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 response to fluctuations in prices of agricultural commodities. I use data on maize prices and trade policies in 12 countries in East and Southern Africa to investigate the effects of short-term export bans on agricultural markets. I fail to reject my null hypothesis that export bans have no effect on price differences between markets and reject a hypothesis that export bans have an effect on price differences at least as large as the theoretical effect of a 5% export tax. I evaluate alternative explanations of this surprising result and develop a qualitative description of what occurs during an export ban that is consistent with a rational expectations storage model. My findings suggest that short-term export bans may in fact increase prices and volatility in the implementing country. The use of temporary export restrictions as price stabilization policies may therefore need to be re-evaluated.

1 Introduction

Dramatic fluctuations in the prices of basic agricultural commodities in recent years have led to renewed interest in the functioning of these markets and the policy instruments that can be used to influence them. In developing countries, where food expenditure makes up a large proportion of household consumption, policies to control or stabilize food prices have proliferated. Temporary export restrictions have been particularly widespread, with at least 33 countries using some form of export restriction since 2006, including all 5 of the top 5 rice producers (China, India, Indonesia, Bangladesh, Vietnam) and 7 of the top 13 wheat producers (China, India, Russia, Pakistan, Ukraine, Argentina, Kazakhstan) (Sharma 2011). This paper focuses on the most common and severe of such restrictions: the short-term export ban.

Temporary export restrictions are widely used, highly controversial, and still not fully understood. Static partial equilibrium analysis suggests that they introduce welfare-reducing price distortions, denying

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local farmers the opportunity to benefit from high prices and the incentive to invest in increasing production (Mitra and Josling 2009). On the other hand, a dynamic model developed by Gouel and Jean (2012) shows that export restrictions could be part of an optimal food price stabilization policy, helping explain their widespread use. Welfare effects on other countries are less ambiguous: 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 2010; Martin and Anderson 2011). Several authors have called for limitations on the use of export restrictions to be included in international trade agreements. Negotiations for such limitations would only be possible if the reasons why these policies are implemented and the effects that they actually have are better understood.

In this paper, I evaluate the actual empirical effects of export bans by addressing the question, *What effect do short-term export bans have on the price differences between markets?* Previous empirical work on export bans has focused on the effects of individual bans on prices and struggled with identification issues in making counterfactual claims about what prices would have been if the ban had not been implemented. In contrast, I look at a context - maize markets in East and Southern Africa - where export bans have been used frequently and repeatedly by multiple countries, and my focus on price differences rather than prices themselves enables me to shed light on the mechanics of how these policies actually affect agricultural markets while avoiding potential endogeneity issues. Drawing on the spatial price analysis literature, I develop a simple structural model to show how export bans affect total trade costs (which are unobservable), and how total trade costs then determine the actual price differences between markets (which I observe). I argue that the timing of export bans is plausibly exogenous to price differences and back up this intuition with regression results that show no significant change in price differences in the months leading up to export ban implementation.

Empirical estimation of my model using 10 years of panel data from 79 markets (139 first-order market pairs) in 12 countries leads me to fail to reject my null hypothesis that export bans have no effect on price differences. This result is robust to a variety of alternative specifications, including dropping potential non-traded equilibria and adding second-order market pairs. I am also able to reject an alternate hypothesis that export bans have an effect at least as large as the theoretical effect of a 5% export tax.

I explore a number of potential explanations for my surprising results. I first consider and rule out the possibility that the export bans in my sample are either not binding or not implemented. Instead, I find suggestive evidence that export bans lead to price increases in *both* origin and destination countries and that origin country prices continue to track destination country prices during bans. This is consistent with a dynamic storage model with rational expectations in a context where ban duration is short but uncertain. While origin country traders store maize in expectation of the ban's lifting, the government continues to prolong the ban. Eventually, a supply response (often coupled with other factors) leads to a sharp fall in price and a widening of price differences, prompting the ban's end. This qualitative description is broadly similar to the experience in Russia with export bans on wheat described by Welton (2011). My results suggest that export bans may have unintended consequences, increasing both domestic prices and domestic price volatility. Their use as price stabilization policies may consequently need to be re-evaluated.

Aside from the findings on export bans, this paper also makes several methodological contributions to the spatial price analysis literature. First, I show how the multi-way clustering approach of Cameron et al. (2011) can be used to resolve standard error correlation problems in dyadic regressions where origin and destination markets both appear in multiple pairs. Second, I suggest a simple new inductive technique for controlling for the possibility of segmented (non-traded) equilibria by dropping observations with price differences less than a minimum feasible transport cost. Finally, I provide a benchmark estimate of the effect of changes in fuel prices on the difference in agricultural prices between markets in East and Southern Africa. For a 1 dollar rise in the retail price per liter of diesel, I find that the price difference per 100 kilograms increases by 35.5 cents for every 100 kilometers between the origin and destination market.

The balance of this paper proceeds as follows. In Section 2, I describe the context and my dataset and show how price differences are correlated with borders, infrastructure quality, and fuel prices. Section 3 presents my structural model and derives my basic estimating equation. Section 4 contains the results of my basic specification and robustness checks, while section 5 explores alternative explanations of my results and develops a qualitative description of the dynamics of export bans. Section 6 concludes.

2 Context and Data

Maize is a staple food in 18 countries in East and Southern Africa with a combined population of 426 million. Governments in the region are frequently faced with fluctuations in maize prices, not only from volatility in global market prices for maize, but also from local production shortfalls due to droughts or other natural disasters, which occur every 3-5 years and are increasing in frequency due to climate change.

Two major short-term trade policies are regularly implemented in East and Southern Africa in response to these shocks - export bans and tariff waivers. Both of these policies work by affecting trade costs, the total costs involved in getting a product from one market to another. Trade costs in the region are significant *ex ante* given the poor infrastructure, large formal and informal taxes and tariffs, and high price of fuel. Trade costs create a wedge between the export parity price - the price traders get for selling their maize to another market - and the import parity price - the price traders pay for purchasing maize from another market. Theoretically, export bans work by increasing the trade costs for exports (to infinity if they are fully enforced), lowering the export parity price so as to prevent high external prices from passing through to domestic markets and scarce domestic maize from being shipped abroad. In contrast, tariff waivers work by decreasing the trade costs for imports, lowering the import parity price so as to allow cheap imports into the country to alleviate high prices and domestic shortages. Both export bans and tariff waivers are typically global - applying to all trading partner countries - although the effect of a tariff waiver depends on the pre-existing tariff, which may vary from partner country to partner country.

There is a long-standing literature on spatial price analysis in maize markets in East and Southern Africa (e.g. Goletti and Babu 1994, Van Campenhout 2007). Much of this research has focused on determining the extent to which prices in one market "cause" prices in another market and evaluating market integration using highly-parameterized models. Most papers restrict their attention to markets within a given country. Chapoto and Jayne (2009) provide some comparative reduced-form policy analysis across countries at a regional level (e.g. comparing maize price volatility in countries with hands-off versus interventionist policies towards maize markets). However, there has been little regional analysis of the mechanics of how specific trade policies actually affect markets.

My primary dataset consists of a panel of monthly maize price data collected by the Famine Early Warning System Network (FEWS NET) from 79 markets (cities/towns) in 12 countries in East and Southern Africa: Burundi, Ethiopia, Kenya, Malawi, Mozambique, Rwanda, Somalia, South Sudan, Tanzania, Uganda, Zambia, and Zimbabwe. The monthly price data cover 10 years, from January 2002 to December 2011 (T = 120). 52 of the 79 price series are retail maize prices while the remaining 27 are wholesale prices, an issue I address in my model. The panel is unbalanced as data collection began in many markets after January 2002 and there are a few missing observations throughout. 70 of the 79 markets have at least 6 years (72 months) of data. Of 9480 possible price observations, 1732 (18%) are missing. I will show that my results are robust to restricting the panel to a more balanced subset.

Since export bans work by increasing trade costs, which in turn affect the price differences between markets, my variable of interest is the price differences between market pairs rather than the prices

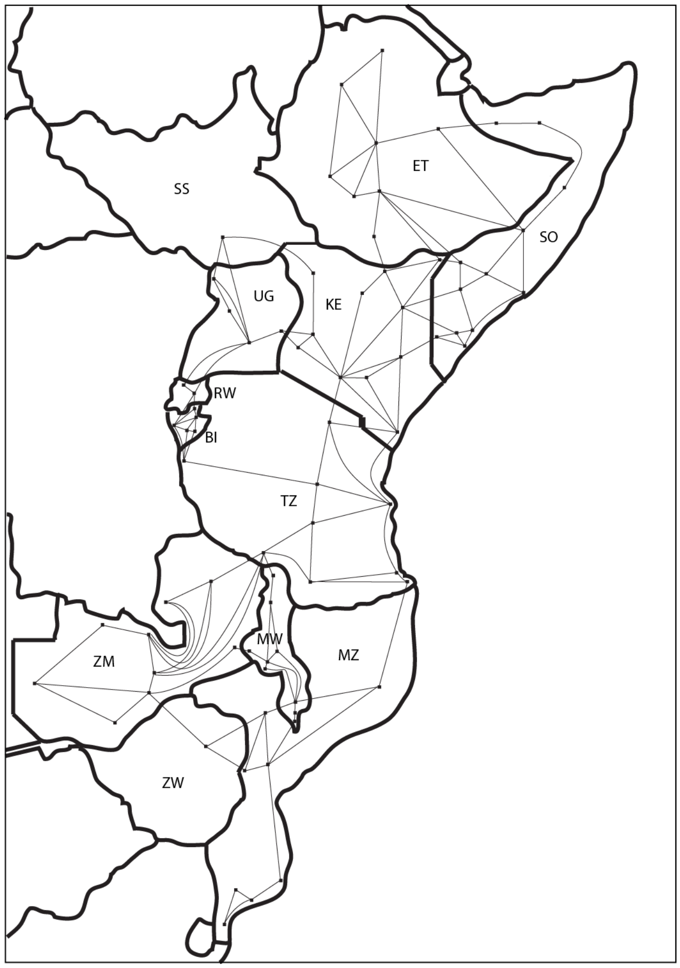

Figure 1: Map of markets and first-order market links

themselves. Trade in the region is almost exclusively by diesel truck and is constrained by geography and the limited road network. If four markets A, B, C, and D lie in that order along a road, I restrict my attention for my primary specification to market pairs AB, BC, and CD without considering a market pair like BD, for which I assume trade costs are the sum of trade costs for BC and CD . Thus I identify and focus on 139 pairs of adjacent markets for which direct trade is feasible (N=139). This restriction is realistic in the context, and I will show that my results are robust to considering second-order pairs (e.g. AC and BD). The distance between markets in first-order pairs ranges from 32 to 1026 kilometers, with a median distance of 318 kilometers. 37 first-order pairs span an international border, while 102 are pairs of domestic markets. A map of the 79 markets with the 139 first-order links is shown in figure 1.

Table 1 presents statistics on the average price of maize (USD/kg) for the 6 highest and lowest priced markets, the average price difference for the 6 links with the highest and lowest differences, and the mean price and price difference for all observations. The markets with the highest prices - Juba in newly-

Table 1: Summary Statistics for Prices and Price Differences

Rank	Market	Price	Rank	Market Link	Price Difference
1	Juba SS	0.779	1	Juba SS - Arua UG	0.525
2	Lodwar KE	0.446	2	Juba SS - Kampala UG	0.509
3	Burao SO	0.431	3	Juba SS - Lodwar KE	0.256
4	Hargeisa SO	0.407	4	Lodwar KE - Eldoret KE	0.225
5	Wajir KE	0.385	5	Wajir KE - Bardera SO	0.185
6	Mandera KE	0.383	6	Mandera KE - Shashemene ET	0.180
74	Mitundu MW	0.205	134	Nairobi KE - Mombasa KE	0.0227
75	Mchinji MW	0.203	135	Kitwe ZM - Solwezi ZM	0.0226
76	Tororo UG	0.196	136	Dar es Salaam TZ - Arusha TZ	0.0208
77	Songea TZ	0.179	137	Arusha TZ - Dodoma TZ	0.0205
78	Masindi UG	0.178	138	Mtwara TZ - Lindi TZ	0.0198
79	Iringa TZ	0.173	139	Dodoma TZ - Dar es Salaam TZ	0.0196
	Mean	**0.267**		**Mean**	**0.0729**
	Observations	**7748**		**Observations**	**12270**

independent South Sudan, Burao and Hargeisa in the breakaway republic of Somaliland, and 3 markets in northern and eastern Kenya - are from maize deficit areas isolated from other markets. The lowest-priced markets, in contrast, are from maize surplus areas in Tanzania, Uganda, and Malawi. Market pairs with the largest price differences are notable for the poor infrastructure, insecure areas, large distances, and borders that lie between them. Those with the smallest price differences are all domestic links connected by good roads. The mean price difference between market pairs is 7.3 US cents - more than 25% of the mean price of 26.7 cents - suggesting the presence of significant trade costs between markets.

To complement the FEWS NET price data, I have compiled a secondary dataset with monthly data for the same period on export bans and tariff rates, as well as fuel prices and infrastructure projects.

Using local newspaper archives, I have identified the starting and ending dates of 13 short-term export bans implemented by 5 countries, ranging in duration from 4 to 54 months and affecting 2046 link-months in my dataset (see table 2). Likewise, I have identified the dates of 5 tariff waivers implemented by 3 countries, ranging from 3 to 11 months in duration and affecting 342 link-months in my dataset[1]. Combining the tariff waiver dates with implementation dates of free trade agreements (the other main source of variation in regional tariff rates during the study period), base tariff rates on maize from country reports to the World Trade Organization, and tariff data obtained from other researchers, I have assembled a complete database of monthly tariff rates for the countries in my dataset[2].

For fuel prices, I construct capital city monthly retail diesel price series for the 12 countries plus the

[1]The 5 waivers were implemented by Kenya (Jun-04 to Aug-04, Feb-09 to Dec-09, and Jun-11 to Post-2011), Tanzania (Jan-08 to May-08), and Zambia (Sep-05 to Mar-06).

[2]No tariff information was available for Somalia and South Sudan - I make the assumption that their tariffs are constant over time, which is realistic given that weak governance in both countries likely precludes active tariff manipulation. World Trade Organization tariff data was obtained from the WTO Tariff Download Facility: http://tariffdata.wto.org/

Table 2: Dates and Duration of Export Bans

Country	Start Month	End Month	Months	Links	Link-Months
Ethiopia	Jan-06	Jul-10	54	6	324
Ethiopia	Mar-11	Post-2011	9[a]	6	54
Kenya	Oct-08	Post-2011	38[a]	12	456
Malawi	Jul-05	Feb-07	19	5	95
Malawi	Apr-08	Aug-09	16	5	80
Malawi	Dec-11	Post-2011	0[a]	5	0
Tanzania	Jul-03	Jan-06	30	11	330
Tanzania	Aug-06	Dec-06	4	11	44
Tanzania	Jan-08	Oct-10	33	11	363
Tanzania	May-11	Oct-11	5	11	55
Zambia	Pre-2002	Jul-03	19[a]	5	95
Zambia	Mar-05	Jul-06	16	5	80
Zambia	May-08	Jul-09	14	5	70
					2046

[a]overlaps with beginning or end of period.

breakaway republic of Somaliland by combining data from FEWS NET and the International Fuel Prices project of GTZ (the German technical cooperation). I use the GTZ data to compute markups over the Dubai Fateh crude oil index (the most relevant for oil imports into East and Southern Africa) and fill in gaps between observations using markups inferred by linear interpolation. I make the assumption that secondary markups between the capital city and other markets in the same country do not vary over time and proceed to use the capital city series for all markets in the country, with the constant secondary markups to be captured by my regression fixed effects[3]. The mean of the average retail diesel prices over the 10-year study period is 1.06 USD per liter. The lowest average retail diesel prices are in oil-producing South Sudan (0.65 USD/L) and stateless (and therefore taxless) Somalia (0.69 USD/L), while the highest prices are in landlocked Rwanda (1.47 USD/L), Malawi (1.44 USD/L), and Burundi (1.33 USD/L).

Finally, I have compiled a database of completion dates of 22 infrastructure projects affecting 19 marketing links (7 international and 12 domestic) between markets in 7 countries using information from government ministries, local newspaper archives, and United Nations reports. This includes 16 road upgrades (typically the paving of dirt roads), 2 bridge constructions (replacing boat crossings), 1 combined road upgrade / bridge construction, and 3 road deminings (in South Sudan).

Suggestive evidence indicates that maize price differences are indeed correlated with trade policies, fuel prices, and infrastructure quality. Table 3 compares the mean price differences for domestic and international market pairs and for links with different levels of baseline infrastructure quality (quality =

[3]To assess the plausibility of this assumption, I analysed 3 years of monthly diesel retail prices for 6 markets from Zambia in my dataset from the Energy Regulation Board of Zambia. While there is some variation in markups between the capital city Lusaka and the other 5 markets over time, the average variance in the markup is 0.0069 USD - only 4.1% of the average variance in the diesel retail price (0.155 USD) - so the effect of my assumption of constant markups should be minimal.

1 being a paved road; quality = 2 being an improved but unpaved road; quality = 3 being an unimproved dirt track). T-tests reject the null hypothesis of equality of price differences between categories at the 5% level. This suggests that something happening at the border (e.g. trade policy implementation) increases price differences and that improved infrastructure quality is inversely correlated with price differences.

Table 3: Price Differences by Link Characteristics

	Average Price Difference	Links
Border = 0	0.061	102
Border = 1	0.110	37
Quality = 1	0.056	67
Quality = 2	0.082	59
Quality = 3	0.154	13

Figure 2 looks just at domestic market links in those countries with more than one market in the dataset (all except South Sudan and Zimbabwe) and plots the average maize price difference per 100 kilometers for each country against the average retail diesel price in that country. There is a clear positive correlation between fuel prices and price differences. Countries above the trend line (Somalia, Kenya, Burundi) are those with relatively poorer infrastructure and higher insecurity. Countries below the trend line (Ethiopia, Zambia, Tanzania) are those with relatively better infrastructure and less insecurity.

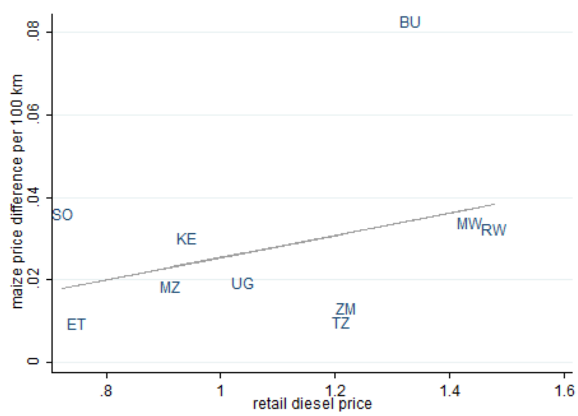

Figure 2: Country-level correlation of fuel prices and maize price differences

In the next section, I develop a simple structural model to illustrate how trade policies, fuel prices, and infrastructure quality affect total trade costs (which are unobservable) and how trade costs in turn affect price differences (which I observe).

3 Model and Empirical Strategy

Consistent with the reality in the region, I assume that there is a competitive zero-profit maize trading sector with constant marginal costs. Unlike the manufactured goods trading sector considered by Atkin and Donaldson (2012) and others, for which distributors in developing countries have significant market power, the trading sector for staple agricultural commodities like maize - which has tens of millions of producers in East and Southern Africa - has few if any barriers to entry. Although imperfect competition is important in more remote rural African grain markets that are small and thinly-traded, larger hub markets of the type in my dataset tend to be highly competitive with many traders and low firm concentration ratios (Osborne 2005; Aker 2010).

Let p_{it} and p_{jt} be the market prices for maize at time t in adjacent markets i and j. Let τ_{ijt} be the total trade costs per unit of maize from market i to market j at time t. Since there is significant anecdotal evidence from the region that borders are porous and export bans are never completely enforced, I model export bans as an increase (perhaps a very large increase) in trade costs and therefore assume that trade is always possible between markets, i.e. $0 < \tau_{ijt} < \infty \ \forall i, j, t$.

Total trade costs τ_{ijt} consist of the following four components:

1) Non-fuel transportation costs. This includes truck and driver hire, costs of loading and unloading, etc. I assume that these costs are symmetric for a given market pair (since traders may hire a truck and driver based in either of the two markets) and are constant over the time period in my dataset except for discrete changes upon completion of infrastructure projects. Letting $r_{(ij)}$ represent the constant component and $I_{k(ij)t}$ be an indicator variable for the completion of infrastructure project k affecting market pair (ij), non-fuel transportation costs can be written as $r_{(ij)} + \sum_k \beta_k I_{k(ij)t}$.

2) Fuel costs. Fuel costs can be written as $\beta_g D_{(ij)} g_{it}$, where β_g is the product of the average consumption in liters per kilometer of a truck multiplied by the average fraction of a truck taken up by a unit of maize, $D_{(ij)}$ is the distance in kilometers between markets i and j, and g_{it} is the retail price of a liter of diesel in market i in period t.

3) Taxes, bribes, and exchange costs. Transport of maize is subject to official *ad valorem* international tariffs t_{jt} and unobserved taxes, bribes, and exchange costs $t^U_{(ij)}$ (which I assume are constant over time for each market pair). In addition, in the case of an applicable export ban in country i (indicator variable X_{it}), transport of maize is subject to an additional unknown price-independent cost of β_X. Total taxes and bribes per unit maize are therefore $\beta_X B_{(ij)} X_{it} + \beta_t B_{(ij)} t_{jt} p_i + t^U_{(ij)}$ where $B_{(ij)}$ is an indicator variable

for an international border between i and j. I have added a coefficient $\beta_t < 1$ on the tariff term because tariffs are often only partially applied in exchange for bribes ($\beta_t = 1$ if tariffs are perfectly enforced).
4) Wholesale-retail markups (local distribution costs). This includes storage, labor, local transport, store rent, local taxes, etc. In the case where I observe retail prices in i and j, I make the assumption that wholesale-retail markups in a given market are constant over the time period of my dataset, but I allow wholesale-retail markups to vary from market to market (e.g. an urban market may have a larger markup than a rural market). Let M_i be an indicator variable equalling 1 if the price data I have for market i is retail price data and m_i be the wholesale-retail markup in market i. Then the net effect of wholesale-retail markups on trade costs between markets i and j can be written as $-M_i m_i + M_j m_j$.

Combining the costs from the four components above and adding directional market pair fixed effects ϕ_{ij}, which absorb several terms at once[4], I obtain the following expression for total trade costs τ_{ijt}:

$$\tau_{ijt} = \beta_X B_{(ij)} X_{it} + \beta_t B_{(ij)} t_{jt} p_i + \beta_g D_{(ij)} g_{it} + \sum_k \beta_k I_{k(ij)t} + \phi_{ij} \tag{1}$$

Equation (1) summarizes how changes in export bans, tariff rates, fuel prices, and infrastructure affect overall trade costs given the assumptions I have made about various other factors staying constant over time. Even if these other factors did vary over time, equation (1) would still be identified with the addition of an error term as long as that variation is stochastic and not correlated with the regressors. However, since I observe price differences rather than total trade costs, understanding how trade costs translate into price differences is essential for equation (1) to be of use.

The spatial price analysis literature has grappled repeatedly with how to use differences in prices, which are relatively easy to observe, to make inferences about total trade costs, which are difficult if not impossible to observe (Fackler and Goodwin 2001). I adapt the theoretical framework from the literature, starting with a no-arbitrage condition. For each period t for each pair (ij), I assign index j to the higher priced market and index i to the lower priced market and define $\Delta p_{(ij)t} = p_{jt} - p_{it} \geq 0$ and $\tau_{(ij)t} = \tau_{ijt}$. The competitive, zero-profit trading sector will bring maize from market i to market j if $\mathbb{E}[\Delta p_{(ij)t}] \geq \tau_{(ij)t}$. Competition ensures that the following no-arbitrage condition holds:

$$\tau_{(ij)t} \geq \mathbb{E}[\Delta p_{(ij)t}] \tag{2}$$

Following Baulch (1997) and others, I identify three possible trading regimes based on the relative

[4]Note that $\phi_{ij} \neq \phi_{ji}$ due to the addition and subtraction of the markup terms ($-M_i m_i + M_j m_j \neq -M_j m_j + M_i m_i$).

magnitude of actual observed price differences and the unobserved total trade costs:

1) In regime 1, $\Delta p_{(ij)t} = \tau_{(ij)t}$. The markets are in a competitive tradable equilibrium with no arbitrage opportunities. Maize is tradable from i to j and $\Delta p_{(ij)t}$ increases one for one with an increase in $\tau_{(ij)t}$.

2) In regime 2, $\tau_{(ij)t} > \Delta p_{(ij)t}$. The markets are in segmented equilibrium. Trade does not occur because the price difference between the markets is too small and the trade costs too large. Local prices are determined by local supply and demand, and price differences are unaffected by changes in trade costs (unless such changes cause the market pair to enter one of the other regimes).

3) In regime 3, $\Delta p_{(ij)t} > \tau_{(ij)t}$. Here the markets are in disequilibrium following a shock in which the realized price difference is greater than the expected price difference. There are arbitrage opportunities from i to j. There may be an adjustment period back to a regime 1 competitive tradable equilibrium.

For markets with relatively low trade costs and consistent import-export relationships, restricting attention to regimes 1 and 3 would be appropriate, in which case the relationship between trade costs and price differences would be relatively straight-forward. I assume that any adjustment period is shorter than the one month time intervals in my price dataset and therefore model regime 3 with a mean-zero normally distributed error term ϵ based on a realization that differs from expectation. Intuitively, under regime 1 traders transport according to expected price differences, but shocks may cause those price differences to be larger ($\epsilon > 0$) or smaller ($\epsilon < 0$) than expected, in which case they make a small profit or loss that particular period. Thus I have $\Delta p_{(ij)t} = \tau_{(ij)t} + \epsilon_{(ij)t}$ for regimes 1 and 3, which translates into the following basic specification based on equation (1) above:

$$\Delta p_{(ij)t} = \beta_X B_{(ij)} X_{it} + \beta_t B_{(ij)} t_{jt} p_i + \beta_g D_{(ij)} g_{it} + \sum_k \beta_k I_{k(ij)t} + \phi_{ij} + \epsilon_{(ij)t} \tag{3}$$

However, given the relatively high trade costs and the fact that maize is produced locally in almost all of the areas covered by my dataset, it would be unrealistic to assume that no observations fall under regime 2 and to base my estimation solely on equation (3). Suppose that market pair (ij) falls into regime 2 with probability $\pi_{(ij)t} \in [0, 1]$, which is an increasing function of trade costs $\tau_{(ij)t}$. Then the equation actually determining price differences is not (3) but rather:

$$\Delta p_{(ij)t} = [1 - \pi_{(ij)t}(\tau_{(ij)t})][\tau_{(ij)t} + \epsilon_{(ij)t}] + \pi_{(ij)t}(\tau_{(ij)t})[\Delta p_{(ij)t} | \Delta p_{(ij)t} < (\tau_{(ij)t} + \epsilon_{(ij)t})] \tag{4}$$

Whereas estimating equation (3) gives reduced-form estimates conditional on a particular history of $\pi_{(ij)t}(\tau_{(ij)t})$'s, which can be used to determine the actual effects of past policies, estimating equation (4) yields the "correct" estimates of the structural parameters conditional on being in a tradable equilibrium, which are necessary for predicting the effects of similar policies in other circumstances. However, equation (4) clearly cannot be estimated without knowing more about the domestic supply and demand curves for each market in each time period, which affect both the term for the price differences in segmented equilibrium and the probability $\pi_{(ij)t}(\tau_{(ij)t})$ itself.

The spatial price analysis literature has attempted to deal with the possibility of regime 2 by making significant assumptions that are unrealistic in my context. The two most common approaches are the Parity Bounds Model (PBM) and the Threshold Autoregressive (TAR) model. The PBM, formalized by Baulch (1997) based on Sexton, Kling, and Carman (1991) and others, uses a single observation of baseline transport costs between markets combined with distributional assumptions on the price differences within each regime to estimate the probability that two markets fall in each regime and draw conclusions about their degree of integration. The TAR model, as applied to commodity markets by Obstfeld and Taylor (1997), uses time series properties and the assumption of a fixed but unknown trade cost to estimate the probability of a market pair falling in regime 3 and an adjustment parameter for the speed with which the pair returns to a no-arbitrage equilibrium (regimes 1/2). In their review of this literature, Fackler and Goodwin (2001) highlight the problematic distributional and functional form assumptions in these highly-parameterized models. Moreover, these models cannot readily accommodate changes in trade costs over time.

In light of these shortcomings, I adopt a more inductive approach in the spirit of more recent papers such as Moser, Barrett, and Minten (2009). In particular, suppose that there are regime 2 observations and that they can be identified. Estimating equation (3) without the regime 2 observations would therefore give the correct estimates of the structural parameters, while estimating it with all observations would give estimates of these coefficients that are biased towards zero. Recent empirical evidence in contexts where regime 2 observations can be identified confirms this downward bias when all observations are included (Atkin and Donaldson 2012).

In the following section, I take equation (3) as my basic specification and then experiment with different ways of identifying and eliminating potential regime 2 data points using varying thresholds of minimum feasible transport costs. For my data, this simple exercise enables me to adequately control for the possibility of regime 2 observations and confirm the robustness of my baseline results. In other cases, similar techniques could at the very least be used to establish tighter bounds on coefficient estimates. Given the importance of the policy issues that often hinge on spatial price analysis, it seems worthwhile to employ this type of inductive approach rather than not estimating at all due to the potential problems regime 2 poses for identification or making unrealistic distributional or functional form assumptions that might bias results in less transparent ways.

4 Baseline Results and Robustness Checks

In order for the estimates for the equations derived in the previous section to be unbiased, the right-hand side variables must be exogenous, i.e. maize price differences between markets in a given time period must not affect trade policy implementation, fuel prices, or infrastructure project completion. Fuel prices, which are largely determined by prices in the international markets supplying the region, are clearly not affected by local maize price differences. While infrastructure projects and trade agreements may be more likely to be put in place between markets with larger *ex ante* price differences, they are typically planned years in advance, so actual completion/implementation dates are predetermined and unlikely to be affected by price differences in proximate periods. Export bans and tariff waivers, on the other hand, are discretionary policies used by governments to affect maize markets, so their exogeneity is less certain. Although high maize *prices* are often used as the rationale for imposing these policies, intuition suggests that they are exogenous to price *differences* between markets. In particular, the events that typically trigger these policies - international price spikes, local or regional production shortfalls, etc. - seem unlikely to affect the trade costs between markets. If trade costs are unchanged, trade policies would only be endogenous to price differences if the events that trigger them also cause market pairs to shift from one trading regime to another.

I evaluate exogeneity of discretionary trade policies by testing whether price differences for a given market pair in the months leading up to the implementation of an export ban or tariff waiver differ significantly from price differences in other non-policy periods. To do so, I adapt a technique from de Janvry, McIntosh, and Sadoulet (2010) and run the following regression, where m_{-n} are indicator variables for the month n prior to the implementation of the policy and $\phi_{(ij)}$ are market pair fixed effects:

$$\Delta p_{(ij)t} = \beta_1 m_{-1,(ij)t} + \beta_2 m_{-2,(ij)t} + \beta_3 m_{-3,(ij)t} + \beta_4 m_{-4,(ij)t} + \phi_{(ij)} + \epsilon_{(ij)t} \tag{5}$$

For each policy type, I run the regression for non-policy periods for all market pairs affected by the policy. Results in table 4 suggest that export bans are exogenous to price differences but tariff waivers are not. There is no significant difference in price differences in the months leading up to an export ban, but price differences are 2-4 cents higher in the months leading up to a tariff waiver.

Table 4: Test of Exogeneity of Trade Policies

	(1)	(2)
1 month prior	-0.00881	0.0444***
	(0.0122)	(0.0121)
2 months prior	-0.0140	0.0332*
	(0.0132)	(0.0164)
3 months prior	-0.00990	0.0397**
	(0.0110)	(0.0154)
4 months prior	0.00275	0.0239***
	(0.0115)	(0.00817)
Policy	Export ban	Tariff waiver
Observations	1669	2229
Links	29	22

Note: Robust standard errors in () clustered by link; *significant at 10%, ** at 5%, *** at 1%.

These results are consistent with anecdotal evidence that suggests that tariff waivers are often implemented after a domestic production shortfall that has shifted the country from a self-sufficient regime 2 equilibrium to a regime 1 importing equilibrium. In contrast, export bans are typically implemented in countries that are consistently in a regime 1 exporting equilibrium, so the events that trigger export bans are not associated with changes in the prevailing trading regime. In the estimations that follow, I address the endogeneity of tariff waivers by dropping all observations under tariff waivers from my dataset and showing that doing so does not affect the estimation of my other coefficients.

Having established that export bans are exogenous to price differences, I proceed to use my data to estimate my basic specification from equation (3). Standard errors for this estimation must be calculated carefully. A first step is to cluster at the market pair level since as with most panel data $\mathbb{E}[\epsilon_{(ij)t_1}, \epsilon_{(ij)t_2}] \neq 0$. However, the market pair structure also has features of a dyadic regression. In particular, a price shock in a given market will affect the price differences of all of its market pairs: $\mathbb{E}[\epsilon_{(ij)t}, \epsilon_{(ik)t}] \neq 0$ and $\mathbb{E}[\epsilon_{(ij)t}, \epsilon_{(kj)t}] \neq 0$. Fafchamps and Gubert (2007) have derived formulas for consistent standard errors in dyadic regressions, but their technique is not easily applied to a panel data setting where there are multiple observations per pair and the pairs are not saturated (each market is in a pair with only a small specified subset of the other markets). Instead, my preferred specification employs the multi-way clustering technique developed by Cameron et al. (2011) to cluster at both the origin market level and the destination market level, which addresses directly the correlation issues described above. This approach could be relevant in other studies where the dyadic dependent variable is a difference in variable values between the dyad members (in this case the difference in price between the two markets).

Table 5 reports results from my basic specification. Column (1) shows results including the data points under tariff waivers, while column (2) - my preferred specification - shows results with these data points excluded. In both cases, I cannot reject the null hypothesis that export bans have no effect on price differences. The fact that tariff waivers are implemented during periods with high price differences explains the negative coefficient on the tariff-price term in column (1). Excluding the tariff waiver data points so that variation in tariff rates is driven only by exogenous shifts in tariffs leads me to fail to reject the null hypothesis that tariff changes have no effect on price differences.

Table 5: Basic Specification

	(1)	**(2)**	(3)	(4)
Export ban	-0.00291	**-0.00266**	-0.00266	-0.00285
	(0.119)	**(0.0128)**	(0.0133)	(0.0126)
Tariff * Origin price	-0.151**	**0.0667**	0.0667	0.0532
	(0.0709)	**(0.195)**	(0.158)	(0.191)
Distance * Gas price	0.0000364***	**0.0000355*** **	0.0000355***	0.0000339***
	(0.0000100)	**(9.42E-06)**	(8.18E-06)	(8.99E-06)
Infrastructure	Yes	**Yes**	Yes	No
Observations	12104	**11855**	11855	11855
Clustered errors	Origin & Destination	**Origin & Destination**	Link	Origin & Destination
Clusters	79 & 79	**79 & 79**	139	79 & 79

Note: Robust standard errors in () clustered as indicated; *significant at 10%, ** at 5%, *** at 1%.

Using the standard errors from my preferred specification and the mean maize price of 26.7 cents from table 1, I can reject an alternate hypothesis that export bans have an effect at least as large as that of a 5% export tax ($0.05 * 26.7 = 1.34$ cents) at an 11% 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, if temporary 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. For tariffs, I can reject at the standard 5% significance level a hypothesis that tariff rate changes have an implementation level of at least 40%.

In contrast to the results for trade policies, changes in the retail diesel price have a highly significant effect on price differences. The point estimate from my preferred specification indicates that a 1 dollar rise in the price of a liter of diesel causes the price difference to increase by 0.355 cents per kilogram (35.5 cents per 100 kilograms) for every 100 kilometers between the origin and destination markets. This is remarkably consistent with a back-of-the-envelope calculation based on my model in Section 3.

For a typical 10-ton truck consuming 0.4 liters per kilometer (40 L/100km = 6.25 mpg), the expected increase in trade costs per kilogram of maize per 100 kilometers with a 1 dollar rise in fuel costs is: $\beta_g D_{(ij)} \Delta g_{it} = 0.0001 * 0.4 * 100 * 1 = 0.004 = 0.4$ cents.

Column (3) of table 5 shows standard errors clustered at the more conventional market pair level, which does not change the significance of my results as compared to my preferred multi-way clustering at the origin and destination market levels. For all subsequent pair-level regressions in this paper, I compute both sets of standard errors using the two different clustering approaches but report only the multi-way clustering standard errors as the differences between the two are not significant.

Coefficients for the 22 infrastructure project indicator variables are not reported in table 5 since the estimates lack statistical power. Column (4) shows that my results are robust to excluding these variables.

Table 6 shows that my basic results are robust to controlling for time-specific shocks, time trends, the inclusion of domestic market pairs, outliers, the unbalancedness of the panel, and the restriction to first-order market pairs. Column (1) shows the results of my preferred specification from column (2) of table 5 for purposes of comparison. In column (2), I include quarter indicator variables and a time trend to control for the passage of time. My results for the coefficients on export bans and tariffs are still not significantly different from zero. Similar results were obtained using year or month indicator variables, with or without the time trend. The estimate of the coefficient on fuel prices is much smaller and not significantly different from zero in this specification, but this is to be expected given that fuel prices are highly correlated across markets since they are driven by international fuel prices.

Table 6: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
Export ban	**-0.00266**	-0.00906	-0.00143	0.00224	0.00270	0.00114
	(0.0128)	(0.0116)	(0.0134)	(0.00571)	(0.00255)	(0.00777)
Tariff * Origin price	**0.0667**	-0.0206	0.0808	0.0687	0.208	0.0151
	(0.195)	(0.151)	(0.206)	(0.195)	(0.137)	(0.130)
Distance * Gas price	**0.0000355*** **	5.86E-07	0.0000197*	0.0000333***	7.10E-06	0.0000263***
	(9.42E-06)	(9.06E-06)	(0.0000107)	(8.90E-06)	(9.32E-06)	(6.74E-06)
Time trend		0.000496				
		(0.64)				
Time fixed effects		Quarter				
Observations	**11855**	11855	2831	11409	5269	30881
Market Pairs	**139**	139	37	133	74	360
Clusters	**79 & 79**	79 & 79	47 & 47	76 & 76	54 & 55	79 & 79

Note: Robust standard errors in () clustered by origin and destination; *significant at 10%, ** at 5%, *** at 1%.

In column (3) of table 6, I rerun my basic regression using only the 37 market pairs that span an international border. This only affects the estimate of the coefficient on fuel prices, which is slightly smaller and less significant, possibly reflecting some error introduced by trucks buying fuel in destination countries rather than origin countries. In column (4), I return to my full dataset but exclude the three markets in South Sudan and the breakaway republic of Somaliland (Juba SS, Hargeisa SO, and Burao SO). These markets were significant outliers in terms of both prices and price differences in the summary statistics in table 1, and fluctuations in their security situation could conceivably have introduced bias, but their exclusion does not affect my results. It does, however, reduce the standard error on my point estimate for export bans significantly. 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 (5) of table 6, I explore whether the unbalancedness of the panel is affecting my results by trimming my dataset. To maximize balancedness, I first exclude all data points before September 2005 (when monitoring began in Burundi), reducing my dataset to 6 years and 4 months. Then I exclude 25 markets (and the 66 market pairs of which they are members) with more than 3 months of missing data in this shorter time period. With these adjustments, of the 4104 possible price observations in my new panel, only 37 (0.9%) are missing, as opposed to 18% in my original panel. As shown in column (5), rerunning my basic specification with this trimmed panel does not affect my basic null results, although the coefficient on gas prices is smaller and insignificant.

Finally, in column (6), I add 220 second-order market pairs (including 113 second-order international market pairs) to my original 139 first-order market pairs, relaxing my assumption that higher order market pairs could be excluded due to the linear additivity of trade costs. Theoretically, if markets A and C are connected through market B, I expect trade costs for second-order pair AC to be the sum of trade costs for first order-pairs AB and BC. However, due to higher trade costs, second-order pair AC is more likely to be in a regime 2 segmented equilibrium than the first-order pairs are. Hence, including second-order pairs might bias the estimates of my structural parameters towards zero. 236 second-order pairs are possible in my network - I include all except 16 that involve crossing two borders. My results with 359 first and second-order pairs, shown in column (6), are not significantly different from my basic specification, although all coefficient estimates are closer to zero, consistent with an increased frequency of regime 2 observations.

In table 7, I proceed to directly address the possibility of bias from regime 2 observations using the strategy developed in the previous section. Teravaninthorn and Raballand (2009) present data on transport prices for several major African transport routes that range from 6 to 11 cents per ton-kilometer.

In the upper panel of table 7, I progressively exclude observations of price differences for each market pair that fall below 2.5, 5, 7.5, and 10 cents per ton-kilometer. This cuts out increasing numbers of potential regime 2 observations and should reduce the downward bias in the estimates of my structural parameters. Consistent with my model, the coefficient on fuel prices increases as I eliminate more of the potential regime 2 observations, although all of the estimates are within the 95% confidence interval of my original estimate, shown in column (1). The coefficients for both export bans and tariffs, on the other hand, remain statistically indistinguishable from zero. Thus I conclude that my failure to reject the null hypotheses of zero effect of export bans and tariff rate changes is not due to the presence of regime 2 observations.

Table 7: Regime 2 Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Export ban	**-0.00266**	-0.000720	0.000615	-0.000284	0.000442
	(0.0128)	(0.0143)	(0.0161)	(0.0180)	(0.0200)
Tariff * Origin price	**0.0667**	0.0793	0.0878	0.0908	0.0639
	(0.195)	(0.177)	(0.181)	(0.170)	(0.166)
Distance * Gas price	**0.0000355*** **	0.0000430***	0.0000439***	0.0000466***	0.0000534***
	(9.42E-06)	(0.0000105)	(0.0000124)	(0.0000145)	(0.0000195)
Threshold		2.5 cents/Tkm	5 cents/Tkm	7.5 cents/Tkm	10 cents/Tkm
Observations	**11855**	10592	9343	8277	7313
	(6)	(7)	(8)	(9)	(10)
Export ban	-0.00134	-0.00156	-0.000800	-0.00184	-0.00414
	(0.0139)	(0.0153)	(0.0167)	(0.0197)	(0.0218)
Tariff * Origin price	0.0518	0.0889	0.0808	0.0732	0.0857
	(0.177)	(0.177)	(0.153)	(0.140)	(0.129)
Distance * Gas price	0.0000392***	0.0000414***	0.0000424***	0.0000434***	0.0000454***
	(9.92E-06)	(0.0000111)	(0.0000131)	(0.0000151)	(0.0000181)
Threshold	1 cent	2 cents	3 cents	4 cents	5 cents
Observations	10533	9127	7867	6698	5701

Note: Robust standard errors in () clustered by origin and destination; *significant at 10%, ** at 5%, *** at 1%.

In the lower panel of table 7, I experiment with an even simpler way of controlling for potential regime 2 observations. If benchmark data on transport costs were unavailable, another possible method is to exclude observations with the smallest price differences as potential regime 2 observations. Here, I progressively exclude observations with price differences less than 1 cent, 2 cents, 3 cents, 4 cents, and 5 cents. The results are qualitatively the same as those obtained using the per-kilometer transport cost thresholds in the upper panel. This suggests that this could be a viable alternative for controlling for possible bias of regime 2 observations in contexts where transport cost data is unavailable.

All of my robustness checks have confirmed that export bans (and tariff rate changes) have no detectable effect on price differences between markets. In a further set of robustness checks not presented here, I interacted implementing country indicator variables with the export ban indicator variable to look at potential heterogeneous effects. Again, none of the coefficients were statistically different from zero, indicating that none of the countries' export bans had a statistically significant effect on price differences. I also ran similar regressions using individual ban indicator variables and found that only 1 of the 13 export bans had an effect on price differences significant at the standard 5% level[5].

In the next section, I evaluate possible explanations of my surprising results and develop a qualitative description of what takes place during an export ban.

5 Analysis

Several explanations for my failure to reject the null hypotheses of zero effect for export bans and tariff rate changes are possible. First, international trade might never occur so that trade policies are always non-binding. International trade costs might be so high that border-spanning market pairs are always in regime 2, segmented equilibria with local prices determined by domestic supply and demand. However, both official government statistics and data collected by FEWS NET monitoring of cross border trade at border points throughout the region during the study period indicate that maize is actively traded across borders, allowing me to rule out permanent segmented equilibrium.

A second, more plausible explanation is that trade policies are not fully enforced. As shown in Section 2, market pairs spanning borders have larger price differences, but this may be due to factors that are not sensitive to changes in official trade policy. For example, the working paper version of Jayne, Myers, and Nyoro (2008) describes informal arrangements whereby Kenyan border officials record and charge tariff on a small fraction of the maize traders bring across the border in exchange for a bribe. The authors also cite reports that a significant portion of maize imported into Kenya from Uganda and Tanzania is smuggled and hence not subject to tariffs at all. Their empirical analysis shows that the 25-30% tariffs imposed by Kenya during their study period (1989-2004) had only a 3-5% effect on maize prices. In this environment of imperfect enforcement, changes in official tariff rates might not translate into changes in trade costs and price differences. For example, given a long-standing bribery arrangement between a border official and a trader, a change in official tariff rates could easily have no effect on the size of the

[5]The one ban with a statistically significant effect was the ban in Zambia that was already in place at the beginning of my dataset. This is consistent with my findings in the next section that price differences only increase during the final months of export bans. It should also be noted that many of the coefficient estimates for the individual ban indicator variables for the shorter and earlier bans (including this one) have low statistical power due to the small number of observations.

payments the trader makes at the border.

While imperfect implementation may be a sufficient explanation for the null effect of changes in tariff rates, it does not explain the lack of a detectable effect for export bans. Export bans are dramatic, highly-publicized policies that are designed to be easily and visibly enforced, and news reports from the region suggest that the bans did disrupt trade even if some maize may still have crossed borders through more costly informal smuggling routes. In addition, in contrast to tariffs, since export bans are all-or-none policies, even if border officials let some maize through during bans in exchange for bribes they would be unlikely to collect an equivalent bribe in the absence of a ban, so export bans should have a significant effect on trade costs.

I proceed to use my dataset to assemble suggestive evidence for what actually happens during an export ban. I first adapt a technique used by Engel and Rogers (1996) to see if export bans affect the variability of price differences even if they don't have a statistically significant effect on their magnitude. For each of the 29 market pairs affected by export bans, I calculate the standard deviation of the price differences during ban periods and during non-ban periods. I then run the following reduced form regression:

$$SD_{(ij)X} = \alpha + \beta X + \phi_{(ij)} + \epsilon_{(ij)X} \tag{6}$$

where $X = 1$ for export ban periods, $X = 0$ for non-ban periods, and $SD_{(ij)X}$ is the standard deviation of the price differences of market pair (ij) during period type X. My results (not shown here) indicate that the standard deviation of price differences is 27% higher in export ban periods than in non-ban periods (significant at the 5% level). Similar regressions for prices indicate that the standard deviations of origin and destination market prices are 48% and 49% higher in export ban periods than in non-ban periods (significant at the 1% level). These results cannot be interpreted causally unless export bans are exogenous to volatility, which cannot be readily tested. However, the correlation between export bans and volatility suggests that the bans may be destabilizing local markets and trade even if they are not affecting the size of price differences.

Despite the fact that origin market prices, destination market prices, and price differences are more volatile during export bans, prices and price changes on either side of the border are *more* correlated during bans than during non-ban periods. For the 29 market pairs directly affected by export bans, the correlation coefficient of the prices in each pair during normal non-ban periods (1722 observations) is 0.558, while the correlation coefficient of the prices during export bans (1613 observations) is 0.592.

Using the Fisher transformation (under the assumption of a bivariate normal distribution), a hypothesis of equal correlation can be rejected at a 5% level. Similarly, the correlation coefficient of price movements ($p_t - p_{t-1}$) is 0.0947 during non-ban periods but rises to 0.233 during bans, with a hypothesis of equal correlation rejected at a 1% level. This suggests that origin and destination market prices track each other more closely during bans than they do during non-ban periods.

I next try running reduced form regressions using origin and destination prices as my dependent variables based on my original specification for price differences. I include market-level fixed effects and control for fuel prices and infrastructure projects, estimating:

$$p_{mt} = \beta_1 B_{(ij)} X_{it} + \beta_2 g_{it} + \sum_k \beta_k I_{k(ij)t} + \phi_m + \epsilon_{mt} \tag{7}$$

for $m = i$ and $m = j$. Export bans are likely to be endogenous to prices, so once again such regressions cannot have a causal interpretation. However, one would expect *ex ante* that export bans - implemented with the intention of insulating domestic markets from high international prices - would keep origin market prices in relative control as compared to high destination market prices. Instead, results in table 8 show that prices in *both* origin and destination markets are 3-4 cents higher than average during export bans.

Table 8: Price Regressions

	(1)	(2)
Export ban	0.0402***	0.0341***
	(0.00951)	(0.0141)
Gas price	0.122***	0.114***
	(0.0142)	(0.0131)
Dependent variable	Destination price	Origin price
Observations	13306	13732

Note: Robust standard errors in () clustered by market; *significant at 10%, ** at 5%, *** at 1%.

In table 9, I adopt a more dynamic approach, looking for potential effects on price differences at different stages of export bans. First, I divide the 8 export bans for which I have start-to-finish data into quarters and re-estimate equation (5) for the market pairs directly affected by these bans, letting m_{-n} be indicator variables for the first, second, third, and fourth ban quarters. Results in column (1) show that there is a significant increase in price differences in the final quarter of export bans. To confirm this result, I rerun the regression using only data points during export bans and letting m_{-n} be indicator variables for the month n prior to the lifting of the ban. Results in column (2) confirm that price differences are significantly larger in the final few months of export bans than their average during the bans.

Table 9: Price Differences at the End of Export Bans

	(1)		(2)
1st quarter of ban	0.00969	1 month before	0.0390***
	(0.0167)		(0.0101)
2nd quarter of ban	0.0232	2 months before	0.0411***
	(0.0197)		(0.0110)
3rd quarter of ban	0.0123	3 months before	0.0346***
	(0.0160)		(0.0105)
4th quarter of ban	0.0434***	4 months before	0.0364***
	(0.0113)		(0.00754)
Observations	3335	Observations	1614

Note: Robust standard errors in () clustered by link;
*significant at 10%, ** at 5%, *** at 1%.

Finally, I undertake some simple graphical analysis to help visualize what goes on during export bans. In figure 3, I combine data from the market pairs directly affected by the 8 export bans for which I have complete start-to-finish data to plot the average price in the origin market and the average price in the destination market just before, during, and just after an export ban. The vertical lines in figure 3 mark the beginning and end of the ban. Because the 8 bans very greatly in length, the middle months of each ban are divided into six periods of equal length to enable direct comparison.

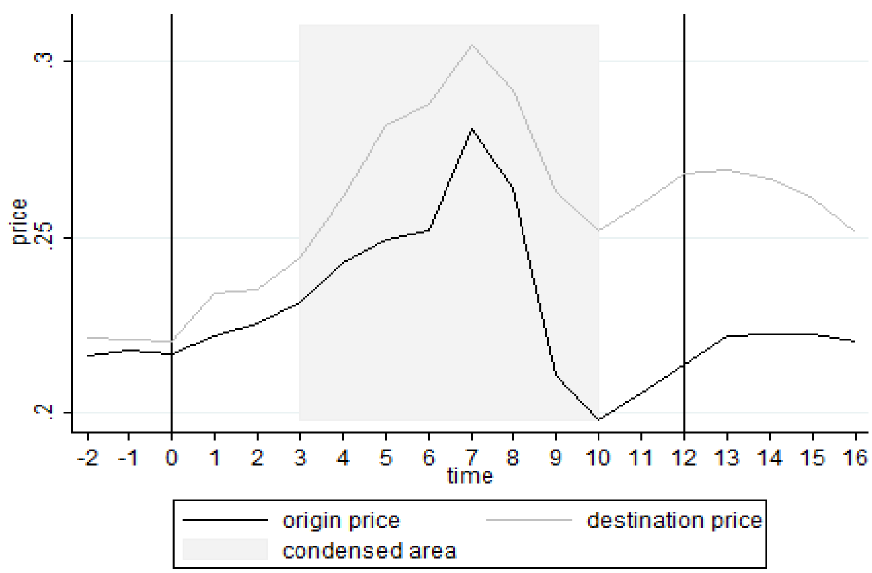

Figure 3: Average evolution of prices during export bans

Figure 3 should be interpreted carefully since prices and price differences vary greatly from market pair to market pair, price series vary greatly from ban to ban, and the relatively low number of observations (59) for each period makes precise analysis difficult. However, four qualitative observations can be made, which are robust to the elimination of each of the 8 individual bans from the overall graph. First, origin and destination market prices rise in tandem during the initial months of an export ban and continue to track each other during the ban. This is consistent with my null result of no statistically significant effect of bans on price differences and my finding that origin and destination prices and price changes are more correlated during bans than during non-ban periods. Second, prices in both destination and origin markets appear to be generally higher and more volatile during export bans, in line with the regression results from equations (6) and (7). Third, price differences widen significantly near the very end of the ban, as was shown by the results in table 9. This widening of price differences typically occurs after a sharp fall in both prices in a period corresponding to the local maize harvest. And fourth, these large price differences take at least several months to disappear after the end of the ban, which anecdotal evidence suggests is due to trade backups resulting from diverse factors including limited issuance of export licenses in the months following the official lifting of the ban and insufficient transport capacity for large quantities of backed-up maize.

Taken together, the suggestive results in this section allow me to assemble a qualitative description of what happens during export bans that is consistent both with anecdotal evidence from the region and with optimizing behavior by competitive traders in a rational expectations storage model (Gustafson 1958; Williams and Wright 1991). A critical issue for traders in both origin and destination countries is that the ban is temporary and its length discretionary. Even if an initial duration is announced by the government, it is often prolonged, and traders in both countries must factor in their expectations about the ban duration when making storage decisions.

The initial imposition of an export ban seems to cause prices in both origin and destination countries to rise. Destination country prices rise since supplies from the origin country are cut off and maize during the ban must be sourced either from the country's own stocks or from alternative, more costly exporters. As a ban of uncertain duration continues without being lifted, prices may be driven ever higher until a supply response can occur. The rising destination country price in turn causes a rise in the expected future price for origin country traders, since when the ban is lifted they expect to be able to sell maize abroad at the higher price. This causes origin country traders to increase stocks of maize, decreasing supply in origin country markets and thereby increasing the domestic price.

With rising domestic prices, the government keeps the ban in place. Eventually, a supply response in both countries, compounded in some cases by external factors on the world market and sharply increasing costs of storage in the origin country (as existing facilities reach capacity) causes prices to fall and price differences to widen. As the price difference between the origin and destination countries grows, producers and traders begin putting increasing pressure on the government to lift the ban, which it does since prices have fallen. This period of large price differences at the end of bans is sufficiently short that the average effect of export bans on price differences is not significantly different from zero.

This description is consistent with the analysis of maize policy in the region by Tschirley and Jayne (2010), who describe a "credible commitment problem" between governments and traders who must each base their behavior on expectations of what the other will do but are unable to make credible commitments to each other, resulting in sub-optimal outcomes. It is also remarkably similar to a qualitative description of Russia's 2010-2011 wheat export ban by Welton (2011). Since Russia is a major supplier for international wheat markets, its export ban in August 2010 caused international prices to increase. However, domestic wheat prices continued to track international wheat prices during the ban (price differences were unaffected). Welton attributes much of this phenomenon to hoarding of wheat by Russian traders, who put the wheat destined for export in storage rather than selling it domestically as the government had intended. Eventually, Russian wheat prices started to fall, diverging from international prices, and the government (under pressure from producers and traders) let the ban expire in July 2011. Although the empirical effects of short-term export bans may vary depending on the institutional and policy environment, the parallels between my findings and the Russian case suggest that similar patterns may emerge in different contexts.

6 Conclusion

I have used an extensive dataset on maize prices and trade policies in 12 African countries over 10 years to investigate the empirical effects of short-term export bans. Drawing on the spatial price analysis literature, I developed a structural model to shed light on how changes in trade policies and transportation costs affect overall trade costs and how trade costs translate into price differences under different equilibrium regimes. My initial estimation based on this model yielded the surprising result that export bans do not have a statistically significant effect on the price differences between markets. This result is robust to a variety of alternative specifications and modifications of the dataset, including the elimination of potential non-traded equilibria and the inclusion of second-order market pairs. I am also able to reject a hypothesis that the effect of export bans on price differences is at least as large as the theoretical effect of a 5% export tax. I employ several novel techniques in my estimation - including multi-way clustering at the origin and destination market levels and identification of potential non-traded equilibria using varying thresholds of minimum transport costs - that may be useful in other contexts with dyadic regressions or high trade costs.

Further analysis enabled me to develop a qualitative description of what actually takes place during a typical export ban that helps explain my quantitative results. Export bans are correlated with equivalent price increases in both destination and origin countries. Implementation is typically followed by a price surge on both sides of the border, and origin country prices continue to track destination country prices despite the fact that trade is cut off. This is consistent with optimizing behavior by origin country traders who store maize with the expectation that the ban will be lifted and exports will resume. Eventually, prices fall and price differences widen, prompting the government to lift the ban.

My results have significant policy implications. Export bans and other short-term trade policies have been widely used in recent years to respond to international price fluctuations and domestic production shortfalls. In 2011 alone, 3 of the 12 countries in my sample announced new export bans on maize, with the most recent ban by Malawi announced on December 29th. While export bans are disruptive for neighboring 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 their empirical effects are often not what policy-makers might expect. Instead of keeping scarce maize at home and lowering prices, export bans may effectively tie it up in storage, causing prices in both origin and destination countries to rise further than they otherwise would. Volatility of both prices and price differences is higher during export bans than during normal periods. Since governments typically conceive of and justify short-term export restrictions as price stabilization mechanisms intended to prevent domestic price increases and limit volatility, my findings suggest that they may need to reconsider their use of these policies.

Many of the results in this paper are suggestive and open up significant avenues for further research to confirm or better understand them. A better empirical understanding of the strategic behavior of the private sector in response to discretionary short-term trade policies would be particularly helpful in explaining or predicting unintended consequences. Empirical work on border crossings and the extent to which changes in trade policies are implemented could help explain why some policy changes appear to have little or no effect. Finally, with continued high and volatile prices for agricultural commodities and the increasing frequency of disruptive climatic events, exploration of alternative options for price stabilization continues to be an urgent research priority.

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