



Empirical effects of short-term export bans: The case of African maize[☆]



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ABSTRACT

Temporary export restrictions have been widely used in recent years in an attempt to stabilize domestic prices of staple grains. I use monthly, market-level price data to investigate the empirical effects of 13 short-term export bans on maize implemented by 5 countries in East and Southern Africa. I find no statistically significant effect of export bans on the price gaps between pairs of affected cross-border markets. My results for price gaps match those from a model simulation in which export bans are not implemented. However, prices and price volatility in the implementing country are significantly higher during export ban periods in the data than in the model simulation with no bans. Export bans in the region are imperfectly enforced, divert trade into the informal sector, and appear to destabilize domestic markets rather than stabilizing them.

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1. Introduction

The prices of basic agricultural commodities have fluctuated dramatically over the last decade. In developing countries, where food expenditure makes up a large proportion of household consumption, these price fluctuations have led to a proliferation of policies to control or stabilize food prices. Temporary export restrictions have been particularly widespread, with at least 33 countries using some form of export restriction during the 2007–2008 food price spike and its aftermath, including 4 of the top 5 rice producers (China, India, Bangladesh, Vietnam) and 7 of the top 13 wheat producers (China, India, Russia, Pakistan, Ukraine, Argentina, Kazakhstan) (Sharma, 2011). This article focuses on the most common and severe of such restrictions: the short-term export ban.

The literature on export restrictions has focused on understanding why countries implement them and the role they play in exacerbating international price spikes. Theoretically, export restrictions introduce welfare-reducing price distortions, with local farmers losing more than local consumers gain from lower domestic prices (Mitra and Josling, 2009; Liefert et al., 2011). Governments likely implement export restrictions because they

put more weight on consumers' interests than those of producers, are more concerned about negative deviations from the status quo than positive ones, or seek to avoid extreme events (Abbott, 2011). Gouel and Jean (2015) have also shown that export restrictions can be part of an optimal dynamic food price stabilization policy when consumers are risk averse and insurance markets are incomplete. Regardless of their domestic rationale, the welfare effects on other countries appear to be unambiguously negative: by cutting off supply to the world market during times of high prices, export restrictions magnify international price fluctuations and have been criticized for representing a beggar-thy-neighbor approach to trade (Headey, 2011; Martin and Anderson, 2011; Anderson and Nelgen, 2012).

This article provides new empirical evidence from East and Southern Africa that export bans do not always have the effects that governments and policy analysts think they do. Most notably, in all previous theoretical work (including Mitra and Josling, 2009; Liefert et al., 2011; Abbott, 2011; Martin and Anderson, 2011; Anderson and Nelgen, 2012), export restrictions increase the gap in price between the implementing country and its trading partner (typically modeled as the rest of the world). If the implementing country is a small open economy (as in Mitra and Josling, 2009), the world price remains unchanged while the domestic price falls. If the implementing country is large enough to affect the price of its trading partner (as in Abbott, 2011), the domestic price falls while the trading partner's price increases. In contrast to this clear and intuitive theoretical prediction, I find that export bans in East and Southern Africa have had no detectable effect on price gaps between implementing countries and their trading partners.

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Previous efforts to measure the effects of export bans in the empirical literature have been limited by endogeneity issues. Since export restrictions in most parts of the world were one-time policies implemented nearly simultaneously during the 2007–2008 food price spike (a period when many other shocks were also occurring), it has been difficult to cleanly identify their effects. Some studies have relied on computable general equilibrium models for simulations of export bans (e.g. [Diao and Kennedy, 2016](#)). Another strand of the literature has used Markov-switching vector autoregressive and other time series models to test for alternate regimes of price transmission during periods with and without export bans. [Ihle et al. \(2009\)](#) find evidence of two price transmission regimes between Tanzania and Kenya, but the “high margin” transmission regime does not correspond well to the three Tanzanian maize export bans in their dataset. [Götz et al. \(2013\)](#) find a regime of reduced integration between wheat markets in Russia and Ukraine and the world market during those countries’ 2007–2008 export restrictions. [Baylis et al. \(2014\)](#) find reduced market integration both between Indian ports and the world market and between producing and consuming markets within India during India’s export bans on rice and wheat.

This paper contributes to the existing empirical literature on export bans in three main ways. First, by using market-level data from a region where export bans have been implemented repeatedly by multiple countries, I end up with a larger dataset with much more variation than previous studies, enabling me to run explicit tests for potential endogeneity and control for unobservables through location and time fixed effects. Second, by combining my empirical analysis with simulations from an estimated model, I can compare actual market outcomes to outcomes if bans had not been implemented or if they had been implemented in different ways. Third, these data and techniques enable me to establish a new result with clear policy implications: export bans in East and Southern Africa have not had their intended effects, and they appear to lead to higher and more volatile prices in the countries implementing the bans.

I use monthly, market-level maize price data from 49 large hub markets in 12 countries over a 10-year period during which 5 of these countries (Ethiopia, Kenya, Tanzania, Malawi, and Zambia) implemented 13 distinct export bans on maize (the main staple grain in the region) in response to high international prices or domestic production shortfalls. I first document a surprising and robust empirical result: export bans in this region do not have a statistically significant effect on the gaps in prices between pairs of affected cross-border markets. In addition to holding when the bans of all 5 implementing countries are taken together, this result also holds when the bans of each of the 5 countries are considered separately.

I proceed to compare my empirical results to results from simulations using an estimated dynamic monthly model of grain storage and trade in sub-Saharan Africa that includes nearly all of the same markets and cross-border trade routes ([Porteous, 2017](#)). Price series from model simulations with and without export bans help me rule out different explanations for my results and control for potential endogeneity. When the 13 export bans are fully enforced, there is a large and statistically significant increase in cross-border price gaps during bans in the model simulations, even when traders are able to anticipate the bans with perfect foresight. The absence of an effect on the price gaps in the data matches a model simulation in which the export bans are not implemented. However, prices in both implementing and trading partner countries are significantly higher during export bans in the data than in the model simulation with no implementation, suggesting that the bans are affecting markets in other ways.

Information collected from market participants in the region indicates that export bans are imperfectly enforced, with informal

local traders as well as some formal traders who are able to secure export permits through back-door channels able to continue trading during bans. These alternative trade channels may be subject to capacity constraints, but these constraints appear to only bind at the very end of bans. However, the unpredictable, *ad hoc* nature of the bans and their enforcement appears to destabilize markets on both sides of the border. In addition to prices that are higher than they would have been without bans, price volatility is also significantly higher in the implementing country.

Taken together, my results suggest that export bans in East and Southern Africa do not have their intended effects of stabilizing or lowering domestic prices or insulating them from high international prices and have unintended destabilizing consequences instead. Policy-makers in the region and in similar countries elsewhere should therefore re-evaluate their use even when they appear justified on political economy grounds. My results are also a cautionary note for studies that have used model-based simulations to estimate the effects of export restrictions (e.g. [Ahmed et al., 2012](#); [Diao and Kennedy, 2016](#)), as these effects are likely different in practice if the export restrictions in question are imperfectly enforced.

2. A surprising empirical result

Maize is the primary staple grain produced and consumed in East and Southern Africa. Empirical evidence suggests that while imperfect competition is important in small, remote rural grain markets in sub-Saharan Africa, larger hub markets of the type considered here are competitive, with many traders and low firm concentration ratios ([Osborne, 2005, 2010](#)). Trade in the region is almost exclusively by diesel truck and is constrained by geography and the limited road network. Although most maize production is consumed domestically, maize is actively traded across all of the borders in the region. Formal maize trade volumes recorded in official trade statistics between the 12 countries considered here averaged 424,000 metric tons annually during the study period ([Gaulier and Zignago, 2010](#)), with at least another 120,000 metric tons of unrecorded informal cross-border trade ([Tschirley and Jayne, 2010](#)), together representing roughly 3% of the 19 million metric tons produced annually in the region.

My primary dataset consists of a panel of monthly maize price data from large hub markets (major towns) in East and Southern Africa assembled by the Famine Early Warning System Network (FEWS NET) and covering the 10-year period from January 2002 to December 2011. Using local newspaper archives and FEWS NET monitoring reports, I identified the starting and ending dates of 13 short-term export bans implemented by 5 countries during this period,¹ ranging in duration from 4 to 54 months ([Table 1](#)).

I then selected the major markets on either side of the affected international borders from the FEWS NET database and identified the pairs of cross-border markets directly linked by transportation infrastructure. With competitive trade, any price change caused by an export ban should be detectable at these directly-linked cross-border markets, with markets further away from the border experiencing equivalent price changes if they are trading with the directly-linked markets and no price change otherwise. The resulting dataset includes 49 markets and 40 cross-border market pairs ([Fig. 1](#)). This includes an additional 6 markets in areas not covered by the FEWS NET database in western Tanzania, eastern Malawi, and northern Mozambique, which I added to my dataset using equivalent price data from the Ministries of Agriculture (Malawi

¹ The implementation and lifting of bans is announced publicly and hence readily observable. The degree to which bans are actually enforced is unobservable, although I collect and report anecdotal evidence on enforcement later in this article.

Table 1
Dates of 13 export bans.

Country	Start month	End month	Affected pairs
Ethiopia	January-06	July-10	6
Ethiopia	March-11	Post-2011	6
Kenya	October-08	Post-2011	14
Malawi	July-05	February-07	7
Malawi	April-08	August-09	7
Malawi	December-11	Post-2011	7
Tanzania	July-03	January-06	18
Tanzania	August-06	December-06	18
Tanzania	January-08	October-10	18
Tanzania	May-11	October-11	18
Zambia	Pre-2002	July-03	7
Zambia	March-05	July-06	7
Zambia	May-08	July-09	7

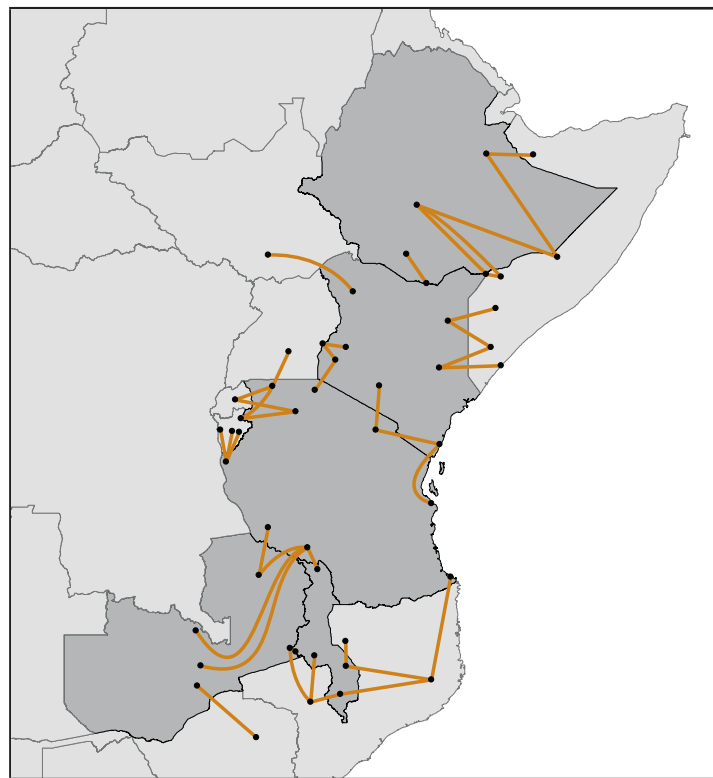


Fig. 1. Map of 49 markets and 40 market pairs in dataset.

and Mozambique) and of Industry, Trade, and Marketing (Tanzania) in these countries.

The median market town has a population of 178,000, and the median market pair is separated by a road distance of 345 km. All prices are expressed in US dollars per kilogram using monthly exchange rates provided by FEWS NET. The mean maize price across all markets and all periods is \$0.274/kg.² The price data from the 16 markets in Tanzania (9), Rwanda (1), Uganda (2), and southern Kenya (4) are labeled as wholesale prices while the price data from the remaining 33 markets are labeled as retail prices, although in practice there is often little distinction between wholesale and

retail markets in the region. I show later that my results are robust to excluding the 11 market pairs (between Tanzania and Burundi, Zambia, Malawi, and Mozambique) that involve a wholesale market and a retail market. The price series are not all complete as data collection began in some markets after January 2002 and there are a few missing observations throughout. The median price series has 102 of 120 possible observations, and 40 of the 49 markets have at least 6 years (72 observations) of data. Of 5880 possible price observations, 1435 (19%) are missing. I show later that my results are robust to restricting the panel to a more balanced subset.

Export bans are likely implemented during periods of high prices and are thus endogenous to prices. My main empirical specification estimates the effects of export bans on the *price gaps* between pairs of cross-border markets instead. Export bans are unlikely to be endogenous to price gaps, since the events that trigger them are unlikely to affect the costs of trade between the cross-border market pairs. Later in this section, I show that price gaps between pairs of cross-border markets are no different in the

² International maize prices in major world maize markets averaged \$0.15–0.16/kg during this period. The higher average prices in the countries of interest here reflect both the position of the region as a whole as a net maize importer (despite the fact that many individual countries and markets are net maize exporters) and the very high trade costs to and from the world market and between markets within the region (Porteous, 2017).

Table 2
Effects of export bans on price gaps.

	(1)	(2)	(3)	(4)	(5)
Export ban	0.0000214 (0.00959)	0.0000214 (0.00969)	−0.00139 (0.0100)	−0.0154 (0.0124)	0.00296 (0.00954)
Distance * Gas price			0.0000231 (0.0000158)		
Infrastructure			Yes		
Time trend				0.00128 (0.00110)	
Time fixed effects				Quarter	
Observations	3253	3253	3253	3253	3253
Standard errors	Dyadic	Cluster: pairs	Dyadic	Dyadic	Dyadic

Note: Directional market pair fixed effects included in all columns. Robust standard errors in () calculated as indicated.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

months leading up to export bans than in other non-ban periods. In the following section, I confirm with model simulations that in the absence of any bans price gaps would not have been higher or lower during periods in which bans were in fact implemented than in periods in which they were not.

In theory, export bans work by increasing the costs of trade between cross-border market pairs (to infinity if the ban is perfectly enforced). The spatial price analysis literature has thought carefully about the relation between price gaps, which are observable, and total trade costs, which are typically unobservable (Fackler and Goodwin, 2001). Under competitive trade, the price gap between a pair of markets is equivalent to the total trade costs between those markets if trade is occurring, which Baulch (1997) and others have called “regime 1.” If trade is not occurring, the markets are in a segmented equilibrium (Baulch’s “regime 2”), and the price gap between them is a lower bound on the total trade costs. The price gap may also temporarily exceed the trade costs if the markets are in disequilibrium following a shock (Baulch’s “regime 3”). Regimes 1 and 3 motivate a regression of the form:

$$\Delta P_{ijt} = \beta B_{it} + \phi_{ij} + \epsilon_{ijt} \quad (1)$$

In Eq. (1), $\Delta P_{ijt} = P_{jt} - P_{it}$ is the price gap between market j and market i in month t , B_{it} is an indicator variable for an export ban affecting exports from market i in month t , ϕ_{ij} is a directional market pair fixed effect capturing components of price differences that do not vary over time (e.g. baseline trade costs), and ϵ_{ijt} is a mean-zero error term reflecting the possibility of a shock that would cause price gaps to be greater or less than trade costs during a particular month t . Each pair of markets m and n has two possible directional market pairs: mn or nm . In estimating Eq. (1), I assign each time period’s price gap for a given pair of markets to the directional market pair for which the price gap is positive, letting the lower priced market be the origin market (i) and the higher priced market be the destination market (j). This means that the effect I estimate is based on comparing the price gaps during ban and non-ban periods using only those periods that have gaps consistent with trade in the direction affected by the ban.

For markets with relatively low trade costs and consistent import-export relationships, restricting attention to regimes 1 and 3 would be appropriate. However, given the high trade costs in the agricultural sector in sub-Saharan Africa (Porteous, 2017) and the fact that maize is produced locally in all of the markets in my dataset, it is important to account for the possibility of regime 2 segmented equilibria in which export bans would have no effect because they are not binding. If I include these “no-trade” observations in my estimation, the resulting estimate of my parameter of interest β is a valid measure of the effects of export bans in a reduced-form sense conditional on market

conditions at the time of ban implementation but is a downwardly-biased estimate of the effects of export bans conditional on the ban actually binding and preventing trade that would otherwise have occurred. Recent empirical evidence in contexts where regime 2 observations can be identified confirms this downward bias when all observations are included (Atkin and Donaldson, 2015). In the regressions that follow, I experiment with different ways of identifying and excluding potential regime 2 no-trade observations and compare my subsequent results to my baseline reduced-form result using all observations.

Column 1 of Table 2 shows results from the specification given in Eq. (1) with all observations. The mean of the dependent variable (the gap in prices between cross-border market pairs) is \$0.0853/kg.³ The point estimate for the effect of export bans on this gap is less than three-thousandths of a US cent or less than three-hundredths of a percent of the mean price gap and is not statistically significantly different from zero at any confidence level. I calculate standard errors directly because of the complicated nature of potential correlation between the residuals in my dataset. The standard approach with panel data (shown in column 2) would be to cluster at the market pair level to allow for correlation of residuals for a given market pair in different time periods. However, as is clear from the map in Fig. 1, the market pair structure also has features of a dyadic regression, with a single market often being a member of multiple market pairs. To deal with this additional source of correlation, I extend the approach of Fafchamps and Gubert (2007) for calculating consistent standard errors in cross-sectional dyadic regressions, allowing for correlation of residuals between any observations sharing at least one common market (even if those observations are in different time periods) while continuing to assume that residuals are independent across observations with no common markets. The standard errors calculated using this dyadic approach (column 1) are very close to those obtained by clustering at the market pair level (column 2).

Using the standard errors from column 1 and the mean maize price of \$0.274/kg, I can reject an alternate hypothesis that export bans have an effect at least as large as that of a 5% export tax ($0.05 * 0.0137/\text{kg}$) at an 8% significance level. A 5% export tax is at the low end of short-term trade policy responses to commodity market price fluctuations—temporary export taxes of 25–40% are not uncommon (Sharma, 2011). Of course, such taxes may (like export bans) not translate into empirical price differences, so the benchmark used here should be interpreted as the theoretical effect of a permanent 5% export tax.

³ Panel variance decomposition shows that 62.6% of the variation in this variable comes from within directional market pairs and the remaining 37.4% from between directional market pairs.

The specification in Eq. (1) implicitly assumes that no other variables besides export bans systematically affect price gaps over time. In columns 3 and 4 of Table 2, I introduce additional covariates to capture some of this potential temporal variation. Recent results from Dillon and Barrett (2016) highlight the importance of fuel prices for maize trade in East and Southern Africa. I construct monthly retail diesel price series in US dollars per liter at the national level for my 12 countries of interest (plus the break-away republic of Somaliland) by using biennial observations from the International Fuel Prices project of GTZ (the German technical cooperation) to compute markups over the Dubai Fateh crude oil index (the most relevant for oil imports into East and Southern Africa) and filling in gaps between GTZ observations using markups inferred by linear interpolation. In column 3, I add a term interacting these fuel prices in the origin market with the distance to the destination market as well as a set of indicator variables for major infrastructure projects affecting particular cross-border links compiled from government ministries and local newspaper archives. The point estimate for the diesel-distance coefficient corresponds to the expected cost of a 10-metric ton truck consuming 23 liters per 100 km (11 miles per gallon), although it is not statistically significant at conventional levels. In column 4, I include quarterly time fixed effects and a time trend instead. In both of these new specifications, the coefficient estimate on export bans is negative and not statistically different from zero. Similar results were obtained using monthly and annual fixed effects with and without a time trend as well as including all variables from both columns 3 and 4.

In column 5 of Table 2, I use a one-month lag of the export ban variable ($B_{i,t-1}$ instead of B_{it}) in case export ban implementation and lifting occurs with some delay. The point estimate for the effect of export bans remains close to zero and statistically insignificant. Similar results were obtained using lags of two, three, or four months.

Table 3
Price gaps prior to bans.

Export ban	−0.00025 (0.0117)
1 month prior	0.00621 (0.0138)
2 months prior	−0.00824 (0.0117)
3 months prior	−0.00228 (0.0111)
4 months prior	0.000834 (0.0103)
Observations	3253

Note: Directional market pair fixed effects included.
Robust dyadic standard errors in ().

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

In Table 3, I provide some initial evidence confirming that export bans are exogenous to price gaps by rerunning my preferred specification from column 1 of Table 2 with additional indicator variables for each of the four months immediately prior to export bans. The fact that all of the coefficient estimates for these indicator variables are close to zero and statistically insignificant suggests that export bans are not implemented in response to abnormally large or small price gaps between pairs of cross-border markets.

In Table 4, I modify my dataset in different ways to check for robustness. In column 1, I exclude two outliers: the pairs involving Juba, South Sudan and Hargeisa, Somaliland, which have the highest and most volatile prices of the markets in my dataset. Their exclusion does not affect my results but does reduce the standard error on my export ban coefficient estimate. This enables me to reject my alternate hypothesis that the effect of export bans is as large as the theoretical effect of a 5% export tax at a 3% significance level. In column 2, I explore whether the unbalancedness of the panel is affecting my results by excluding all observations before January 2006, reducing my dataset from ten years to six. With this adjustment, of the 3528 possible price observations in my new panel, only 171 (4.8%) are missing, as opposed to 19% in my original panel. In column 3, I exclude the 11 market pairs connecting a wholesale market in Tanzania to a retail market in another country. The remaining 29 pairs in my dataset all involve either two wholesale markets or two retail markets, with any systematic differences between these two pair types accounted for by the market pair fixed effects. My basic result that export bans do not have a statistically significant effect on the price gaps between pairs of affected cross-border markets remains unchanged.

In column 4 of Table 4, I include negative price gaps so that each observation occurs twice (once for directional market pair ij and once for ji). I expect export bans to have no effect in periods with negative price gaps as they should not be binding. My coefficient estimate is now negative and statistically significant, but it returns to a near-zero, insignificant level when pairs with origin markets in Kenya are dropped in column 5. The Horn of Africa famine occurred during Kenya's only export ban, resulting in very high prices of maize in some regions of Kenya that already had negative price gaps with their trading partners, which explains the correlation with a decreased (more negative) price gap despite the fact that the export ban was almost certainly not binding in these regions.

In a further set of robustness checks not presented here, I interacted implementing country indicator variables with the export ban indicator variable in my initial dataset of positive price gaps to look at potential heterogeneous effects. None of the coefficients on these country indicator variables were statistically different from zero. This indicates that none of the countries' export bans had a statistically significant effect on the price gaps between pairs of affected cross-border markets, despite observed or unobserved differences in timing, duration, objectives, and enforcement across countries.

I next consider the possibility that regime 2, segmented equilibrium observations are biasing my coefficient estimate towards

Table 4
Robustness checks.

	(1)	(2)	(3)	(4)	(5)
Export ban	0.00311 (0.00559)	−0.00619 (0.0114)	−0.000866 (0.0156)	−0.0290** (0.0140)	−0.00383 (0.00845)
Observations	3096	2579	2380	6506	5293

Note: Directional market pair fixed effects included in all columns. Robust dyadic standard errors in ().

* significant at 10%.

** Significant at 5%.

*** at 1%.

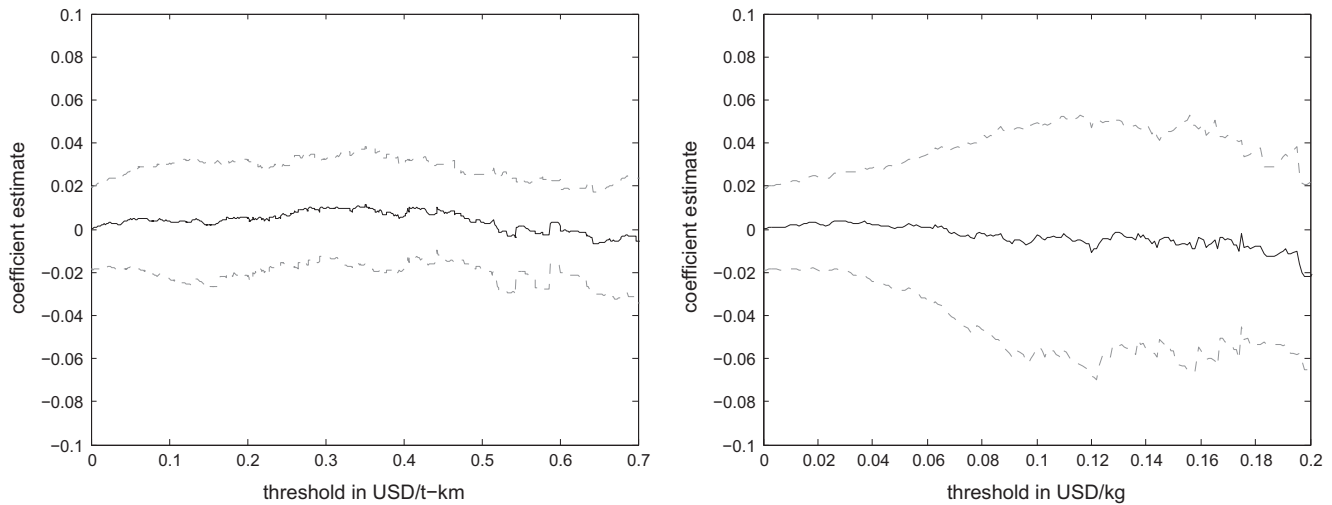


Fig. 2. Estimate of export ban coefficient (solid black) and bounds of 95% confidence interval (dashed gray) with increasing threshold in USD/t-km (left) and USD/kg (right).

zero. Suppose that export bans do increase price gaps significantly for market pair-periods where they prevent trade from occurring but that I am not detecting this increase because I am including many other observations of segmented equilibria where trade would not occur with or without an export ban. In this case, if I were to progressively drop increasing numbers of these regime 2 observations from my dataset my coefficient estimate should increase.

In Fig. 2, I experiment with two different ways of identifying and excluding potential regime 2 observations. [Teravaninthorn and Raballand \(2009\)](#) present data on transport prices for several major African transport corridors that range from \$0.07 to \$0.13 per metric ton-kilometer. [Porteous \(2017\)](#) finds that total trade costs are roughly double these baseline freight rates and are higher off of major corridors, with a median trade cost of \$0.29/t-km. Among the cross-border market pairs considered in this article, the maximum per-distance trade cost estimated by [Porteous \(2017\)](#) is \$0.70/t-km, and the maximum absolute trade cost is \$0.20/kg. Since trade costs are an upper bound on price gaps in regime 2, I proceed by dropping all observations in my dataset with price gaps below a progressively increasing threshold. In the left panel of Fig. 2, I use the per-distance price gap and progressively drop observations from 0 up to \$0.70/t-km. In the right panel, I use the absolute price gap and progressively drop observations from 0 up to \$0.20/kg. At the maximum threshold, only 354 (10.9%) and 297 (9.1%) observations remain in the dataset. In both cases, the point estimate stays statistically insignificant and very close to zero, and there is no sign of an upward trend as I drop increasing numbers of potential regime 2 observations. I conclude that my failure to detect an effect of export bans on cross-border price gaps is not due to the presence of regime 2 observations.

3. Comparison to model simulations

In this section, I run simulations using an estimated dynamic monthly model of grain storage and trade in sub-Saharan Africa ([Porteous, 2017](#)) to help understand the surprising empirical result from the previous section. The model of [Porteous \(2017\)](#), which is based on a long tradition of spatial temporal models of competitive storage and trade in the agricultural economics literature (including [Takayama and Judge, 1964](#); [Williams and Wright, 1991](#); [Gouel and Jean, 2015](#)), consists of a representative consumer and a representative competitive trader in each of 230 large hub markets covering all 42 countries of continental sub-Saharan Africa. The model

includes maize and five other major staple grains, and its demand, storage cost, and trade cost parameters were estimated using local production, consumption, and price data from May 2003 to April 2013. Demand is assumed to have a constant price elasticity for a grain composite and a constant elasticity of substitution between grains.⁴ Demand share parameters for individual grains and per capita demand shifters are estimated at the country level. Per-unit additive monthly storage costs and monthly interest rates are estimated at the regional level. Per-unit additive trade costs are estimated for each of 413 overland bilateral transportation links as well as to and from the world market through 30 major ports.

Given these estimated parameters along with current and expected future production and world prices, traders decide each month how much of each grain to sell locally, to put into storage in each of the 230 locations, and to trade along each of the 413 transportation links as well as with the world market. In the baseline version of the [Porteous \(2017\)](#) model that I use here, current production is taken as given (based on harvest data for the period), so production itself does not change in any of the simulations I do below. This is an appropriate assumption given the nature of export bans in the region, which are typically intended as short-term policies specific to a harvest year, even if they are then sometimes extended to cover additional harvest years. Traders, who are assumed to have complete information about current market conditions, make storage decisions under the expectation that future harvests will equal a linear prediction using the previous 10 harvests and that future world prices will equal current world prices. These expectations are then updated when new harvests and world prices are observed.

[Porteous \(2017\)](#) shows that the equilibrium price series and trade flows from the estimated model are highly correlated with observed market-level price data and national trade data. That said, the model cannot reproduce the data exactly due to the many local demand, information, and cost shocks that are unobserved. To the extent that such unobserved shocks are uncorrelated with export bans, the model simulations can provide helpful evidence on how price gaps (and prices themselves) should have been affected by export bans. The two main explanations for any discrepancies between the predicted model outcomes and the actual

⁴ [Porteous \(2017\)](#) uses the price elasticity of -0.066 estimated by [Roberts and Schlenker \(2013\)](#) and an elasticity of substitution of 1, which are within the 95% confidence intervals of his instrumental variables estimates with the landed world price as an instrument. These are the elasticities used for the simulations here.

observed outcomes are (i) differences in the way export bans are implemented in the model and in reality and (ii) the correlation or interaction of these unobserved shocks and export bans.

I use the model to run three simulations for the 10-year time period of the empirical exercise in the previous section. In all three simulations, current and expected future production and world prices are the same as in the baseline model, but equilibrium local prices change in response to current or expected future changes in the trade cost parameters due to export bans. In the first simulation, I assume that export bans are not implemented so that trade is possible between cross-border market pairs during every month at the pair-specific trade costs from the original estimated model. In the second simulation, I assume that the 13 export bans from Table 1 are implemented and perfectly enforced and that traders are naïve, so the imposition and lifting of the bans takes traders by surprise. This means that prior to bans storage and trade decisions are made assuming that trade will always be possible at the pair-specific trade costs, and during bans these decisions are made assuming that trade will never again be possible between the affected pairs (trade costs are infinite). In the third simulation, I assume that the bans are implemented and perfectly enforced but that traders have perfect foresight about the imposition and lifting of bans. This gives them the possibility of exporting prematurely before bans are imposed and storing for future exports during bans. Realistically, trader behavior is likely somewhere in between the second and third simulations, given that precise information about future discretionary government actions is not available but that some anticipation is certainly possible.

After solving month by month for the full continent-wide equilibrium for each of the three simulations, I extract the price series for the 47 markets and 33 market pairs corresponding most closely to the 49 markets and 40 market pairs from the dataset used in the previous section. Fig. 3 highlights these market pairs against the backdrop of the other markets and transportation links in the

continent-wide model. I then run the same regression from Eq. (1) using the price gaps for these market pairs from each of the model simulations (Table 5). The results reported here do not change significantly when all affected cross-border pairs from the model (including those with Namibia, D.R. Congo, Eritrea, Sudan, and the world market) are included.

The results in Table 5 are helpful for distinguishing between different explanations for my finding in the previous section that export bans do not have a statistically significant effect on the price gaps between cross-border markets. One possible explanation is that export bans do increase price gaps but are implemented during periods of abnormally small price gaps. This endogeneity due to omitted variables or reverse causality could bias my initial coefficient estimate towards zero. I can rule out this explanation using the first simulation, which shows that in the absence of export bans, the difference between the cross-border price gaps during periods when export bans were and were not actually implemented would not have been significantly different from zero. These results complement my empirical findings in Table 3, which showed no significant difference between the price gaps in the months just before export ban implementation and those in other non-ban periods.

A second explanation is that export bans are not binding or that the trade flows they do prevent are so small that the bans do not have a significant effect on price gaps. I can rule out this explanation using the second simulation, which shows a very large effect of export bans on cross-border price gaps (significant at the 1% level) when traders do not anticipate ban imposition and lifting. The size of the effect is nearly four times larger than the average price gap of \$0.0853/kg.

A third possible explanation is that since maize is storable and bans are temporary, traders are able to limit the actual effects of export bans when they can anticipate their imposition and lifting. The third simulation shows that perfect foresight would enable

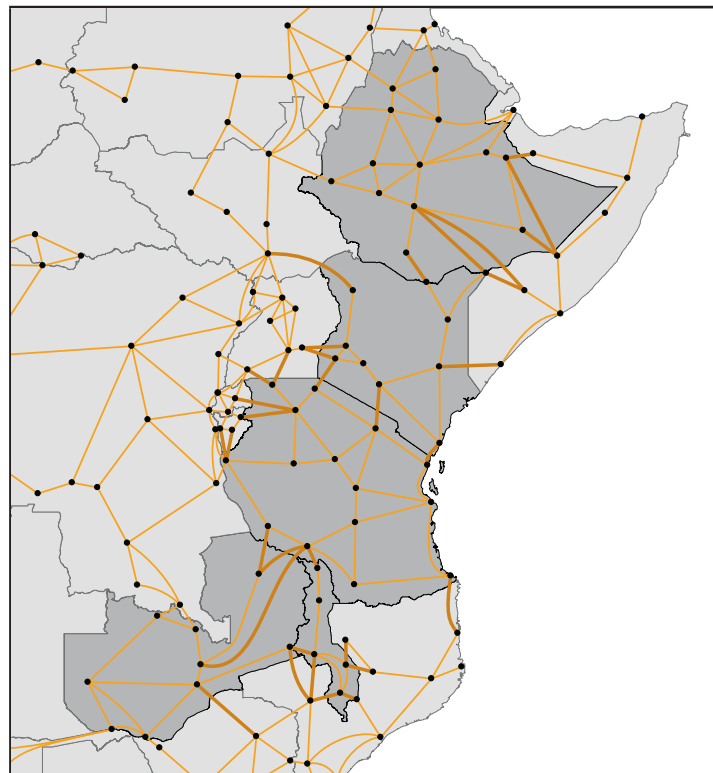


Fig. 3. Map of 33 selected market pairs (darker lines) from Porteous (2017) model.

traders to cut the effect of export bans on cross-border price gaps nearly in half, but the effect is still large and statistically significant at the 1% level. Interviews with traders in the region confirm that high storage costs (including the high cost of capital) make it costly for them to hold on to stocks while waiting for a ban to be lifted.

My coefficient estimate using the actual price data in Table 2 is well within the 95% confidence interval of my estimate from the model simulation without export ban implementation in the first column of Table 5. This means that I cannot reject a hypothesis that export bans are simply not enforced. To further investigate this hypothesis, I run additional regressions looking at the effects of export bans on prices. These regressions are of the form:

$$P_{it} = \beta B_{it} + \phi_i + \epsilon_{it} \quad (2)$$

for origin markets in export ban implementing countries and

$$P_{jt} = \beta B_{jt} + \phi_j + \epsilon_{jt} \quad (3)$$

for destination markets in trading partner countries, where ϕ_i and ϕ_j are fixed effects for origin and destination markets respectively. I continue to restrict the data to the direction of the positive price gap so that a given market pair has a single origin market and destination market each period. I run these two regressions on the price data as well as the price series from each of the three model simulations (Table 6).

A concern with the specifications in Eqs. 2 and 3 discussed previously is that export bans are likely endogenous to prices as they are ostensibly implemented during periods of high prices. The model simulation with no bans helps me to control for this endogeneity. Results in Table 6 confirm that in the absence of export bans, prices in both origin and destination markets would have been 2 US cents/kg higher during periods when bans were in fact in place than in periods when they were not. However, in the data, prices in both origin and destination markets are 6 US cents/kg higher during export ban periods. The net effect of export bans thus appears to be a 4 US cents/kg increase in prices in both origin

and destination markets. This effect is statistically significant at the 1% level for origin markets and at the 5% level for destination markets, since the standard errors from Table 6 imply non-overlapping confidence intervals for the coefficient estimates from the data and the “no bans” simulation at these levels.

These results suggest that export bans are having some effects on market outcomes after all, but there are two caveats. First, although the model captures the key factors that lead governments to implement export bans (local production shortfalls and high international prices), I cannot completely rule out the possibility that an unobserved variable not captured by the model (e.g. a local demand shock) causes higher prices and triggers export bans that are then not enforced. Second, the effects in the data are still very different from those in the model simulations in which the bans are fully enforced with either naïve traders or traders with perfect foresight. In both of these simulations, destination market prices increase substantially during bans while origin prices fall (naïve) or remain statistically unchanged (perfect foresight), effects which are in line with theory.

In the following section, I present information collected from market participants in the region about ban enforcement that helps explain my findings.

4. Imperfect enforcement and destabilizing stabilization

As part of the research for this article, I obtained information about ban enforcement from market participants in Malawi, Tanzania, and Zambia, which together are responsible for 10 of the 13 export bans in my dataset. I conducted interviews with formal and informal private traders of all sizes, trader associations, farmers, government officials, and market observers including FEWS NET and the World Food Programme in these countries. I also visited six border points in the region during export bans.

The consensus among market participants is that export bans are implemented but imperfectly enforced. The formal export of maize requires an export permit, typically issued by the Ministry of Agriculture. When an export ban is imposed, permits are no longer issued. The bans shut down most of the formal maize trade, but maize continues to cross borders. Some formal traders (particularly those with the right political connections) are able to obtain export permits during bans through back-door channels. Informal traders, who may not be eligible for or choose to obtain export permits even during non-ban periods, are also able to continue moving maize across borders during bans. At official border points, informal traders often use bicycles, which are not regulated, to move maize between trucks on either side of the border. Informal traders also use unofficial border crossings along the long, porous land borders between countries in the region.

Table 5
Results using simulated price series.

	No bans	Naïve	Foresight
Export ban	−0.00359 (0.00287)	0.301*** (0.0726)	0.178*** (0.0408)
Observations	2938	2958	2934

Note: Directional market pair fixed effects included in all columns. Robust dyadic standard errors in ().

* significant at 10%.

** significant at 5%.

*** Significant at 1%.

Table 6
Price regressions with data and model simulations.

	Data	Data	No bans	No bans
Dependent variable	Orig. price	Dest. price	Orig. price	Dest. price
Export ban	0.0624*** (0.00887)	0.0574*** (0.0117)	0.0220*** (0.00444)	0.0183*** (0.00467)
Observations	3253	3253	2938	2938
	Naïve	Naïve	Foresight	Foresight
Dependent variable	Orig. price	Dest. price	Orig. price	Dest. price
Export ban	−0.0438*** (0.0130)	0.256*** (0.0509)	−0.0104 (0.0682)	0.167*** (0.0312)
Observations	2958	2958	2934	2934

Note: Market fixed effects included in all columns. Robust standard errors in () clustered by market.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Table 7
Effect on price gaps by ban quarter.

1st quarter of ban	–0.00485 (0.00667)
2nd quarter of ban	0.011831 (0.00959)
3rd quarter of ban	0.0108 (0.0163)
4th quarter of ban	0.0182** (0.00774)
Observations	3159

Note: Bans without start-to-finish data excluded. Directional market pair fixed effects included. Robust dyadic standard errors in ().

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

The total volume of maize that can be transported across affected borders during bans may be subject to capacity constraints. At and around official border points, market participants report that enforcement is positively correlated with volume, with border officials generally tolerating low volumes of informal trade during bans but initiating patrols and crackdowns when volumes increase. At one border point in Malawi, FEWS NET monitors estimate the volume of informal maize trade tolerated by officials to be 200 metric tons per month. This compares to formal export volumes that have occasionally been as high as 10,000 metric tons or more through this border point in non-ban months. At an unofficial border crossing I visited elsewhere, a single dugout canoe with a capacity of 0.7 metric tons is available to ferry maize between trucks across a river border during bans, allowing for the transport of approximately 1000 metric tons per month.

Theoretically, if capacity constraints are binding during bans, the price gaps between pairs of affected cross-border markets should increase, which I do not observe in the data. However, anecdotal evidence suggests that bans are often lifted in response to complaints from farmers and traders about a lack of trading opportunities following bumper harvests, precisely the time when capacity constraints may start to bind. To assess this possibility, I divide the nine export bans for which I have start-to-finish data into quarters, drop observations from the other four bans, and redo my main specification with separate indicator variables for the first, second, third, and fourth quarters of export bans. Results in Table 7 indicate that while price gaps are unchanged in the first three quarters of bans, they do exhibit a small but statistically significant increase in the final quarter of bans. This suggests that capacity constraints on informal cross-border trade generally only bind at the very end of bans, when large new harvests may make governments amenable to lifting the bans anyway.

Of potentially greater concern is that market participants report that the climate of uncertainty created by discretionary export bans with *ad hoc* enforcement destabilizes markets. Faced with uncertain fluctuations in bans, permit issuing, and enforcement, both formal and informal traders engage less in long-term storage for future trade, in contractual agreements with cross-border purchasers (particularly those with long-term delivery commitments), and in long-distance trade across far-off borders where they have fewer connections and less local knowledge about informal channels. Instead, they prioritize short-term, non-contractual, local transactions. This potentially weakens the capacity of markets to respond efficiently to the harvest shortfalls or price increases that characterize export ban periods.

To see whether these destabilizing effects show up in the data, I compute the standard deviation of prices during export ban and non-export ban periods for each origin and destination market. I

Table 8
Standard deviation regressions with data and model simulations.

	Data	Data	No bans	No bans
Dependent variable	Orig. SD	Dest. SD	Orig. SD	Dest. SD
Export ban	0.0238*** (0.00833)	0.0198 (0.0162)	0.00573 (0.00971)	–0.00189 (0.00499)
Observations	78	82	52	64
Mean SD (no bans)	0.0662	0.0873	0.0648	0.0682
	Naïve	Naïve	Foresight	Foresight
Dependent variable	Orig. SD	Dest. SD	Orig. SD	Dest. SD
Export ban	0.00819 (0.00796)	0.0661** (0.0296)	0.00936 (0.00655)	0.0693** (0.0299)
Observations	53	65	53	64
Mean SD (no bans)	0.0638	0.0654	0.0627	0.0676

Note: Market fixed effects included in all columns. Robust standard errors in () clustered by market.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

then regress the standard deviations for each type of market on my ban indicator variable and market fixed effects (Table 8). In the data, the standard deviation of prices for origin markets is 36% higher during export bans than its average in non-ban periods (statistically significant at the 1% level). This does not appear to be due to endogeneity issues as there is no significant difference in standard deviation between ban and non-ban periods in my model simulation without ban implementation. The coefficient estimate for destination markets is also much larger in the data (23% of its non-ban average) than in the “no bans” simulation, but it is not statistically significant at conventional levels and is smaller than the coefficient estimate for model simulations when bans are implemented and fully enforced.⁵

In addition to increasing uncertainty, export bans also send a signal to all market participants that the implementing government is concerned about upcoming shortfalls or high prices of maize. In an environment where small traders and individual consumers are not always able to obtain and aggregate market information completely, an export ban may lead market participants to revise upward their expected prices for the months ahead, especially if they know the ban will be enforced imperfectly and not keep prices in check. Higher expected prices then push up current prices as storage becomes more attractive. Accounts of “panic buying” and “hoarding” in the implementing country itself are not uncommon during the region's export bans. These dynamics help explain why export bans appear to be increasing prices in both origin and destination markets beyond what they would be in the absence of a ban.

Taken together, my results suggest that export bans are having very different effects than those intended by implementing-country governments in East and Southern Africa. Rather than cutting off trade, export bans divert it to the informal sector. Rather than widening price gaps, export bans do not affect them. Rather than maintaining or lowering domestic prices and domestic price volatility, export bans appear to increase both. Export bans may thus be contributing to high and volatile domestic maize prices in a cycle that makes governments all the more inclined to implement them.

⁵ Results using the coefficient of variation (the standard deviation normalized by the mean) are similar. The coefficient of variation is 10 mean percentage points higher during export bans for origin markets (significant at the 1% level) and 7 mean percentage points higher for destination markets (significant only at the 15% level), whereas it is 1 mean percentage point higher for origin markets and 1 percentage point lower for destination markets in the “no bans” simulation, neither of which are statistically significant.

5. Conclusions and policy implications

I have used monthly data on maize prices from 40 pairs of cross-border markets to investigate the empirical effects of 13 short-term export bans implemented by 5 countries in East and Southern Africa over a ten-year time period. My initial estimation yielded the surprising result that export bans do not have a statistically significant effect on cross-border price gaps. This result is robust to a variety of alternative specifications and modifications of the dataset, including the elimination of potential segmented equilibria in which the bans might not be binding. I am also able to reject a hypothesis that the effect of export bans on price gaps is at least as large as the theoretical effect of a 5% export tax.

I compared my empirical results to results from running the same regressions on price series obtained from three simulations using an estimated dynamic monthly model of grain storage and trade in sub-Saharan Africa (Porteous, 2017). The simulations enabled me to rule out several potential explanations for my surprising result, including that bans are implemented in periods of abnormally low price gaps, that bans are not binding, and that bans are ineffective due to trader anticipation. My results on price gaps are consistent with a model simulation in which bans are not implemented, but actual prices in both implementing country and trading partner markets are significantly higher during bans than in this simulation, as is price volatility in implementing country markets.

Information collected from market participants and visits to affected border points indicates that export bans in the region are imperfectly enforced. Export bans divert trade into the informal sector, which appears to be able to move enough maize across borders to keep price gaps from widening until market conditions change and the implementing government is ready to lift the ban. However, the increased prices and volatility during export ban periods compared to the model simulation with no bans suggest that the bans are destabilizing markets by pushing traders into short-term, non-contractual, local transactions and by signaling future price increases without actually preventing them.

While it is already widely accepted that export bans are disruptive for trading partner countries, they can theoretically be justified by the countries that implement them, particularly those that weight consumers' welfare more than that of producers. My results, however, suggest that they may have unexpected effects. Instead of stabilizing and lowering domestic prices, export bans in East and Southern Africa appear to destabilize markets, leading to increases in both domestic prices and domestic price volatility. Governments in the region should therefore reconsider their use of these policies even when they seem justified on political economy grounds.

My findings on export bans complement those of other studies on the effects of discretionary price stabilization policies in the region. Anderson and Nelgen (2012) find domestic agricultural price volatility greater than border price volatility in Africa, which they interpret as evidence that stabilization policies may in fact be destabilizing domestic markets. Jayne (2012) presents evidence that the countries in East and Southern Africa that have been the most aggressive in implementing maize price stabilization policies like export bans have experienced the greatest maize price volatility. Jayne et al. (2006) and Tschirley and Jayne (2010) document how recent food crises in the region were exacerbated by *ad hoc* government interventions that interfered with the private sector's response.

Although many of my findings appear to depend on the institutional and geographic details of East and Southern Africa, these features are not unique to countries in the region. Many of the countries that implemented export restrictions during the 2007–

2008 food price spike and its aftermath were developing countries with relatively weak institutions. Although countries like India that are mostly surrounded by water may find it easier to enforce trade policies, others like Ukraine have long and relatively porous land borders, and the active smuggling of rice from Indonesia to the Philippines suggests that even island countries are not immune to informal trade circumventing trade barriers. Moreover, chronologies of trade policies used during this period reveal how unpredictable and *ad hoc* they were in many countries (Sharma, 2011; Headey, 2011). The results presented here highlight how these types of discretionary stabilization policies can end up being destabilizing—even for the implementing countries themselves.

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