origin Province-by-destination Province fixed effects.

Table A13: Non-mask textile trade predicts mask trade

	Dependent variable:			
	$\frac{\text{Mask purchases}_{od}}{\sum_o \text{Mask purchases}_{od}}$			
	(1)	(2)	(3)	(4)
$\frac{\text{Non-mask textile purchases}_{od}}{\sum_o \text{Non-mask textile purchases}_{od}}$	0.107**	0.099**	0.103**	0.100**
	(0.046)	(0.046)	(0.046)	(0.046)
log distance$_{od}$		−0.010**		−0.012***
		(0.004)		(0.004)
Intra-provincial trade$_{od}$		0.002		
		(0.005)		
Destination Sector FE	X	X	X	X
Origin Sector FE	X	X	X	X
Origin province × Destination province			X	X
# clusters (sub-Districts)	128	128	128	128
# of observations	5,588	5,588	5,588	5,588

Second, in Table A14 we present estimates of Equation A9. We find that higher origin sub-District o mask manufacturing intensity leaving out destination sub-District d predicts origin sub-District o destination sub-District d-specific mask manufacturing intensity. This finding is robust to the inclusion of origin Province fixed effects.

Table A14: leave-out mask manufacturing intensity predicts destination sub-District-specific mask manufacturing intensity

	Dependent variable:	
	$\frac{M_{od}^{\text{mnf}}}{M_{od}^{\text{mnf}}+M_{od}^{\text{oth}}}$	
	(1)	(2)
$\frac{\sum_{d' \neq d} M_{od'}^{\text{mnf}}}{\sum_{d' \neq d} M_{od'}^{\text{mnf}}+M_{od'}^{\text{oth}}}$	0.857***	0.854***
	(0.049)	(0.050)
Destination Sector FE	X	X
Origin Province FE		X
# clusters (sub-Districts)	181	181
# of observations	677	677

J Cost-effectiveness

As described in Section 3.3, we estimate large decreases in COVID-19 infections caused by domestic mask manufacturing in Rwanda. In this section, we compare these impacts to available estimates of the fiscal costs of promoting domestic mask manufacturing in order; this provides the cost-effectiveness of promotion of domestic mask manufacturing as a policy to reduce COVID-19 infections.

First, to calculate the decrease in COVID-19 infections caused by domestic mask manufacturing in Rwanda, we multiply, in May through August, monthly national new COVID-19 infections (from the Rwanda Biomedical Center) by our estimates of the monthly impacts of domestic mask manufacturing on COVID-19 infections from Section 3.3. As noted in Section 3.3.1, these estimates provide a lower bound on the impacts of domestic mask manufacturing on COVID-19 infections. Using this approach, we calculate domestic mask manufacturing averted 4,300 COVID-19 infections from May through August.

Second, the fiscal costs of promoting domestic mask manufacturing are the sum of the costs of two policies described in Section 2.2: the VAT exemption for domestically manufactured masks, and administrative licensing costs. We calculate an upper bound on a fiscal costs of the VAT exemption as the value-added tax rate (18%) times the total turnover of exempted masks to final consumers from May through August. While we do not have data on licensing costs, we note that the small number of mask manufacturers suggests that the associated licensing and audit costs are likely to be small relative to the costs of the VAT exemption. Using this approach, we calculate a total cost of RwF 41 million (approximately $40,000) of the promotion of domestic mask manufacturing.

Third, we take the ratio of these two estimates, and find the cost of averting a COVID-19 infection through promotion of domestic mask manufacturing in Rwanda was 9,500 RwF/infection (approximately $9.4/infection). This is an order of magnitude smaller than estimates from Kenya of treating a COVID-19 infection, which range from $278 to $5,879 (Barasa & Kairu, 2020).

Appendix references

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