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Assistant Professor of Economics

15 September 2026

Emily Breza, Arun Chandrasekhar and Davide Viviano
Organizers, NBER Workshop on Development Economics and Networks for Early-Career Researchers

Application to present at the workshop in Cambridge on 5 December 2026

Dear Professors Breza, Chandrasekhar and Viviano,

I would like to present "Firm Sourcing After a Border Closure: Evidence from Rwanda-Uganda", joint work with Andrew B. Bernard and John Spray.

I have just completed my first year as an Assistant Professor of Economics at HEC Paris. I joined HEC after a postdoctoral year at MIT. I completed my PhD at the London School of Economics in summer 2024.

The paper uses linked customs and VAT records to study how Rwandan firms responded when the Rwanda–Uganda border closed in 2019. Exposed firms switched quickly to new source countries. For a firm that had sourced all its 2013–15 imports from Uganda, the probability of importing from Tanzania rose by 8.2 percentage points in 2019, and almost half of the exposed firms importing from Tanzania in 2019–20 had no recorded Tanzanian imports in 2016–18. The adjustment was costly: the exit hazard of a fully exposed firm rose by 7.3 percentage points.

I enclose my CV, a one-page statement of motivation and research interests, and the paper.

Recommendation

Andrew B. Bernard, my co-author on this paper, could provide a recommendation if I am shortlisted.

Andrew B. Bernard
Kadas T'90 Distinguished Professor and Area Chair, Economics
Tuck School of Business at Dartmouth
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Sincerely,

Arnaud Dyèvre

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Fields

Macroeconomics (growth, innovation & firm heterogeneity)
Public Economics

Academic appointments

HEC, Paris — 2025 –
Assistant Professor of Economics

Massachusetts Institute of Technology, Cambridge — 2024 – 2025
Post-Doctoral Associate, Science for Progress Initiative (J-PAL)

Affiliations

CenTax (LSE-Warwick), **STICERD** (LSE), **CFM** (LSE), **Innovation Lab** (Collège de France)
Visiting Scholar

Education

London School of Economics, London
Ph.D. in Economics — 2020 – 2024
Committee: Xavier Jaravel, Benjamin Moll, Arun Advani, John Van Reenen

M.Res. in Economics (*Distinction*) — 2018 – 2020

Sorbonne Université, Paris
B.Sc. in Mathematics — 2014 – 2018

Publications

Innovation Catalysts: How Multinationals Reshape the Global Geography of Innovation — 2022
with Riccardo Crescenzi & Frank Neffke
Economic Geography

Graduating from Food Insecurity: Evidence from Graduation Projects in Burundi and Rwanda — 2019
with Stephen Devereux, Keetie Roelen, Ricardo Sabates, Rachel Sabates-Wheeler & Dimitri Stoelinga
Food Security

Working papers

The CenTax Business-to-Worker Register: Technical Note — 2026
with Andy Summers, Arun Advani, Helen Hughson, Sebastian Gazmuri-Barker & Sanaya Mahajan

Public R&D Spillovers and Productivity Growth — 2024
EEA & UniCredit Foundation Best Job Market Paper Award 2023
Finalist - ECB Young Economist Prize

Work in progress

The Rwanda-Uganda border closure: Impacts and Adaptation through Production Networks
with Andrew Bernard & John Spray

Fragmentation, Employment and Compensation in Production Networks
with Jan Bakker, Kalina Manova & Andreas Moxnes

70 Years of Patents Matched to Firms: Methodology and Insights About Firm Heterogeneity
with Oliver Seager

The Impacts of Migrants on Firm Performance
with Arun Advani and Andy Summers

Awards, grants & fellowships

Awards

Excellence in Refereeing Award, *AEJ: Applied Economics*	2026
European Central Bank Young Economist Prize – Finalist	2024
EEA & UniCredit Foundation Econ Job Market Best Paper Award	2023
LSE Class Teacher Award, Department of Economics	2021
LSE Excellence in Education Award, Department of Economics	2021
Distinction, M.Res. in Economics, LSE	2020
LSE Student Union Teaching Award, nominated by students	2020

Grants

ANR JCJC (€281,507, PI)	2026
OpenAI Research grant ($115,000, co-PI)	2026
HEC Foundation grant (€25,000, PI)	2026
IGC Small Research Grant (£18,000, co-PI)	2022
STICERD Ph.D. research grant (£5,000, PI)	2022
LSE Race Equity Innovation Fund (£2,000, co-applicant)	2022
CEPR-STEG Ph.D. Research Grant (£15,000, PI)	2021
STICERD Ph.D. research grant (£5,000, PI)	2021

Fellowships

National Bank of Belgium visiting fellowship	2022
Alan Turing Institute Enrichment Student fellowship	2020 – 2022
ESRC Doctoral fellowship	2018 – 2023

Research experience

National Bank of Belgium, Brussels
Visiting Ph.D. Researcher — 2022

Alan Turing Institute, London
Visiting Ph.D. Student ('Engage@Turing' and 'Enrichment') — 2020 – 2022

London School of Economics, London
RA to Profs. Arun Advani, Felix König & Andy Summers — 2019 – 2021
RA to Profs. Camille Landais & Johannes Spinnewijn, LSE — 2017 – 2018

Teaching experience

HEC Paris, Jouy-en-Josas
Introductory Macroeconomics (L3) – co-taught with T. Michalski & A. Bergeaud — 2025 – 26
Pre-sessional course on Macroeconomics (M1) — 2025
AI Agents for Academic Research – HEC PhD elective (two 3-hour sessions) — 2026 – 27

NBER Summer Institute, Cambridge, MA
Innovation Research Boot Camp 2025 – TA & social chair — 2025

London School of Economics, London
M.Sc. Macroeconomics – TA to Matthias Doepke, Wouter den Haan & Alwyn Young — 2021 - 2023
M.Sc. & Ph.D. Statistics and Probabilities for Economics – TA to Marcia Schafgans — 2020 - 2023
B.Sc. Econometrics – TA to Taisuke Otsu, Steve Pischke & Canh Thien Dang — 2019 - 2020
Python for Social Scientists – weeklong Course for Ph.D. students — 2019

LSE, Widening Participation school, online during Covid
Economics teacher for students from London state schools (outreach program) — 2020

Skills

IT Coding agents (Claude Code, Codex and Gemini Code Assist), Stata, Matlab, Python, ArcGis, Gephi, Git & GitHub, HTML & Markdown

Languages **French** (native), **English** (fluent), **German** (fading)

Conferences and seminars

2026: Lucca Macro Workshop, ESCoE, HMRC, EIEF (forthcoming)
2025: MIT Sloan, Hitotsubashi University, EAYE (London), Society of Economic Dynamics (Copenhagen), Darlington Economic Campus, Ellison Institute of Technology (Oxford), ADR UK (Cardiff), Collège de France/INSEAD, London Business School, Queen Mary University, Tillväxtanalys (online), Bank of England
2024: PhD-EVS, Stockholm, HEC Paris, Tübingen, OECD, RES annual conference, Society of Economic Dynamics (Barcelona), ECB forum on central banking, US National Academies, NBER Summer Institute, NASA, Northwestern Kellogg
2023: Alan Turing Economics Data Science workshop, Warwick PhD conference, Concordi conference, WEFI, CEPR Paris Symposium, EWMES, MINECOFIN (Rwanda)
2022: STEG PhD conference, Bank of Uganda

Outreach and Service

Co-founder and coordinator of the Ph.D. Applicant Mentoring Programme 2020 - 2023
Mentoring scheme for PhD applicants from underrepresented backgrounds.
Jointly led by Ph.D. students at Cambridge, LSE, Oxford, UCL and Warwick

Referee for
Journals: AEJ: Applied Economics, AEJ: Economic Policy, American Economic Review, Applied Geography, Economica, Economic Journal, Organization Science, Quarterly Journal of Economics, Review of Economics & Statistics

Grants: Sloan Foundation

Misc

Citizenships: France and United Kingdom

Updated: August 2026

Statement of Motivation and Research Interests

Arnaud Dyèvre · Assistant Professor of Economics, HEC Paris · dyevre@hec.fr

Research interests

I am coming to the field of production networks from the perspective of economic growth. I believe the dynamic evolution of production networks that accompany the growth process has been understudied: how links form, break, re-form, and how quickly so, over the course of several years or even decades. This is partly due to data constraints (to my knowledge, only the Belgian production network data has a multi-decade coverage), but the increasing availability of production network datasets in devlopping, middle-income and high-income countries, covering ever more years, makes this growth and production network agenda very exciting.

The paper I would like to present at the workshop fits partly in this agenda: it studies how the Rwandan domestic production network rewired after the 2019–2022 closure of the Rwanda–Uganda border. The closure was a shock to the Rwandan economy, and it was also a shock to the Rwandan production network. I study how firms adapted to this shock by changing their sourcing patterns, and I find that they did so very quickly. Some firms did not manage to adapt however, and had to exit. Overall, the story told by the data is one of *network resilience* more than *network fragility*. Most work on production networks takes the network as given and studies how shocks travel through it. As a result, production networks are seen as powerful vehicles to *transmit* shocks, but we know little about how they *absorb* them when firms endogenously source new suppliers and find new customers.

I started the project during my PhD at LSE, and my co-authors and I are close to releasing it as a working paper. This would be the first time this paper is presented to an academic audience (I have presented our findings to policymakers at the Bank of Uganda, the Ministry of the Economy and Finances of Rwanda, the OECD, and the IGC)

The empirical literature on endogenous production networks is small but expanding, and I hope this project can contribute to it. The shock is sharply dated and politically driven, which gives clean identification. We also observe two national production networks, through VAT and customs records from both Rwanda and Uganda.

In other work, I am interested in the impact of public R&D on long-term productivity growth, distributional growth and AI.

Why this workshop

The workshop would be a unique opportunity for me to get feedback on my project from young scholars doing research at the frontier of development and networks. I am also keen to find collaborators that would be interested to work with me on the Rwandan, Ugandan and Belgian production network datasets that I have access to. I think the growth and development aspects of production networks are underexplored, and I would like to discuss with other participants how we can advance this agenda, perhaps together.

Firm Sourcing After a Border Closure:

Evidence from Rwanda-Uganda*

Andrew B. Bernard
Tuck School of Business at Dartmouth
CEPR, CESifo & NBER

Arnaud Dyèvre
HEC Paris

John Spray
International Monetary Fund

September 2026

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Abstract

The 2019–2022 Rwanda–Uganda border closure cut off a major sourcing origin for Rwandan importers. Using linked customs and VAT records, we find surviving exposed Rwandan firms show fast substitution toward Tanzania. For a fully exposed firm, one that sourced all its 2013–15 imports from Uganda, the probability of importing from Tanzania rises 8.2 percentage points in 2019, comparable in magnitude to the 2018 participation rate of 8.3% among all positively exposed firms. Almost half of exposed firms importing from Tanzania in 2019–20 had no recorded Tanzanian imports in 2016–18. Product-level records show the limits of this adjustment. Only one disrupted firm–product pair in ten is re-sourced within a year, so import baskets are reallocated rather than restored. There is a large increase in firm exit following the closure: the exit hazard for a fully exposed firm rises 7.3 percentage points in 2019, against an overall 2018 exit hazard of 13.5%.

Keywords: border closure, origin substitution, supplier search, firm-to-firm trade, production networks, Rwanda
JEL codes: F13, F14, F15, D85, L14, O55

*Bernard: andrew.b.bernard@tuck.dartmouth.edu; Dyèvre: dyevre@hec.fr; Spray: jspray@imf.org. We are grateful to the Rwanda Revenue Authority and the Uganda Revenue Authority for data access. The IGC Rwanda and Uganda teams, especially Harriet Conron, Richard Newfarmer and Christian Lippitsch, provided essential information and assistance on the ground. For their thoughtful comments we thank seminar participants at the OECD, the Bank of Uganda, the National Bank of Rwanda, and STEG. We are grateful to STEG (grant PHD575) and the IGC (grant RWA-22061) for their financial support. Amartya Rajamalla and Oliver Seager provided excellent research assistance. All errors are our own.

1 Introduction

Border closures and related disruptions to cross-border mobility can sever trade links abruptly and propagate through production networks. These closures, whether arising from political tensions, security incidents, epidemics, or infrastructure bottlenecks, are particularly problematic for firms in landlocked economies that typically reach world markets through a small number of corridors. When a corridor closes, the affected firms either locate a new foreign supplier, use a domestic one, or stop trading. This paper studies the responses of importing firms along these margins during the sharp 2019–2022 closure of the Rwanda–Uganda border.

We study that shock using linked administrative microdata. On the Rwandan side, we combine VAT invoice schedules, customs declarations, and tax records to build a firm-year panel spanning 2008–2021. The resulting data allow us to characterize pre-closure exposure and sourcing redundancy, follow reallocation across foreign partners, and relate those adjustments to survival, domestic-network outcomes, and firm performance.

Our analysis relies on a difference-in-differences specification comparing Rwandan firms that imported from Uganda in 2013–15 with other established importers that recorded positive imports from any origin but none from Uganda in that window. Because firms in the treated group are typically larger than in the control one, we use entropy balancing (Hainmueller, 2012) to reweight control firms to match exposed firms on 2018 total sales, network sales, and network purchases.

We measure exposure continuously, as the share of a firm's imports sourced from Uganda three years before the closure. Exposure is strongly bimodal, so a binary treated/control indicator averages firms for which the closure is nearly irrelevant with firms for which it affects almost the entire import basket. Measuring exposure using imports from 2013–15 and examining firm outcomes over 2016–2020 separates the exposure and outcome windows, while allowing us to compare firms with different levels of exposure before and after the closure. Because firms could change suppliers after 2015 and stop importing from Uganda before the closure, we interpret the coefficient as an intention-to-treat-style effect of *historical* Uganda dependence, rather than direct exposure at closure. It also avoids making the exposed and unexposed groups differ most in trade outcomes precisely in the omitted year. The source switching results are robust when we measure exposure over 2017–18 on the wider sample, which includes importers entering after 2015.

The central result is fast substitution toward other countries in the region (Tanzania, the DRC and Burundi). For a firm moving from no Uganda exposure to full exposure, the probability of importing from Tanzania rises 8.2 percentage points in 2019 and 9.0 in 2020, against a 2018 rate among all positively exposed firms of 8.3%. Conditional on importing, imports from the region are estimated to rise by 90.9% in 2019 and 67.7% in 2020. Rwandan firms do not appear to rely on the DRC and Burundi for sourcing imports post closure, so the regional result is largely a Tanzanian one.

Firms affected by the closure choose to redirect their imports from origins they had not used before. Before the closure only 15.5% of Uganda importers also sourced from Tanzania, 6.1% from

the DRC and 2.8% from Burundi. 46.3% of the exposed firms that import from Tanzania in 2019–20 had no recorded Tanzanian imports in 2016–18, rising to 66.2% among Tanzania importers in 2019–20 that had sourced at least half their imports from Uganda. The origins that buffer the shock are new for many exposed firms. These substitution patterns are signs of adoption of new foreign origins under duress rather than the activation of pre-existing redundancy. Search-based models of firm-to-firm trade emphasize that margin (Spray, 2021), but it is rarely observed directly.

While some Rwandan firms are able to find new international suppliers, product-level records show that their substitution of international imports is imperfect. Only 28.8% of Uganda-sourced firm–product pairs have any alternative source before the closure, and almost none of it regional. Among firm–HS6 product pairs disrupted in 2019 (meaning the firm recorded no Uganda imports of that product that year), just 10.3% are sourced from anywhere else within a year.[1] Set against the firm-level regional response, the implication is that firms reallocate their import basket rather than replacing specific products. Agricultural inputs, the core of the Uganda basket, are both the least hedged and the least replaceable.

This imperfect adjustment is costly, on two margins. For a fully exposed surviving firm, total imports fall 1.07 inverse hyperbolic sine (asinh) points in 2019 and 3.09 in 2020 (baseline: 14.03), and the probability of importing at all falls 6.2 and 19.2 percentage points (baseline: 82.6%). Significantly, the firm exit hazard rises 7.3 percentage points in 2019 and 3.7 in 2020 (baseline: 13.5%).

If domestic and foreign sourcing were substitutes at a one-to-two-year horizon, exposed firms would lean on Rwandan suppliers but they do not. We use the richness of the Rwandan VAT data to build the domestic production network, and assess if affected firms rely on local suppliers when they cannot import from Uganda. Domestic purchases, the number of domestic suppliers and the probability of having a domestic supplier exhibit no differential change among treated firms, post closure. We are thus not detecting signs of domestic network rewiring as a consequence of the international trade shock.

The paper separates three margins of firm adjustment to a corridor shock, namely (1) new foreign sourcing, (2) domestic sourcing, and (3) exit, and shows that the observed adjustment runs through the first and third. Two features of the data make this possible. On the foreign side, customs declarations record the origin country of every shipment, so we observe which *origins* a firm uses and when it starts using a new one. We do not observe the identity of the foreign seller, and we make no claim about individual cross-border partnerships. On the domestic side the VAT annexes record transactions between VAT-registered firms, so domestic links *are* observed individually, and there is no domestic rewiring.

Related literature. A large literature shows that shocks transmit along buyer–supplier chains and that concentrated sourcing can magnify exposure to localized disturbances (Acemoglu *et al.*,

[1]This also addresses the concern that the observed substitution reflects rerouting of Ugandan goods through Tanzania rather than genuine origin switching: rerouting at scale would require fraudulent origin certification across hundreds of firms.

2012; Carvalho, 2014; Barrot and Sauvagnat, 2016; Bonadio *et al.*, 2021). In low- and middle-income settings, where a handful of transport corridors connect landlocked economies to seaports, sudden frictions at a single crossing can be especially consequential (Baldwin and Freeman, 2022). The 2019–2022 closure of the Rwanda–Uganda border provides a rare opportunity to study such a shock in a setting where administrative records capture both domestic business-to-business (B2B) transactions and international shipments.

This paper is related to three strands of existing research. The first links production networks and the propagation of shocks. Acemoglu *et al.* (2012) and Carvalho (2014) examine how network structure affects the aggregation of micro shocks while Barrot and Sauvagnat (2016) show how concentrated sourcing magnifies exposure. Boehm *et al.* (2019); Carvalho *et al.* (2021); Bonadio *et al.* (2021) consider natural experiments where shocks propagate along supply chains. Firm-to-firm evidence emphasizes that resilience depends on whether disrupted links can be replaced (Khanna *et al.*, 2022; Huneeus, 2018; Bernard *et al.*, 2019). The contribution here is to observe the replacement margin directly for foreign origins and domestic links, for a disruption that is sharply dated and politically rather than economically driven. Methodologically the paper joins work using VAT paper trails to reconstruct domestic production networks (Pomeranz, 2015; Carrillo *et al.*, 2017; Spray and Wolf, 2018).

In work on search and the formation of trading relationships, buyers do not costlessly locate sellers, and the frictions involved shape both the extensive margin of trade and its response to shocks. Search externalities in firm-to-firm trade have been documented directly in this region (Spray, 2021). Face-to-face contact and the associated search and contracting problems can dominate the cost of forming a new trading relationship (Startz, 2024). The geography of network formation, in which the cost of matching rises with distance and thin markets, has been formalized and estimated in spatial production-network models (Arkolakis *et al.*, 2023). Related work characterizes the two-sided heterogeneity of buyer–seller matching and the churn of trading relationships (Bernard *et al.*, 2018, 2022; Eaton *et al.*, 2011). This paper contributes an unusually clean instance of the object those literatures model: a large group of firms forced to find replacement origins at once, with the timing fixed by policy rather than chosen.

This paper also contributes to the literature on trade costs, corridors, and sourcing. Time and uncertainty in transit act as sizeable ad valorem trade costs (Hummels and Schaur, 2013), administrative delay depresses volumes (Djankov *et al.*, 2010), and corridor access has first-order real effects (Donaldson, 2018; Donaldson and Hornbeck, 2016; Baldwin and Freeman, 2022). On the firm side, input specificity, contracting frictions, and the margins of sourcing shape the scope for substitution (Antràs and Chor, 2013; Antràs and Helpman, 2004; Antras *et al.*, 2017; Grossman *et al.*, 2023). The setting studied here is a policy-driven closure of one corridor in a developing economy that has a second available, allowing the separation of the corridor margin from the supplier margin.

Section 2 describes the closure. Section 3 summarizes the data and samples. Section 4 sets

Figure 1: Rwanda's main international border crossings



Notes: Gatuna/Katuna and Cyanika/Kyanika were closed from late February 2019; heavy cargo at Gatuna was initially directed to Kagitumba/Mirama Hills. The Central Corridor to Dar es Salaam runs through Rusumo, and the crossings to the DRC and Burundi remained open throughout.

out the empirical strategy. Section 5 presents results while Section 6 interprets them. Section 7 concludes.

2 Context

2.1 The 2019–2022 Rwanda–Uganda border closure

Rwanda and Uganda are members of the East African Community (EAC). Rwanda began applying the EAC Customs Union's instruments in July 2009, and the EAC Common Market Protocol, which provides for the progressive free movement of goods, persons, services and capital among member states, entered into force in July 2010. These arrangements seek to liberalize intra-regional trade and provide for the free movement of qualifying regional goods and persons.[2]

Figure 1 locates the affected crossings. The restrictions began on 27–28 February 2019. Rwandan authorities initially said that heavy trucks at Gatuna/Katuna were being diverted to Kagitumba/Mirama Hills for road works, but the restrictions quickly came to reflect a broader political

[2]East African Community, Customs Union overview; East African Community, Common Market overview.

and security dispute,[3] and culminated in a complete ban of Ugandan imports. It remained in place until 31 January 2022, when Rwanda reopened Gatuna while emphasizing that not all grievances had been resolved.[4] Our panel ends before the reopening could generate a usable post-period, so we study the closure and not the reopening. The shock was politically driven and unanticipated, which is what makes the sharp break in the customs data interesting for causal identification purposes.

Rwanda also has a second corridor: the Central Corridor through Tanzania to Dar es Salaam, alongside the Northern Corridor through Uganda and Kenya to Mombasa. The restrictions sharply curtailed Uganda-origin sourcing while leaving Tanzania available as an alternative origin and route,[5] a configuration in which corridor substitution rather than domestic substitution is the natural margin of adjustment.

Importantly, the shock was asymmetric. Rwanda barred Uganda-origin goods, while Uganda did not impose a corresponding prohibition on Rwandan goods. Rwandan firms were therefore still able to export to Uganda. Ugandan statistics continue to record imports from Rwanda after the closure, albeit at lower levels.[6] Rwanda was the more exposed economy, and it is the side on which our data support a firm-level design. Appendix B reports the more limited Ugandan evidence.

2.2 The closure in the trade data

Ugandan exports to Rwanda are much larger than the other way around. Before the closure, Uganda exported more than $200 million annually to Rwanda while Rwanda's exports to Uganda were an order of magnitude smaller.[7]

Figure 2 shows Rwanda's import origins in the last full pre-closure year and the breakdown of goods imported from Uganda, namely vegetable goods, metals, chemicals, mineral products and foodstuffs. Against this backdrop, the abruptness of the 2019 closure is apparent in the trade data. The number of unique Rwandan firms importing from Uganda drops sharply after March 2019 and remains near zero, while the series for other neighboring countries shows no comparable collapse. As Figure 3 shows, the sharp, near-complete fall in the number of firms recording Uganda-origin imports suggests that affected firms had little opportunity to adjust in advance, consistent with contemporary reporting. Online Appendix Figure OA.B.1 shows the same break in values, and Online Appendix Figure OA.B.2 compares Uganda-to-Rwanda flows as reported by Rwandan and by Ugandan customs. The two track each other closely around the closure, which makes it unlikely that the break is a reporting artifact.

Contemporary reporting documents informal smuggling across the land border after the closure, and Ugandan statistics continue to record small Rwanda-bound flows (Top Africa News, 2025).

[3] BBC News, 9 March 2019; Reuters, 5 March 2019, via Voice of America.

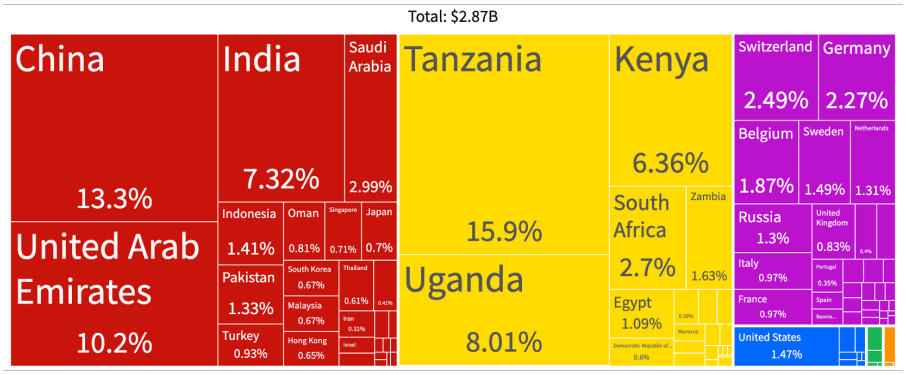
[4] Reuters, 31 January 2022.

[5] Northern Corridor Transit and Transport Co-ordination Authority; Central Corridor Transit Transport Facilitation Agency.

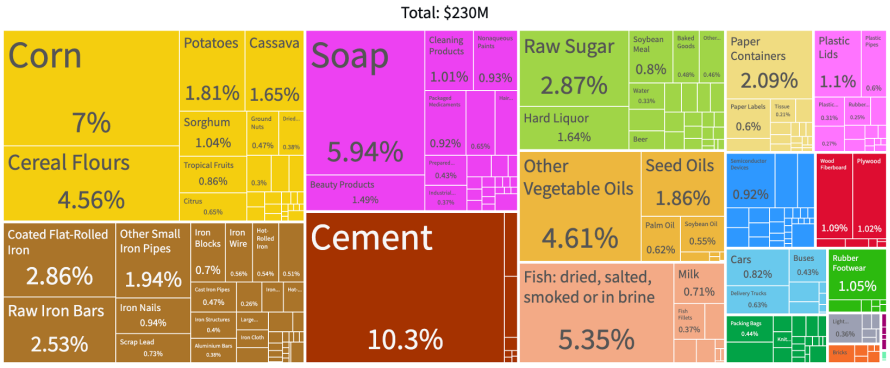
[6] Uganda Bureau of Statistics, *Statistical Abstract 2020*, Section 4.3.12.

[7] Reuters, 28 January 2022.

Figure 2: Rwandan imports in 2018



(a) Countries of origin of all Rwandan imports



(b) Goods imported from Uganda

Notes: Panel (b) uses HS4 categories, 2017 revision. Source: The Observatory of Economic Complexity.

Section 5.8 sets out what this implies for the estimates.

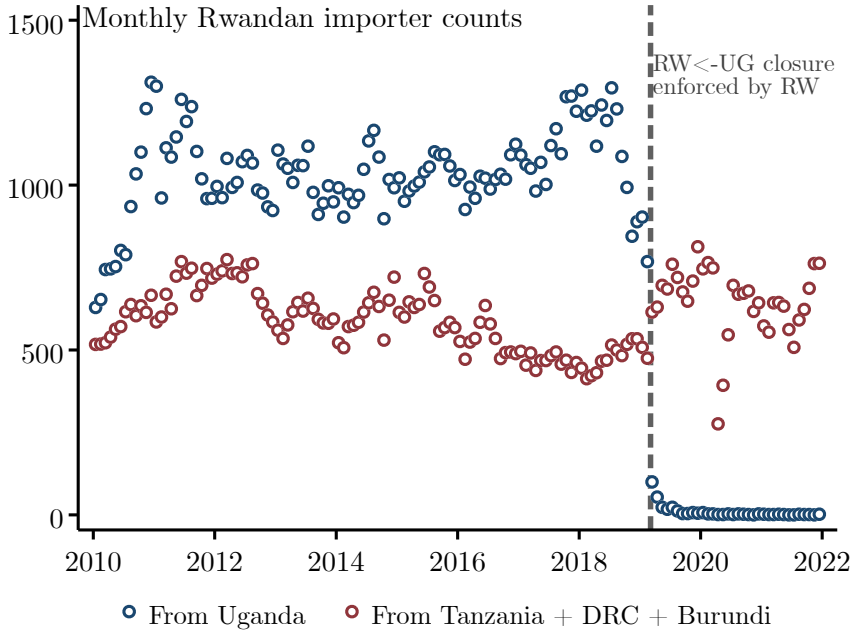
3 Data and estimation samples

All data are administrative records held by the Rwanda Revenue Authority, linked by a stable anonymized taxpayer identifier that is consistent across files and years. Table 1 lists them. All monetary values used below are deflated to 2010 Rwandan francs. In August 2010, one US dollar was about 590 Rwandan francs.[8] To limit the impact of extreme values on summary statistics and regression estimates, variables are winsorized at the 99.5th percentile.

Two features of our data matter for what follows. The invoice annexes make domestic buyer–seller links individually observable, so a null on domestic rewiring is a measured null rather than an inference from aggregates. Customs declarations, by contrast, record the origin *country* of a shipment but not the identity of the foreign seller, so on the international side we observe which

[8]The August 2010 monthly average was 588.97 RWF per US dollar. National Bank of Rwanda, monthly and annual exchange rates.

Figure 3: Monthly Rwandan importer counts, by country of origin



Notes: The figure reports the monthly number of Rwandan firms importing from Uganda and the sum of the corresponding origin-specific counts for Tanzania, the DRC and Burundi. A firm importing from more than one of the three regional origins in a month is counted once for each origin. The dashed line marks 6 March 2019. No origin–month observation is suppressed; a structurally absent origin–month is plotted as zero. These descriptive monthly customs aggregates are raw and are not subject to the firm–year winsorization rule used in the regressions. Source: authors' calculations from Rwandan customs records.

origins a firm uses and when it starts using a new one, and nothing about individual foreign partnerships.

Throughout, *the region* means Rwanda's three neighbors other than Uganda, that is Tanzania, the DRC and Burundi. *Extra-regional* means every other origin. Uganda is counted in neither.

3.1 Estimation samples

Exposure is measured well before the closure. A firm's exposure is the Uganda share of its imports over 2013–2015, and our estimation runs over 2016–2020 with 2018 omitted. The three-year gap between the two windows matters. If exposure is instead measured on the two years immediately before the closure, the exposed and unexposed groups differ most in trade outcomes precisely in the omitted year, and several outcomes then have pre-period coefficients as large as their post-period ones. Section 5.7 documents those differences.

We first retain firms that record nonzero total sales, domestic sales, or domestic purchases in every year covered by our estimation period. The estimation sample further restricts this group to the 4,267 firms with positive recorded imports in 2013–2015, of which 1,694 have strictly positive

Table 1: Administrative datasets

Source	Coverage	Unit	What it provides
Customs declarations (ASYCUDA)	2008–2021	Declaration × HS line	Import and export values (CIF and FOB), quantities, HS codes, origin, provenance and destination countries, border post, declaration regime. Used for trade outcomes, for the origin-specific participation indicators, and to construct exposure.
VAT returns (VAT)	2008–2021	Firm × month	Declared turnover and VAT liability. Primary sales measure.
VAT invoice annexes (VAT annexes)	2013–2021	Buyer × seller × month	Every invoice between VAT-registered firms, from both the sales and the purchase side. Used for all domestic-network outcomes: values, counts of customers and suppliers, participation, and the second-round exposure measure.
Income-tax filings (CIT and PIT)	2008–2021	Firm × year	Turnover, profits. Source of the balance-sheet sales measure used to define survival.
Payroll records (PAYE)	2008–2021	Employer × month	Headcount and remuneration, by permanent, casual and secondary status. Source of the permanent-employment and permanent-wage-bill outcomes used in the analysis.
Taxpayer register	2008–2021	Firm	Entity type, main activity, tax center. Used for sector classification and geography.

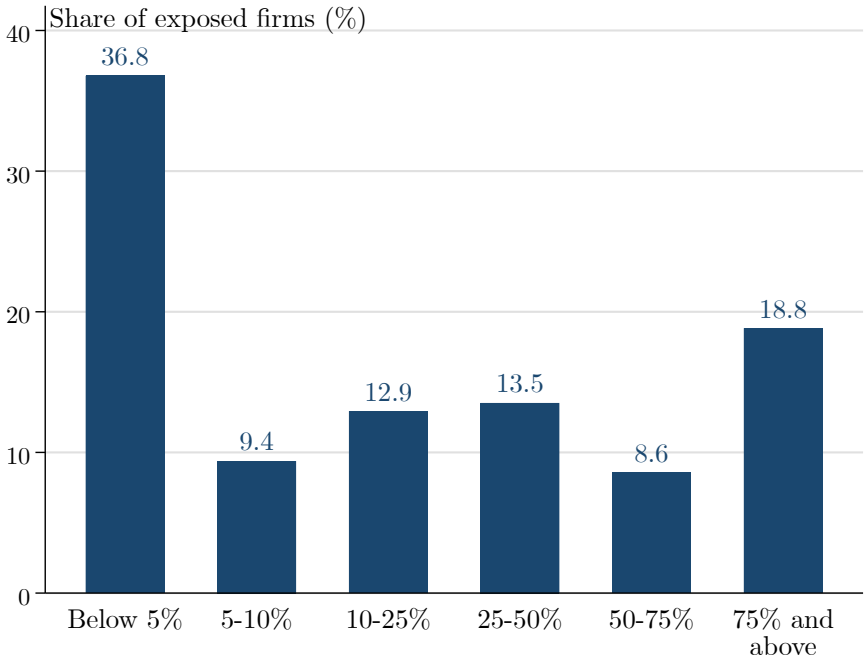
Notes: Monetary values are deflated to 2010 Rwandan francs. Except for Uganda import values, eligible continuous variables are capped above their calendar-year 99.5th percentile. Uganda import values, exposure, and Uganda-import participation remain uncapped. The invoice annexes begin in 2013. Domestic-network outcomes are not available for earlier years. Appendix A documents construction and the rules used to reconcile the sales and purchase annexes.

Uganda exposure and 2,573 have none. Every event study in Section 5 runs on this panel except the exit specifications, which need a wider sample and a different exposure vintage, both explained in Section 5.4.

Restricting to firms already importing in 2013–2015 selects established importers, which is the cost of the design. Section 5.7 therefore re-estimates everything on the wider 5,846-firm panel with exposure measured on 2017–18, which includes importers that entered later. The regional results are the same in both, so they are not an artifact of conditioning on long-running importers.

Two other samples appear. Exit is studied on a wider, unbalanced source panel of 31,091 firms observed 2010–2021, so that pre-closure exit variation exists. The product-level statistics in Section 6.3 use the raw customs universe of 24,435 importers, which is not restricted to firms appearing in the tax panel. We flag which sample is in use wherever it is not the estimation sample.

Figure 4: Uganda share of 2013–15 imports among exposed firms



Notes: Median 13.1%, mean 31.2%, aggregate share 12.9%. $N = 1,694$.

We refer to *exposed firms* as shorthand for the 1,694 with strictly positive exposure to Ugandan imports. Estimates are always the effect of moving exposure from 0 to 1. Figure 4 reports the distribution of exposure among exposed firms. The median is 13.1%, the mean 31.2%, and Uganda accounted for 12.9% of these firms' 2013–15 imports in aggregate. The distribution is bimodal, which motivates our use of a continuous exposure measure. Table 2 reports the 2018 characteristics of exposed and unexposed firms.

Uganda is a common trade partner for Rwandan firms who engage in international trade in the region: conditional on trading with the DRC, Tanzania or Burundi, firms are likely to also trade with Uganda. Among Rwandan firms importing from Tanzania in 2016–18, 58.8% also imported from Uganda. The figures are 56.1% for Burundi importers and 40.5% for DRC importers. Table 3 is a cross-section of the 4,267 estimation-sample firms. These overlaps show that Uganda was a common co-origin among Rwandan firms importing from elsewhere. The reverse is not as true; firms importing from Uganda are not very likely to also import from other countries in the region. This asymetry highlights how central Uganda is as a sourcing origin for Rwandan firms. Uganda lies on the Northern Corridor toward Mombasa; Dar es Salaam is reached through Tanzania on the Central Corridor. Any "never Uganda" restriction on a comparison group therefore selects on sourcing breadth.

Table 2: Descriptive statistics for the estimation sample, 2018

	Exposed (1,694)				Unexposed (2,573)			
	p25	Median	p75	Mean	p25	Median	p75	Mean
Panel A. Size and trade, millions of 2010 RWF								
Total sales	2.5	47.4	337.8	820.7	1.7	34.7	189.1	426.3
Total imports	0.4	7.3	71.8	269.4	0.0	2.6	25.4	98.5
from Uganda	0.0	0.0	0.9	44.2	0.0	0.0	0.0	4.0
extra-regional	0.0	3.7	45.3	201.1	0.0	1.9	20.7	86.6
Panel B. Permanent employment, firms with positive recorded permanent employment								
Permanent employees (E: 797; U: 1,345)	3.0	10.0	36.6	53.3	2.2	7.0	28.4	35.8
Permanent wage bill (E: 797; U: 1,345; m. 2010 RWF)	2.9	16.9	120.3	188.5	2.3	13.9	83.8	147.7
Panel C. Domestic network, counts								
Suppliers	4.0	23.0	68.0	49.5	4.0	20.0	54.0	39.3
Customers	0.0	8.0	65.0	80.0	0.0	5.0	48.0	59.8

	Exposed	Unexposed
Panel D. Participation, percent of firms		
Imported in 2018	82.6	71.1
from Tanzania	8.3	4.7
from the DRC	3.4	2.4
from Burundi	1.3	0.5
extra-regionally	74.7	66.8
Exported in 2018	10.5	5.8
Positive recorded employment	47.0	52.3
Any domestic supplier	95.3	95.2
Any domestic customer	69.3	67.5

Notes: Estimation sample of 4,267 firms, measured in 2018. Exposed firms are the 1,694 with strictly positive Uganda import share in 2013–2015. Monetary values are deflated to 2010 Rwandan francs. Except for Uganda import values, continuous variables are capped above their calendar-year 99.5th percentile. Uganda import values remain uncapped. For Panels A–C, missing outcome values are treated as zero when computing the statistics. Panel B includes only firms with positive permanent recorded employment and reports permanent employees and permanent payroll. Panel C counts distinct verified counterparties; no firms are separately excluded. Panel D reports the percentage of firms with a positive recorded value for each listed activity.

Table 3: Pre-closure origin overlap among Rwandan importers, 2016–18

	Share of base group also importing from, %				
Base group (firms)	Uganda	Tanzania	DRC	Burundi	Non-neighbors
Uganda importers (1,647)		15.5	6.1	2.8	90.5
Tanzania importers (434)	58.8		9.2	6.9	85.0
DRC importers (247)	40.5	16.2		9.7	66.4
Burundi importers (82)	56.1	36.6	29.3		79.3
Non-neighbor importers (3,981)	37.4	9.3	4.1	1.6	

Notes: Firm-level cross-section of the 4,267 firms in the estimation sample. A firm is counted in a group if it imported from that origin at any point in 2016–18. Origin groups are not mutually exclusive.

4 Empirical strategy

4.1 Specification

We estimate

$$y_{it} \;=\; \alpha_i \;+\; \lambda_t \;+\; \sum_{\substack{t=2016\\ t\neq 2018}}^{2020} \beta_t\big(\text{Exposure}_i \times \mathbf{1}\{t\}\big) \;+\; \varepsilon_{it}, \tag{1}$$

with firm and year fixed effects and standard errors clustered at the firm level. β_t is the effect of moving a firm from no Uganda exposure to full exposure. Because treatment occurs at a single common date, the concerns about heterogeneity-robust estimators for staggered designs do not apply.

Value outcomes enter in inverse hyperbolic sine (asinh) so that firm-years with zero trade are retained, but we report participation indicators alongside every value outcome and treat those as the interpretable ones. All monetary values are deflated to 2010 Rwandan francs. An asinh coefficient on an outcome with a large mass at zero is not an elasticity and is not invariant to the unit of account. To facilitate the interpretation, we therefore also report, for each trade outcome, the participation margin and the log value conditional on positive trade.

4.2 Precision and the minimum detectable effect

Several of the paper's results are nulls, and a null is only informative if the design had the power to detect an effect worth caring about. Throughout we therefore report the *minimum detectable effect*, the smallest true effect the design would have detected with 80% probability at a 5% size, equal to 2.802 times the standard error.

4.3 Entropy balancing

Treated firms are larger than the average importer, so we reweight controls by entropy balancing (Hainmueller, 2012) to match on 2018 asinh levels of total sales, network sales, and network purchases. We deliberately do *not* balance on pre-period growth. Balancing on growth in the same variables that appear as outcomes forces their pre-treatment coefficients to zero by construction, producing figures that appear to pass a parallel-trends test the specification cannot fail. Levels-only balancing leaves the pre-trends estimated. The weights are well behaved, with an effective sample size of 2446 of the 2,573 control firms and a maximum control weight of 1.24. We use these weights for every implemented firm-level non-exit event-study outcome. Treated observations each have weights of 1. Exit is estimated separately and remains unweighted.

4.4 Pre-trends

The design relies on three years of pre-closure observations, which we use to assess the parallel trend assumption underpinning the validity of the estimation. It is not flat everywhere, and the outcomes divide into three groups.

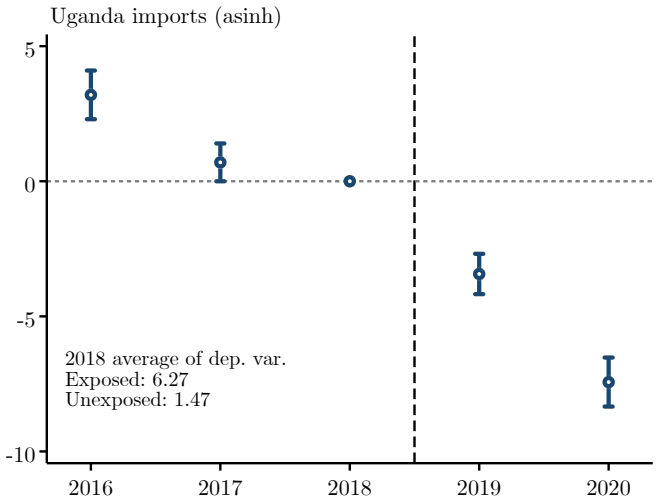
The first group cannot have a flat pre-period. Exposure is defined as the Uganda share of 2013–15 imports, so exposed firms have higher Uganda imports in 2016 and 2017 than unexposed firms by construction, and the movement toward 2018 is that construction unwinding. Imports from Uganda carry pre-coefficients of 3.20 and 0.70 (asinh imports, signifcant at 1 and 10% levels, respectively), and Uganda import participation 0.194 and 0.050 (1 and 5%). These are the first stage. They are not offered as a test of parallel trends and nothing in the paper rests on their being flat.

The second group has broadly flat pre-trends. It includes 10 outcomes of interest: total imports (asinh), the probability of importing, the probabilities of importing from Tanzania and Burundi, the four domestic value and participation outcomes, total sales (asinh), and permanent employment (asinh). All pre-2019 coefficients of these outcomes are indistinguishable from zero with the exception of the 2016 coefficient on domestic purchases (asinh), which is marginally significant at the 5% level ($p = 0.0454$), and negative. When testing 20 pre-period coefficients for significance at the 5% level, we would expect to find one to be significant due to random chance even if the underlying effect were truly zero. We therefore see these estimates as being broadly consistent with parallel trends for these outcomes.

The third group is estimated and does not have flat pre-trends. It contains four outcomes; imports from neighbouring countries (asinh), the probability of importing from DRC, the probability of importing from the region, and the wage bill. For the first three of these outcomes, the 2016 coefficients are negative and significant at the 5% level. Imports from neighbouring countries have a 2016 coefficient of -0.67, while the probability of importing from one of these countries has a 2016 coefficient of -0.041. Both are negative and significant at the 5% level, against positive post-period estimates. Exposed firms were therefore already on a differential upward trend, and the post-period estimates may overstate the effect of the closure. The probability of importing from the DRC has a 2016 coefficient of -0.024, also negative and significant at the 5% level, against post-period estimates of 0.014 and 0.001 that are not distinguishable from zero. The wage bill has a 2016 coefficient of 0.38, positive and significant at the 5% level, against post-period estimates of 0.32 and 0.50 of the same sign, which is why we treat the permanent wage bill as uninformative in Section 5.6.

On balance, the evidence points toward a cautious reading of our estimates, while not invalidating the design. The majority of our outcomes of interest have flat pre-trends, and we refrain from interpreting the few that do not.

Figure 5: Imports from Uganda



Notes: Year-by-year coefficients for asinh imports from Uganda, the first row of Panel A in Table 4. Specification, sample, weighting and clustering as in that table. The 2018 interaction is omitted and plotted at zero, the dashed line separates 2018 from 2019, and bars are 95% confidence intervals. The 2018 averages shown in the panel are descriptive means for firms with strictly positive and with zero exposure.

5 Results

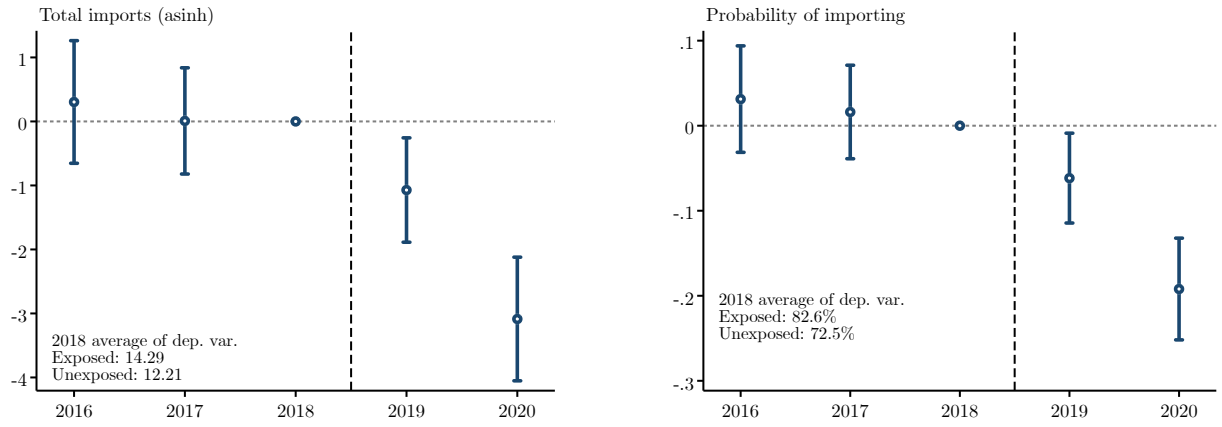
We first document the collapse in Uganda sourcing and the fall in total imports. We then examine substitution toward other origins, before turning to firm exit, domestic connections, and real outcomes among continuing firms.

5.1 Uganda-Rwanda trade

Uganda sourcing collapses for Rwandan firms. The value of Uganda imports falls by 3.43 asinh points in 2019 and 7.43 in 2020 for a fully exposed firm (baseline = 6.27). Decomposed, the probability of importing from Uganda falls 19.4 and 44.2 percentage points, while the log value among firms still importing is not significantly different from zero in either year. Note that the conditional log-value result only applies to firms that still report positive Uganda imports and therefore describes a very selected post-treatment sample (the ban on imports from Uganda was total, in theory). In 2019, 320 exposed firms (18.9%) and 76 unexposed firms (3.0%) still report positive Uganda imports, most of which happened before the closure started in February 2019. In 2020, the first full year post closure, only 8 exposed firms (0.5%) do so, compared with 2 unexposed firms (0.1%). Figure 5 plots the asinh series, while Table 4 decomposes the change into extensive and intensive margins.

The pre-period coefficients on total imports from Uganda are positive, significant and declining: 3.20 in 2016, 0.70 in 2017 and 0 in 2018, by construction. This progressive unwinding of exposure to Ugandan imports from the 2013-2016 window–when exposure is measured–is not an evidence of

Figure 6: Total imports and import participation



(a) Total imports (asinh)

(b) Probability of importing

Notes: Year-by-year coefficients for Panel B of Table 4. Specification, sample, weighting and clustering as in that table. Plotting conventions as in Figure 5.

pre-trend violation for the reason given in Section 4.4. The sign is the opposite of what the same object shows when exposure is measured on 2017–18 (Table 7, Section 5.7), which is the clearest illustration of why the exposure window matters.

5.2 Total imports and import participation

The fall in Uganda imports was not fully offset by imports from other origins. A fully exposed firm's total imports fall 1.07 asinh points in 2019 and 3.09 in 2020, and its probability of importing at all falls 6.2 and 19.2 percentage points. In 2018, 82.6% of exposed firms imported; the entropy-weighted mean among unexposed firms was 72.5%. The pre-period coefficients are 0.30 and 0.01 on values and 0.031 and 0.016 on participation, none of them significant, so these are identified off a flat pre-period. Figure 6 plots the value and participation series in panels (a) and (b), respectively, and Panel B of Table 4 reports the coefficients.

The magnitudes deserve emphasis because they are much smaller than they appear under the alternative exposure window. Measuring exposure on 2017–18 instead gives a fall in import participation of 25.9 and 34.4 percentage points (see Section 5.7 for a complete comparison of the exposure definitions), with pre-period coefficients showing significant, negatively-sloped pre-trends (see Online Appendix OA.C). Almost all of that apparent collapse is the mechanical difference between exposed and unexposed firms in the year the treatment variable is built from, and it disappears once the two windows are separated. What survives is a real but far more modest contraction of the extensive margin of importing.

5.3 Regional substitution

Table 4 reports the event-study coefficients for the trade outcomes and Table 6 those for the domestic network and real outcomes. Imports from the DRC, Burundi and Tanzania rise 1.56 asinh points in 2019 and 1.61 in 2020 (baseline = 1.96). Participation rises 8.4 and 8.9 percentage points (baseline = 11.8%), and the log value among firms with positive regional imports rises 0.65 and 0.52 (baseline = 15.88) although this effect is not distinguishable from zero at the 5% level. The 2016 pre-coefficient on regional import values is −0.67 and significant, and on regional participation −0.041 and significant. Both run against the post-period sign, so the estimates may overstate the regional response.

Origin by origin, the result is that imports shift from Uganda toward Tanzania. For a fully exposed firm, the probability of importing from Tanzania rises 8.2 points in 2019 and 9.0 in 2020, with pre-coefficients of −0.011 and −0.006, neither distinguishable from zero. For scale, the 2018 participation rate among all positively exposed firms was 8.3% (Table 2), similar in magnitude to the percentage-point estimates for full exposure. The DRC and Burundi effects are small. The DRC coefficients are 0.014 and 0.001 and Burundi's are 0.003 and 0.007; none is distinguishable from zero at the 5% level. Figure 7 plots the value and participation series. The Tanzanian participation series sits on zero through the pre-period and steps up in 2019 without retracing. Under the alternative exposure window of 2017–18 the DRC does respond, at 1.8 and 1.3 points. That estimate does not survive the cleaner design, and we do not rely on it.

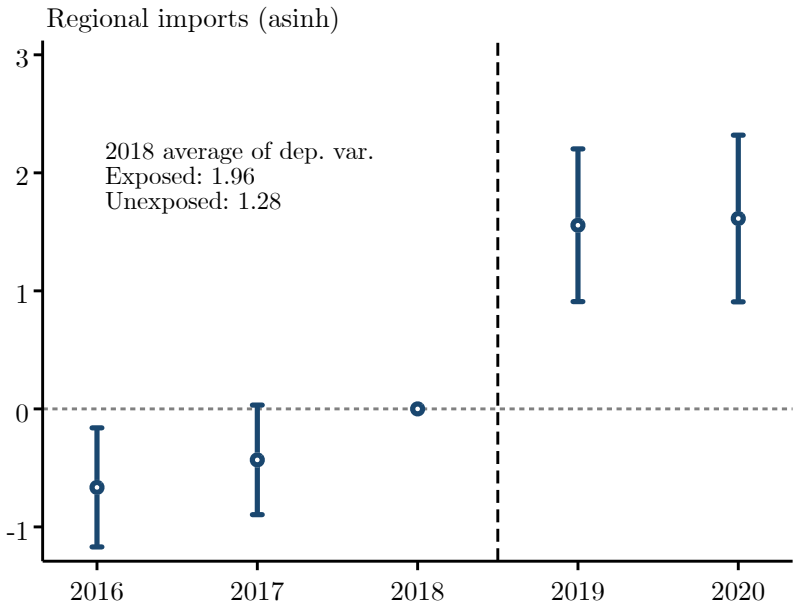
The substitution origins are new for many firms. Table 3 reports pre-closure origin overlap. Only 15.5% of Uganda importers also source from Tanzania, 6.1% from the DRC, and 2.8% from Burundi. Set against a 8.2 point increase in Tanzanian participation, this implies that many of the firms that begin importing from Tanzania after the closure had not used the origin before.

We can test this directly rather than inferring it from aggregates. Of the 272 exposed firms that import from Tanzania in 2019–20, 126, or 46.3%, had recorded no Tanzanian import at any point in 2016–18. Restricting to firms that had sourced at least half their imports from Uganda, the share with no recorded Tanzanian imports in 2016–18 rises to 66.2%. The more exposed the firm, the more likely its replacement origin is one it had not used during 2016–18.

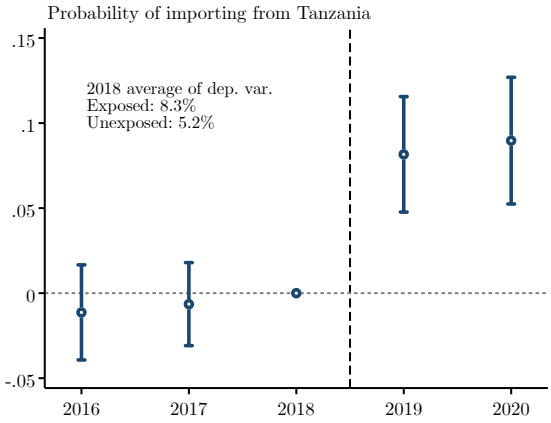
This is origin adoption rather than the activation of redundancy. Among exposed firms importing from Tanzania in 2019–20, about half had no recorded Tanzanian imports in 2016–18; this share is about two-thirds among those with at least 50% Uganda exposure.

As shown in Online Appendix OA.C, extra-regional sourcing is not cleanly identified under either window. Under the main specification, imports from non-neighboring origins are not significantly different from zero in 2019 or 2020 (Online Appendix Table OA.C.1), and the pre-period is not clean. Under the 2017–18 window the same outcome is significant in both years, but against pre-period coefficients of the same size and of both signs. We therefore draw no conclusion about extra-regional sourcing.

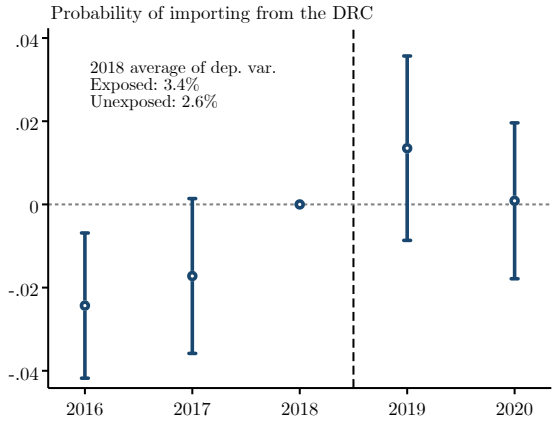
Figure 7: Regional substitution, values and participation



(a) Import values from the region (asinh)



(b) Probability of importing from Tanzania



(c) Probability of importing from the DRC

Notes: Year-by-year coefficients for three Panel C outcomes in Table 4: asinh imports from Tanzania, the DRC and Burundi in panel (a), and origin-specific participation in panels (b) and (c). Specification, sample, weighting and clustering as in that table. Plotting conventions as in Figure 5.

5.4 Exit

A second major response to the border closure occurs along the extensive margin of firm survival and exit. A contemporaneous exit indicator is mechanically zero before the first post-year in any sample conditioned on surviving to 2018, so it admits no test of parallel trends. We therefore estimate on the unbalanced 2010–2021 panel with a discrete-time hazard, in which the risk set is

Table 4: Foreign sourcing after the closure

	Pre-period		Post-period	
Outcome	2016	2017	2019	2020
Panel A. Ugandan imports				
Imports from Uganda (asinh)	3.197***	0.699*	−3.432***	−7.432***
	(0.460)	(0.357)	(0.381)	(0.463)
Pr(import from Uganda)	0.194***	0.050**	−0.194***	−0.442***
	(0.030)	(0.024)	(0.025)	(0.028)
Log value | positive	0.446**	−0.027	−0.196	1.541
	(0.193)	(0.160)	(0.194)	(1.272)
Panel B. Total imports				
Total imports (asinh)	0.304	0.008	−1.072***	−3.087***
	(0.489)	(0.423)	(0.416)	(0.493)
Pr(any imports)	0.031	0.016	−0.062**	−0.192***
	(0.032)	(0.028)	(0.027)	(0.031)
Panel C. Regional substitution				
Imports from DRC/Burundi/Tanzania	−0.665***	−0.432*	1.556***	1.613***
	(0.257)	(0.237)	(0.330)	(0.360)
Pr(import from region)	−0.041**	−0.028*	0.084***	0.089***
	(0.016)	(0.015)	(0.020)	(0.021)
Log value | positive	−0.459	−0.090	0.647*	0.517
	(0.475)	(0.335)	(0.355)	(0.353)
Pr(import from Tanzania)	−0.011	−0.006	0.082***	0.090***
	(0.014)	(0.012)	(0.017)	(0.019)
Pr(import from DRC)	−0.024***	−0.017*	0.014	0.001
	(0.009)	(0.009)	(0.011)	(0.010)
Pr(import from Burundi)	−0.004	−0.001	0.003	0.007*
	(0.004)	(0.005)	(0.005)	(0.004)

Notes: Event-study coefficients from (1). Exposure is the Uganda share of 2013–15 imports and the effect is that of moving it from 0 to 1. Estimation sample of 4,267 firms, 2016–2020, 2018 omitted. Firm and year fixed effects. All outcomes use the 2018 levels-only entropy weights described in Section 4; exposed observations retain unit weight. Standard errors clustered at the firm level in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. Value outcomes are asinh of 2010 Rwandan francs. Probability outcomes are untransformed, so their coefficients are shares, not percentages. Table 6 reports the domestic-network and real outcomes.

firms alive at $t-1$ and pre-closure exit variation exists.

We define a firm as alive in a year if it reports positive balance-sheet sales in its income-tax return and an exit if it records no positive balance-sheet sales; we estimate the hazard on the window 2012–2020 with 2018 as the reference year. We also report estimates below using alternative definitions of whether a firm is alive and obtain consistent results across definitions. The hazard specifications control for lagged sales as a proxy for firm size, since larger firms are less likely to

exit.

The exit specifications use exposure measured on 2017–18 rather than 2013–15. The earlier vintage does not have a usable pre-period here as conditioning on firms importing in 2013–15 selects on survival through to the exposure window and removes the pre-closure exit variation that identifies the hazard in the pre-period. The 2017–18 vintage preserves that variation, at the cost of the contamination that affects the trade specifications (a cost that does not apply to exit, since the exit outcome is not mechanically related to import composition in the omitted year).

The exit hazard of a fully exposed firm rises 7.3 percentage points in 2019 and 3.7 in 2020. Pre-closure coefficients are 0.026, -0.035^{***} and -0.027^{**}, so the pre-closure hazard is lower for fully exposed firms leading up to the border closure. Pre-trends are a concern here, and the post closure coefficients should therefore be interpretated with caution. Because the hazard is a linear probability model, firm fixed effects can be included alongside the year effects, and adding them leaves the 2019 coefficient largely unchanged at 0.071 and raises the 2020 coefficient to 0.073. Figure 8 plots the headline specification.

The response is confined to the most exposed. Splitting exposure into firms above 75% and firms between zero and 75%, against the same zero-exposure comparison group, the heavily exposed carry all of it. Their hazard rises 7.2 percentage points in 2019 and 3.5 in 2020, while the moderately exposed sit at -1.4 and -1.2 points. The Wald tests comparing the two groups give $p < 0.001$ in 2019 and $p = 0.0021$ in 2020. The regression estimation sample contains 1,593 heavily exposed firms, 2,273 moderately exposed firms, and 9,970 zero-exposure firms. Online Appendix Figure OA.D.1 plots both groups' event-study coefficients. Neither group has a flat pre-period, so this split belongs with the outcomes set out in Section 4.4, but the 2019 estimate for the heavily exposed is more than twice any of their pre-closure coefficients. Exit is concentrated among firms that lost most of their import basket rather than spread across exposed firms generally.

A concern is that 6,935 of the 31,091 firms in the exit panel have no positive measured total imports in 2017–18 and enter with exposure set to zero, so the estimate could be picking up importers against non-importers. Restricting the panel to the 24,156 firms with positive measured total imports leaves 11,351 firms and 51,987 firm-years in the hazard regression and gives coefficients of 0.080 (s.e. 0.013) in 2019 and 0.040 (s.e. 0.014) in 2020. The restricted-sample coefficients are also positive in 2015 (0.044) and 2017 (0.029), so this exercise is a sample-composition check rather than a separate event-study estimate.

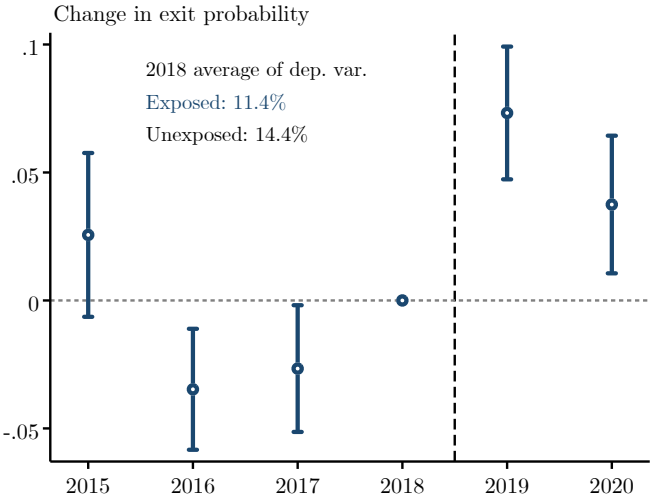
Survival can also be defined on the VAT annexes rather than on income-tax filings. Table 5 re-estimates the hazard counting a firm as alive if it records any activity in the annexes, and again if it records either balance-sheet sales or network activity. The coefficients across all three definitions of firm exit are positive, large, and significant in both years. While the post-closure estimates are similar in size and significance across definitions, there is substantial variation in the pre-closure coefficients. This suggests that the pre-trends are sensitive to the survival definition, and that the post-closure estimates should be interpreted with caution.

Table 5: The exit hazard, by survival definition

	Pre-period			Post-period	
Survival definition	2015	2016	2017	2019	2020
Balance-sheet sales	0.026	−0.035***	−0.027**	0.073***	0.037***
	(0.016)	(0.012)	(0.013)	(0.013)	(0.014)
VAT-network activity	−0.004	−0.013	−0.021*	0.080***	0.064***
	(0.015)	(0.011)	(0.012)	(0.012)	(0.012)
Sales or network activity	0.038***	−0.005	−0.032***	0.072***	0.058***
	(0.014)	(0.009)	(0.009)	(0.011)	(0.011)

Notes: Effect of moving exposure from 0 to 1 on the probability of exit, in shares. The unbalanced source panel contains 31,091 firms observed 2010–2021; the regression risk set varies by survival definition. Estimation window 2012–2020, 2018 omitted, year fixed effects with lagged sales as a control. Canonical sales are capped above the calendar-year 99.5th percentile before the lag and asinh transformation; exposure and survival indicators remain raw. A firm is alive under the first definition if it reports positive balance-sheet sales, under the second if it records any domestic sale or purchase in the VAT annexes, and under the third if either holds. Standard errors clustered at the firm level in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. Online Appendix OA.D reports the rolling-window design.

Figure 8: Exit hazard rises sharply for fully exposed firms after the closure

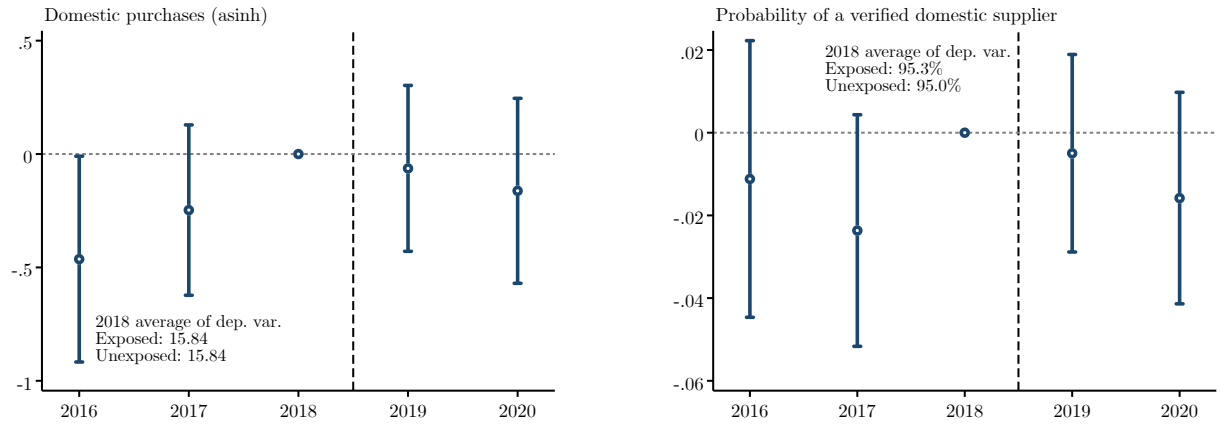


Notes: Year-by-year coefficients for the balance-sheet sales row of Table 5. Specification, sample and clustering as in that table. Exposure is the firm's Uganda share of 2017–18 imports. Plotting conventions as in Figure 5. Unlike the event studies on trade outcomes, this pre-period is estimated rather than mechanically zero.

5.5 Domestic connections

We now turn to the domestic activities of exposed surviving firms in Table 6. The VAT annexes record every transaction between registered firms, so a shift toward, or a churn of, domestic suppliers should be visible if firms adjust this margin after the closure. However, there is no evidence of such adjustment. Domestic purchase values are −0.06 in 2019 and −0.16 in 2020, and the probability

Figure 9: Domestic purchases and supplier participation among exposed firms



(a) Domestic purchases (asinh)

(b) Probability of any domestic supplier

Notes: Year-by-year coefficients for the first two rows of Panel A in Table 6. Specification, sample, weighting and clustering as in that table. Plotting conventions as in Figure 5.

of having any domestic supplier is −0.005 and −0.016. On the sales side domestic values are 0.16 and −0.46 and customer participation is 0.011 and −0.022. None of the eight is distinguishable from zero. The minimum detectable effect on supplier participation is 3.4 percentage points in 2019 and 3.7 in 2020, so an effect large enough to matter economically should have been detected. When assessing the response of the domestic network is terms of number of trading partners rather than transaction values and extensive margin participation, we find that, similarly, the number of domestic suppliers shows no statistically significant post-closure change. The number of domestic customers of exposed firms is flat for most of the period and decreases slightly in 2020 (Online Appendix Figure OA.E.6, panels a and b).

Across the six domestic-network outcomes in Panel A of Table 6, nine of the twelve pre-period coefficients are indistinguishable from zero at the 5% level. The exceptions are network purchase values in 2016 (−0.46) and domestic supplier counts in 2016 (−0.115) and 2017 (−0.114). The supplier-count results therefore warrant caution, while customer counts show no significant pre-closure differences. Figure 9 plots purchase values and supplier participation; Online Appendix Figure OA.E.6 plots supplier and customer counts.

5.6 Real outcomes among continuing firms

Panel B of Table 6 and Online Appendix Figure OA.E.7 show no evidence of a contraction in recorded permanent employment among continuing firms. The coefficients for a fully exposed firm are 0.027 asinh points in 2019 and 0.080 in 2020, with the latter significant at the 5% level. Neither pre-period coefficient is significant at this threshold. The minimum effects detectable with 80% power are 0.06 and 0.11 asinh points, respectively. These results concern firms that remain active throughout the sample period and do not capture employment losses through firm exit.

Table 6: Domestic network and real outcomes

	Pre-period		Post-period	
Outcome	2016	2017	2019	2020
Panel A. Domestic network				
Network purchases (asinh)	−0.463**	−0.247	−0.063	−0.162
	(0.231)	(0.192)	(0.187)	(0.208)
Pr(any domestic supplier)	−0.011	−0.024*	−0.005	−0.016
	(0.017)	(0.014)	(0.012)	(0.013)
Domestic supplier count (asinh)	−0.115**	−0.114***	0.002	−0.107*
	(0.058)	(0.040)	(0.046)	(0.063)
Network sales (asinh)	−0.425	−0.049	0.164	−0.458
	(0.296)	(0.229)	(0.234)	(0.345)
Pr(any domestic customer)	−0.024	−0.008	0.011	−0.022
	(0.018)	(0.013)	(0.013)	(0.019)
Domestic customer count (asinh)	−0.040	−0.005	−0.050	−0.210**
	(0.065)	(0.044)	(0.052)	(0.083)
Panel B. Real outcomes				
Total sales (asinh)	0.180	0.346	0.511*	−0.507
	(0.271)	(0.216)	(0.262)	(0.390)
Permanent employment (asinh)	0.065*	0.019	0.027	0.080**
	(0.036)	(0.020)	(0.020)	(0.038)
Permanent wage bill (asinh)	0.384**	0.178	0.322**	0.499**
	(0.175)	(0.113)	(0.139)	(0.219)

Notes: Event-study coefficients from (1), as in Table 4, with controls reweighted by levels-only entropy balancing on 2018 asinh total sales, network sales and network purchases. Supplier and customer counts refer to distinct verified domestic counterparties; a missing counterparty record is coded as zero. Standard errors clustered at the firm level in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. Minimum detectable effects at 80% power for the 2019 and 2020 coefficients: total sales 0.73 and 1.09, permanent employment 0.06 and 0.11, permanent wage bill 0.39 and 0.61, Pr(any domestic supplier) 0.03 and 0.04.

Total sales are estimated less precisely. The coefficients are 0.51 asinh points in 2019 and −0.51 in 2020, neither significant at the 5% level. The minimum effects detectable with 80% power are 0.73 and 1.09 asinh points. The sign reversal and wide confidence intervals leave the sales response uncertain. The permanent-wage-bill coefficients are positive and significant at 0.32 and 0.50 asinh points. The minimum effects detectable with 80% power are 0.39 and 0.61 asinh points, respectively. However, the positive and significant 2016 coefficient of 0.38 raises a pre-trend concern, limiting the causal interpretation of the post-closure estimates.

5.7 Robustness to the exposure window

The main specification measures exposure three years before the closure, which restricts the sample to firms already importing in 2013–15. The early window therefore trades some contemporaneous relevance for a cleaner separation between exposure measurement and the event-study period. The regional result could be a feature of established importers, or the estimates could simply be noisier because early exposure is a weaker proxy for the exposure that mattered in 2019.

Table 7 re-estimates the trade outcomes with exposure measured on 2017–18 imports, on the wider 5,846-firm panel that includes importers entering after 2015. The exercise therefore changes both the exposure vintage and the estimation sample. Regional substitution is present in both. The Tanzanian participation effect is 0.082 and 0.090 under the main specification and 0.064 and 0.058 under the later window, and regional import values are 1.56 and 1.61 against 1.33 and 1.22. The later window gives smaller estimates, which is what attenuation from a mechanically contaminated comparison would produce. The change in regional sourcing is not an artifact of conditioning on long-running importers.

Participation in DRC imports rises 1.8 and 1.3 points under the later window and is not distinguishable from zero under the main one, so the DRC belongs to the weaker of the two designs. And total imports and import participation, which the main specification identifies, are not identified under the later window at all, for the reason given in Online Appendix OA.C.

Table 7: Trade outcomes under the two exposure windows

	Exposure 2013–15		Exposure 2017–18	
Outcome	2019	2020	2019	2020
Imports from Uganda (asinh)	-3.432^{***}	-7.432^{***}	-5.951^{***}	-10.154^{***}
Imports from the region (asinh)	1.556^{***}	1.613^{***}	1.333^{***}	1.222^{***}
Pr(import from the region)	0.084^{***}	0.089^{***}	0.078^{***}	0.069^{***}
Pr(import from Tanzania)	0.082^{***}	0.090^{***}	0.064^{***}	0.058^{***}
Pr(import from DRC)	0.014	0.001	0.018^{***}	0.013^{**}
Total imports (asinh)	-1.072^{***}	-3.087^{***}	-3.730^{***}	-4.827^{***}
Pr(any imports)	-0.062^{**}	-0.192^{***}	-0.259^{***}	-0.344^{***}
Firms	4,267		5,846	
Pre-period years	2016–17		2015–17	

Notes: Entropy-weighted event-study coefficients from (1), effect of moving exposure from 0 to 1, 2018 omitted, firm and year fixed effects, standard errors clustered at the firm level. Within each exposure-window sample, controls are reweighted to the corresponding exposed group using the same 2018 levels-only entropy-balancing variables. The left block is the main specification of the paper. The right block measures exposure on 2017–18 imports and runs on the wider panel, which includes importers that entered after 2015. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. Uganda import values and both exposure measures remain raw; eligible continuous outcomes are capped above the common calendar-year 99.5th-percentile thresholds. Pre-period coefficients for the last two rows under the later window are as large as the post-period ones, which is why those estimates are not reported as results. See Online Appendix Table OA.C.1.

5.8 Limitations of the design

Four features of the data and setting constrain what the design can identify.

First, our estimates capture effects on formal Uganda-sourced trade as recorded in Rwandan customs. The documented informal leakage takes the form of small-scale smuggling, with goods concealed in motorcycle compartments, under agricultural produce, and in personal luggage, rather than formal commercial shipments (Top Africa News, 2025). Our estimation sample is VAT-registered firms operating through formal customs channels, whose import activity is recorded in ASYCUDA declarations. Flows of that kind are neither conducted by firms of this type nor visible in the administrative data we use, so they are unlikely to affect the estimates materially. To the extent that any of the firms we observe substituted into informal channels, the estimates overstate the disruption those firms experienced.

Second, the VAT invoice annexes that underpin the domestic-network results cover only transactions between VAT-registered firms. Unprocessed agricultural and livestock products are exempt from VAT in Rwanda, as are agricultural inputs and equipment. Transactions in these goods do not generate VAT invoices regardless of firm size, and are therefore invisible in the annexes that identify domestic buyer–seller links. We can bound the resulting gap. Classifying the pre-closure Uganda basket at the six-digit level, 25% of its 2016–18 customs value falls in the unprocessed agricultural and livestock goods that the exemption covers, once milled and otherwise processed food products are treated as taxable. Exposure to the gap is concentrated rather than general. Three quarters of Uganda-sourcing importers buy no exempt goods from Uganda at all, while 18% source more than nine tenths of their Uganda basket in exempt goods, and that minority accounts for 27% of pre-closure Uganda import value. A domestic shift into locally sourced unprocessed agricultural inputs would be invisible for those firms. For the three quarters whose Uganda basket is entirely VAT-observable, the annexes see the goods in question, and the domestic null for them is measured rather than assumed.

Third, customs declarations record the origin country of each shipment but not the identity of the foreign seller. We observe which origins a firm uses and when it starts using a new one, but nothing about individual cross-border partnerships. Exposure is defined at the firm–partner-country level throughout, and no result in this paper identifies effects on specific buyer–seller pairs across the border.

Fourth, 2020 is both the first full year of the border closure and the year of the COVID-19 pandemic. Year fixed effects absorb any shock common to exposed and unexposed firms, but they do not account for heterogeneous COVID effects that may have fallen disproportionately on already-weakened exposed firms. The 2019 coefficients, which predate the pandemic, are therefore the cleaner estimates of the closure effect. The 2020 coefficients should be interpreted as reflecting the joint impact of the closure and any differential COVID exposure.

6 Mechanisms and future work

Exposed firms lose an origin, replace it abroad and regionally within a year (about half of exposed firms importing from Tanzania in 2019–20 had not used that origin in 2016–18), do not touch their own domestic network, and exit at a much higher rate. The evidence is consistent with the following simple search framework. Firms facing corridor disruption search for replacement origins along the surviving corridor. The corridor infrastructure that makes Tanzania accessible is a shared resource that benefits all searching firms simultaneously, regardless of their pre-closure portfolios. Search succeeds quickly for standardized goods in thick markets, but fails at the product level for specific agricultural inputs. Domestic search does not occur because the relevant market, domestic suppliers of previously imported inputs, does not exist at scale. Firms for whom search fails exit.

6.1 Corridor redundancy, not supplier redundancy

Rwanda is landlocked but connected to two deep-sea ports: Mombasa via the Northern Corridor through Uganda and Kenya, and Dar es Salaam via the Central Corridor through Tanzania. Each corridor also provides access to tariff-free imports from major EAC partners, Uganda and Kenya on the Northern Corridor, Tanzania and Kenya on the Central. The closure removes one corridor and leaves the other, and the observed substitution follows the surviving route. This is redundancy at the level of infrastructure rather than the firm's supplier portfolio, and the distinction matters. A firm benefits from corridor redundancy whether or not it has diversified suppliers, provided it can find a counterparty on the surviving route. Table 3 speaks only to the second. Most exposed firms have no regional sourcing to fall back on. One extension in future work would be to categorize products by whether Tanzania has a revealed comparative advantage in them. Firms importing such products would presumably be less disrupted by the closure, substituting more and exiting less at the same exposure.

6.2 New origins, adopted quickly

Only 13.9% of exposed firms source anything from Tanzania before the closure, and roughly half of exposed firms importing from Tanzania in 2019–20 had no recorded Tanzanian imports in 2016–18. Search models predict that the cost of finding a new supplier depends on the thickness of the relevant market and the informational distance between buyer and seller (Spray, 2021). Uganda-sourced imports are concentrated in relatively standardized goods, such as vegetable products, foodstuffs, basic metals and chemicals, for which many potential suppliers exist along the Central Corridor. Search over a thick market for standardized inputs is exactly where fast replacement should be feasible. Firms already familiar with the Central Corridor should adopt Tanzanian suppliers faster. As prior users begin at full participation, the question has to be shifted to focus the value or the range of what they buy rather than to whether they buy at all.

6.3 Substitution happens at the firm level, not the product level

The firm-level results show exposed firms turning to the region. The product-level customs records show something different. We construct a panel at the level of the firm, the six-digit HS product and the origin country (a *triple*) from the Rwandan customs declarations. A firm–product *pair* aggregates over origins. Among the 19,397 firm–product pairs sourced from Uganda in 2017–18, only 28.8% have any alternative source for the same six-digit product in the same window. Of the 19,397 pairs, 27.8% have an extra-regional alternative and only 1.7% a regional one. The two categories are not mutually exclusive. Redundancy exists, but essentially never in the region that later buffers the shock. Product-level double sourcing and the firm-level figure of 13.9% of exposed firms sourcing anything from Tanzania are therefore consistent, because pre-closure alternatives are almost entirely extra-regional.

Replacement of the specific disrupted product is rare. A pair counts as disrupted if the firm records no Uganda imports of that product in 2019. Of the 18,071 Uganda-sourced pairs disrupted on that definition, only 10.3% obtained a non-Uganda source for the same six-digit product within a year and 12.0% within two. Of that 12.0%, regional origins account for 1.7 percentage points and extra-regional origins for 11.0. The two overlap slightly, since a pair can be re-sourced from both.

The two sets of numbers come from different universes. The event studies use the tax panel of 4,267 firms, while these product-level statistics use the wider customs universe of 24,435 importers. A fully exposed firm's import value from the region rises 1.56 asinh points and its probability of importing from Tanzania rises 8.2 percentage points in 2019, while nine in ten of the individual products it loses are not sourced again within a year, and seven in eight not within two. Firms do not replace what they lose. They reallocate their import basket, buying different goods from new regional origins. Substitution operates on the composition of what firms buy, not on the continuity of specific product lines.

Table 8 shows the sectoral split. Non-agricultural firm–product pairs have a pre-closure alternative 37.6% of the time and are replaced within two years in 15.2% of cases. For agricultural products, the vegetable goods and foodstuffs at the core of the Uganda basket, the figures are 8.3% and 4.0%. Agricultural inputs sourced from Uganda are both the least hedged before the closure and the least replaceable after it.

The relationships that do form are overwhelmingly new. Among post-closure firm × HS6 × origin triples recorded by exposed firms, 77.4% of regional triples have no counterpart in 2015–18, against 65.5% for extra-regional triples and 29.6% for the residual Uganda triples. Regional sourcing after the closure is newer than sourcing anywhere else.

6.4 Why domestic replacement does not happen

Three asymmetries seem relevant. The Uganda-sourced basket consists largely of goods Rwanda does not produce at scale, so domestic substitution would require a new domestic productive

Table 8: Product-level redundancy and substitution (percent, except counts)

	All	Non-agricultural	Agricultural
Pre-closure alternative source for the same HS6, 2017–18			
Any alternative origin	28.8	37.6	8.3
Regional (TZ/DRC/BI)	1.7	1.2	2.7
Extra-regional	27.8	37.2	5.9
Firm–product pairs	19397	13541	5856
Substitution among pairs disrupted in 2019			
Substitute within one year	10.3	13.2	3.4
Substitute within two years	12.0	15.2	4.0
... regional	1.7	1.6	2.1
... extra-regional	11.0	14.7	2.1
Disrupted pairs	18071	12802	5269

Notes: Firm × HS6 × origin panel built from Rwandan customs declarations, 2015–2020. 24,435 importers in 2017–18, of which 27.0% sourced from Uganda. A pair is Uganda-sourced if the firm imported that HS6 from Uganda in 2017–18, and disrupted if it recorded no Uganda imports of that HS6 in 2019. Agricultural products are HS chapters 1–24. Origin categories are not mutually exclusive.

capability rather than a new relationship. Firms understand the closure as a disruption to a route and a partner, not to a product market, and the efficient response to a route shock is to change route. And building a domestic supplier relationship for a previously imported input involves qualification and often supplier investment, on horizons longer than the two post-closure years observed here. The fixed cost of forming a link is precisely what the search literature identifies as the binding constraint (Startz, 2024; Eaton *et al.*, 2011). If firm IDs could be linked across data sets, an interesting specification would be to re-estimate the domestic outcomes on firms whose Uganda basket contains no VAT-exempt goods. This would help sharpen the idea that the lack of domestic capacity is a reason for the lack of subsequent domestic sourcing.

7 Conclusion

We study how firms re-source when a trade corridor closes, using the 2019–2022 Rwanda–Uganda border closure and linked customs and VAT-network microdata. Exposed Rwandan firms replace lost Ugandan sourcing quickly toward other countries in the region. For a fully exposed firm, the probability of importing from Tanzania rises 8.2 percentage points in 2019, comparable in magnitude to the 2018 participation rate of 8.3% among all positively exposed firms. Among exposed firms importing from Tanzania in 2019–20, almost half had no recorded Tanzanian imports in 2016–18. At the product level, however, replacement is rare. Only one disrupted firm–product pair in ten is re-sourced within a year, and agricultural inputs, the core of the Uganda basket, are both the least hedged beforehand and the least replaceable afterwards. Firms reallocate their import basket rather than restoring it, and they do not fully restore its size, with total imports and the probability of importing at all both falling.

The consequences for fully exposed firms vary widely as their exit hazard rises 7.3 percentage points in 2019. For surviving firms, we find no responses in the domestic buyer–seller network. The margin of adjustment firms actually use is the second corridor, not diversification within their own sourcing portfolios, of which most have none regionally. Investments that keep alternative routes viable therefore look more valuable, and considerably more valuable than expectations of rapid domestic substitution, which does not occur.

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Appendix

A Data appendix

This appendix gives the construction rules needed to interpret the estimates.

A.1 Rwanda sources and linkage

The Rwanda Revenue Authority records cover customs declarations, VAT returns and transaction annexes, PAYE payroll records, and CIT/PIT income-tax filings. A stable anonymized taxpayer identifier links these sources and years. Customs values and the monetary tax outcomes are expressed in 2010 Rwandan francs. The analysis panel is annual; the descriptive customs series in Figure 3 is monthly.

VAT Sales and Purchases Annex lines are collapsed to buyer–supplier–year links before network measures are constructed. The monetary measures use transactions with verified counterparties. Supplier and customer counts are counts of distinct verified counterparties. Invoice-line counts are retained in the code under separate names and are not used as firm degrees. The 2016 counterparty identifiers do not share a consistent identifier space with adjacent years. Within-year values and degrees remain usable, but link formation and destruction cannot be measured across 2016.

A.2 Surviving-firm panel and exposure

Sample membership is determined from raw values before any upper-tail adjustment. A firm is active in a year when total sales, verified domestic sales, or verified domestic purchases is nonzero. The parent panel requires activity in every year from 2015 through 2020 and contains 5,846 firms. The main event studies use 2016–2020 and omit 2018.

The main sample additionally requires positive total imports summed over 2013–2015. It contains 4,267 firms, of which 1,694 have positive exposure and 2,573 have zero exposure. Exposure is the firm's raw Uganda import value divided by its raw total import value over 2013–2015. Neither exposure nor its components are winsorized. The separate 2017–18 exposure-window exercise reconstructs the same share from raw 2017–18 imports on the parent panel.

A.3 Outcomes, upper tails, and weights

Total sales use the annual canonical sales measure and, when it is missing, verified domestic sales plus exports. Employment and payroll include permanent staff only. Casual and secondary employment are excluded. Missing sales and purchase flows are treated as zero when applying the activity rule; remaining missing annual flows and counts are then set to zero for analysis. Participation indicators are defined from the corresponding raw values before any cap.

For each eligible continuous outcome and balancing variable, the code calculates the 99.5th percentile separately by calendar year on the 5,846-firm parent panel. Values above the percentile are replaced by the percentile. No observation is deleted or changed to missing, and values at or below the percentile are unchanged. Asinh transformations and conditional-positive logs are constructed afterward. Uganda import value is the sole value exception and remains uncapped in every year and specification. Binary outcomes, exposure, survival indicators, zeros, and missingness are not winsorized.

The entropy weights are estimated from the 2018 capped asinh levels of total sales, verified domestic sales, and verified domestic purchases. Exposed firms receive weight one. The control weights are accepted only if they are positive, finite, and balance all three targets. The code reports their distribution and effective sample size.

A.4 Exit sample

The exit analysis uses a separate unbalanced panel through 2021 and does not impose the surviving-firm condition. A firm is at risk in year t when it is alive in $t-1$ under the relevant activity definition. The headline model is estimated for 2012–2020, omits 2018, includes year fixed effects and lagged asinh real sales, and clusters standard errors by firm. Exposure and survival remain raw. Annual sales are capped before the lag and asinh transformation are constructed.

A.5 Uganda

This subsection summarizes the administrative datasets available for Uganda and the way we prepare them for analysis. The central constraint is linkage. We can connect *international trade* (ASYCUDA customs) to *domestic production-network data* (VAT schedules of sales and purchases), which is enough to define treatment and control at the firm level. However, we cannot reliably link those records to firm-outcome files from Corporate Income Tax (CIT) or Pay-As-You-Earn (PAYE) because the anonymization algorithms differ across extracts. The available post-closure window is also short. Table A.1 reports the coverage windows of the main Uganda datasets.

Dataset	**Good Coverage Starts**	**Good Coverage Ends**
ASYCUDA	January 2010	November 2020
PAYE	January 2009	December 2017
CIT (UNU-WIDER)	July 2013	December 2019
CIT	January 2009	June 2019
VAT, Filing Firm Sales	July 2010	March 2020
VAT, Filing Firm Purchases	December 2009	March 2021

Table A.1: Uganda datasets: coverage windows with reliable completeness

A.5.1 Monthly VAT returns and schedules (domestic production network)

Coverage and structure. Under the VAT Act, taxable persons file a monthly return within 15 days of the period end. The extract we use runs through *April 2020*. There are two relevant identifiers: a return identifier `RTN_ID` (one per monthly filing) and a masked taxpayer TIN (`masked_tin_alpha`), which appears in different roles across files (*e.g.*, `masked_purchaser_tin_alpha`, `masked_supplier_tin_alpha`). The return is composed of several *sections* (C–H) with totals by category, and *schedules* (1–4) that list transaction-level details necessary to reconstruct buyer–supplier links.

Sections (totals). Section C reports sales by VAT status (taxable, zero-rated, exempt) and includes imported services when VAT is due. Its variables are `AMOUNT`, `RATE`, `VAT`, and a `TOT_SALES_OUTPUT_TAX` total. Section D records purchases with analogous fields and a total input tax `TOT_PURCH_INPUT_TAX`. Section F computes *input tax allowed* (`ITC_ALLOWD`) given taxable/exempt sales composition. Section G lists *payment* details (paid vs. withheld VAT), and Section H consolidates the liability: `TOT_OUTPUT`, `TOT_INPUT`, `VAT_PAYABLE`, `VAT_PAID`, and `VAT_DUE`. These sections do *not* contain counterparties. They are used to benchmark aggregates from schedules and to characterize each firm's VAT position by month.

Schedules (transactions). Schedule 1 is the *output tax schedule*. It lists each sale invoice with invoice date/number, description, taxable amount, VAT paid, and, critically, the masked TINs of the seller and purchaser (allowing us to build seller→buyer edges for the production network). Schedule 2 is the *input tax schedule* and lists each purchase invoice and likewise carries masked seller and buyer TINs. Schedule 3 lists *imported inputs* linked to customs assessments (these have historically shown inconsistencies with customs and are flagged for cross-checks), and Schedule 4 covers *administrative expenses* subject to VAT. These schedules are the backbone of the domestic B2B network. After cleaning we aggregate by month (or year) to construct firm-level totals of domestic sales and purchases to/from registered counterparties and to derive degree/volume measures (number of customers/suppliers, number of invoices, etc.).

Identifiers and linkage. All schedules carry `RTN_ID` (to connect to the monthly return) and masked TINs for seller and buyer. This permits (i) within-firm reconciliation against Section totals and (ii) cross-firm linking to form buyer–seller pairs. The masked TIN system is consistent within the VAT universe, enabling a production-network panel through April 2020. Because CIT and PAYE use incompatible masked identifiers in our extracts, we cannot link VAT schedules to those outcome files.

A.5.2 ASYCUDA customs (international trade)

Coverage and identifiers. Transaction-level customs declarations (imports and exports) are available through *November 2020*. Each line contains values and quantities by HS code, partner information (origin/destination), routing and control fields (entry/exit points, regimes, channels), weights, units, taxes, and dates. Crucially, the file includes a masked taxpayer identifier for the

trader (`tin_masked`/`masked_tin_alpha`) that is *linkable* to VAT schedule TINs, enabling analysis of matched firm records in the domestic and international production network.

Variables over time. Core items (*e.g.*, HS code at 6–10 digits, trader identifier, customs procedure, taxes, weights, partner country, registration and release dates) are present throughout 2010–2020. Some fields (*e.g.*, certain HS label variants, routing flags) become available only from 2018 onward. Monetary values are reported in UGX and USD. We use `val_usd` for cross-year comparability and retain UGX for consistency with VAT.

Construction steps. We aggregate transactions to firm–month and firm–year totals by partner and HS, harmonize HS to a common revision (HS17 at six digits) when needed, and link traders to VAT-network firms via masked TINs to identify which domestic firms are exposed to specific partner *countries*, in particular those shipping to or from Rwanda around the 2019 closure. The customs extracts identify the domestic trader only. They carry no name or identifier for the foreign counterparty, so exposure is defined at the firm–partner-country level and not at the level of a cross-border firm pair.

A.5.3 Informal Cross-Border Trade (ICBT)

Coverage and content. The Bank of Uganda/UBOS ICBT series records *lead items* moving informally by customs station, with USD values and dates. The coverage runs through *March 2018*. The files distinguish informal *exports* and *imports* and list the principal commodities contributing the bulk of informal flows (*e.g.*, cattle, milk powder), by station (*e.g.*, Elegu) and month/year. These are not firm-level and serve as contextual evidence about informal traffic near borders. We do not directly use this dataset (or its Rwandan counterpart) in our analysis.

A.5.4 Corporate Income Tax (CIT) returns

Coverage and sections. Company returns (non-individual taxpayers) are available through *April 2021*. The extract contains five groups of sections.

- A *statement of chargeable income and tax assessed* (Sections C and D), which carries return identifiers and, for incorporated taxpayers, holding-company information.
- A *profit and loss account* (Section F) with detailed revenue and cost categories (sales, local and import purchases, cost of sales, wages, overheads, depreciation, financial and administrative expenses) and bottom-line items (profit before and after tax).
- *Quantitative stock and inventory* (Section G) with opening and closing stocks and flows by item and unit.
- A *computation of tax liability* (Section H).
- Schedules for business and profession income, capital allowances, capital gains and losses, and long-term contract income.

Each section includes return dates and coverage periods.

Linkage constraint. Although CIT contains masked taxpayer IDs, they are not harmonized in the same way as the VAT/customs masked TINs in our extracts. We therefore do not link CIT to the production-network or customs universe, and we do not use CIT outcomes for treated–control analysis in Uganda.

A.5.5 Non-individual taxpayer register

Coverage and variables. The register, available through *May 2018*, appears to list formal firms with sector classifications and effective dates for various tax accounts (*e.g.*, CIT, VAT, PAYE, excise). It includes jurisdiction, taxpayer type, organizational form, masked TIN, activity description or ISIC code, and multiple effective-date fields by tax type. The file could be useful for studying firm entry and exit in Uganda, but we cannot link it to the firms in the VAT–customs panel and therefore do not use it here.

A.5.6 PAYE employer filings

Coverage and content. Monthly employer returns report employee counts and payroll tax withholdings. The extract runs to *July 2018*. The "PAYE_CIT" file we process includes employer masked TIN, jurisdiction, industry description and code, filing period, and constructed variables such as number of employees, median/average salary, and total wage bill for the filing window. As with CIT, the lack of identifier harmonization prevents linkage of PAYE employers to the VAT/customs network in our current extracts.

A.5.7 From raw files to an analysis-ready panel

Cleaning and filters. For VAT we take three cleaning steps.

- Drop lines where firms cannot be identified, for example those with missing masked TINs.
- Reconcile schedule sums to the Section C and Section D totals at the return level, flagging large discrepancies.
- De-duplicate invoices using {`masked TIN`, invoice number, invoice date, amount}.

For customs, we restrict to standard import and export regimes, retain released and cleared shipments, and harmonize HS to HS17 at six digits when comparing across years.

Aggregation. We construct firm–month and firm–year totals of domestic sales and purchases to and from registered counterparties (from Schedules 1–2), firm–month and firm–year import and export totals by partner and HS (from ASYCUDA), and network statistics, namely the number of distinct buyers and suppliers, the number of invoices, and total values, used to characterize the domestic margin for firms that also appear in customs. These aggregates underpin our treatment

definition (*e.g.*, firms trading with Rwanda) and our descriptive analysis of substitution across partners around the 2019 shock.

What we *do not* build on the Uganda side. Because of identifier incompatibilities, we do not merge CIT profit/loss accounts, balance sheet items, or PAYE payroll outcomes into the production-network/customs panel. We therefore avoid specifications that would require Uganda-side firm outcomes.

B The Ugandan side of the frontier

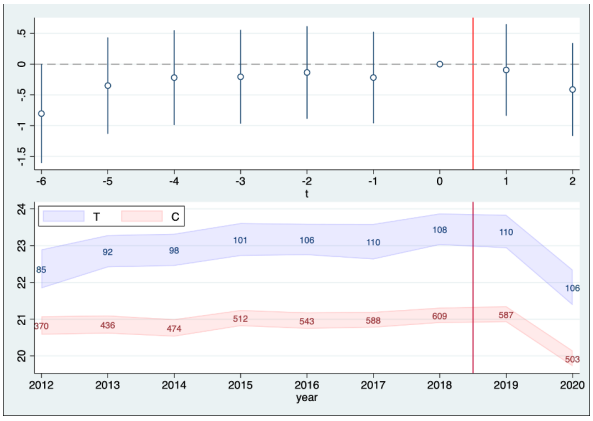
The closure was bilateral. This appendix reports what the Ugandan data support, which is considerably less than the Rwandan analysis, and why the analysis is framed around Rwanda alone.

Three constraints bind. First, the anonymization used in Ugandan payroll and corporate-income-tax records is not compatible with the identifier that links Ugandan VAT to Ugandan customs in our extracts, so no employment, sales or profit outcome can be attached to the linked panel. Second, the Ugandan customs series ends in November 2020 and the VAT buyer–seller schedules in April 2020, giving barely one post-closure year. Third, the group of Ugandan firms exporting to Rwanda before the closure is small.

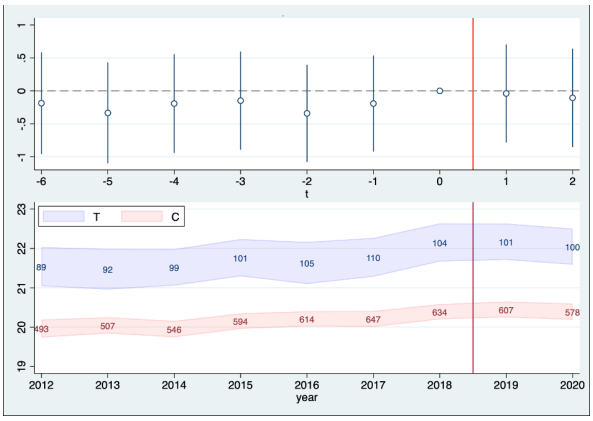
The two-country data structure permits less than it might appear. We observe, for each country, that country's domestic buyer–seller network joined to that country's own customs declarations through a common masked taxpayer identifier. What neither extract contains is an identifier for the *foreign* firm on the other side of a shipment. Rwandan customs names the Rwandan trader and the partner country, Ugandan customs names the Ugandan trader and the partner country. We therefore cannot pair a specific Rwandan importer with the specific Ugandan exporter that supplied it, and no result in this paper does so. Exposure is defined at the firm–partner-country level throughout.

Figures B.1 and B.2 report firm-level event studies for Ugandan firms that traded with Rwanda before the closure, on domestic sales, domestic purchases, and counts of domestic customers and suppliers. The estimates are centered near zero with wide intervals. Given the short post-period, the small treated group, and the absence of real outcomes, we read them as uninformative rather than as evidence of no effect, and we do not build any claim on them. They are not estimated in the continuous-exposure framework of Section 4.

Figure B.1: Ugandan firms trading with Rwanda: domestic sales and purchases



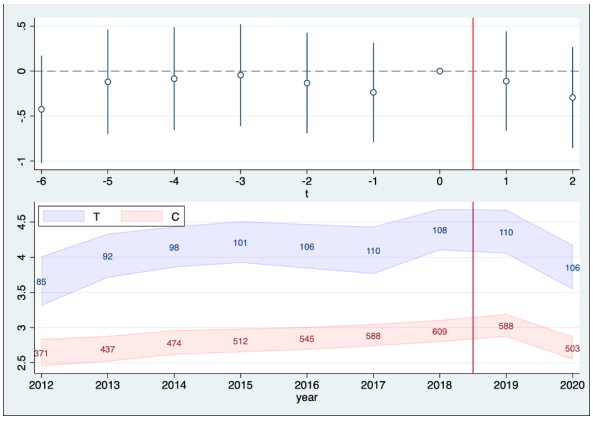
(a) Domestic sales



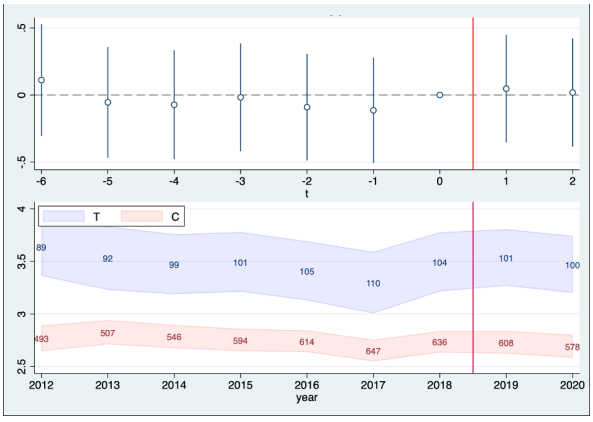
(b) Domestic purchases

Notes: Event-study coefficients with log domestic sales and log domestic purchases as dependent variables. The vertical bars show 95% confidence intervals constructed as the coefficient plus or minus 1.96 standard errors clustered at the firm level. The panel is unbalanced because observations drop out when the outcome is zero. Lower panels plot treated (T) and control (C) group means. These use a binary treatment indicator and log outcomes.

Figure B.2: Ugandan firms trading with Rwanda: domestic network size



(a) Domestic customers



(b) Domestic suppliers

Notes: As Figure B.1, with log counts of domestic customers and suppliers as dependent variables. The vertical bars show 95% confidence intervals constructed as the coefficient plus or minus 1.96 standard errors clustered at the firm level.