Export Bans and Market Integration: The Case of African Maize

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Abstract

I use an extensive dataset 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. I evaluate alternative explanations of this surprising result and develop a qualitative dynamic 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 widespread use of temporary export restrictions as stabilization policies in response to fluctuations in commodity markets 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 poorly understood. Static partial equilibrium welfare analysis suggests that they introduce welfare-reducing price distortions, denying 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

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2011 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 exogenous to price differences and confirm 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 of zero effect on price differences, both for export bans and for changes in tariff rates. This result is robust to a variety of alternative specifications, including dropping potential segmented equilibria and adding second-order market pairs. I am able to reject hypotheses that export bans have an effect at least as large as the theoretical effect of a 5% export tax and that tariff rate changes have an implementation level of at least 38%.

I explore a number of potential explanations for my results. While there is evidence that the null result for tariff changes is due to imperfect implementation, I rule out both segmented pre-policy equilibria and non-implementation as explanations of the null result for export bans. Instead, I find suggestive evidence that export bans increase prices in both the origin and the destination country in their initial months, suggesting a role for hoarding and speculation consistent with a dynamic storage model with rational
expectations. As the ban lengthens, origin country prices eventually fall and the ensuing larger price differences result in the ban’s lifting. This qualitative description is broadly similar to the experience in Russia with export bans on wheat described in Welton 2011. My results suggest that export bans may have unintended consequences, increasing prices (and price volatility) at home rather than stabilizing them. Their use in the region and elsewhere in the world 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 of diesel, I find that the price difference per 100 kilograms increases by 35 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 4.1 contains the results of my basic specification and robustness checks, while section 4.2 explores alternative explanations of my results and develops a qualitative description of the dynamics of export bans. Section 5 concludes.

2 Context and Data

Maize is the staple food in 18 countries in East and Southern Africa with a combined population of 420 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.

Policy responses to maize price fluctuations in the region have focused on manipulating trade costs, the total costs involved in getting a product from one market to another. Trade costs in the region tend to be significant ex ante given the poor infrastructure, large formal and informal taxes and tariffs, and the 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. Figure 1 illustrates this wedge on a simple supply and demand graph for the case of a small open economy/market facing an external market price $p_w$ with importing trade costs $\tau_i$ and exporting trade costs $\tau_e$. Three sample demand curves are shown, each resulting in a different equilibrium trading regime. With demand curve 1, the country is an exporter in equilibrium, the equilibrium price is the export parity price $p_{ep}$, and the price difference with the external market price equals the exporting trade costs ($p_w - p_{ep} = \tau_e$). With demand curve 2, it is in a non-trading equilibrium with the equilibrium price between the export and import parity prices and the price difference less than the relevant trade costs ($p_{ip} - p_w < \tau_i$). With demand curve 3, it is an importer, the equilibrium price is the import parity price $p_{ip}$, and the price difference equals the importing trade costs ($p_{ip} - p_w = \tau_i$).

![Figure 1: The trade cost wedge](image)

Figure 2 illustrates how things change when the country faces either an external price spike (left graph) or a domestic harvest failure (right graph). In the case of the price spike, both import and export parity prices increase. Depending on the demand curve and the pre-shock trading regime, the equilibrium price can increase, and the country can shift from being an importer to an equilibrium with no trade to being an exporter. In the case of the domestic harvest failure, the domestic supply curve shifts in while the import and export parity prices are unaffected. Again, this can cause the equilibrium price to increase, but this time the country shifts in the opposite direction, from being an exporter towards an equilibrium with no trade or all the way to being an importer. In either case, the price difference with the external price changes only if the equilibrium trading regime changes or the initial equilibrium was non-trading.

Two major short-term policies are regularly implemented in East and Southern Africa in response to shocks. These policies include import and export subsidies, which can help stabilize prices and reduce the impact of trade costs. However, it is important to consider the long-term implications of such policies, as they may lead to a misallocation of resources and a reduction in efficiency. Additionally, policies that promote diversification and reduce dependence on a single export commodity can help mitigate the effects of trade shocks. 

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1 In reality, high trade costs mean that countries/markets in the region rarely face perfectly elastic import supply and export demand curves as shown here. However, the small open economy example is useful for illustrating simply the effects of trade costs and trade policies.
to these shocks. The first, export bans, increase the trade costs for exports (to infinity if they are fully enforced), lowering the export parity price but leaving the domestic supply curve and import parity price unchanged. The effect of a perfectly implemented export ban is shown for the trading partner price spike in the left graph of Figure 3, which in the case of the first two demand curves returns the price to or below the original pre-shock equilibrium and increases the price difference with the external price. The second, short-term tariff waivers, decrease the trade costs for imports, lowering the import parity price without affecting the domestic supply curve or the export parity price. The effect of a tariff waiver when tariffs are the only initial importing trade cost is shown for the domestic harvest failure in the right graph of Figure 3. All three demand curves result in the country importing in the post-policy equilibrium, and price differences with the external price have all been reduced to zero. 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. Mutambatsere et al. 2007, Van Campenhout 2007). However, analysis is usually limited to determining the extent to which prices in one market “cause” prices in another market and evaluating market efficiency and integration using highly-parameterized models. Most papers restrict their attention to markets within a given country. Some analysis of specific trade policies has been undertaken at an individual country level (e.g. Jayne et al. 2008 on Kenya). 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 pro-active versus hands-off policies towards maize markets). However, there has been little regional analysis of the mechanics of how 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 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 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 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 4. All markets have at least 2 links, with the average market having 3.5 links and the maximum links being 8 (Nairobi KE).
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-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 cents - more than 25% of the mean price of 26.7 US 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 10 year period on export bans and tariff rates (the two main policies that cause trade costs.
Table 1: Summary statistics for prices and price differences

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

Mean 0.267
Observations 7748

Mean 0.0729
Observations 12270

in the region to change over time\(^2\), 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 during this period. As shown in Table 2, the export bans range in duration from 4 to 54 months and affect 2119 link-months in my dataset.

Table 2: Export bans

<table>
<thead>
<tr>
<th>Country</th>
<th>Start Month</th>
<th>End Month</th>
<th>Months</th>
<th>Links</th>
<th>Link-Months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ethiopia</td>
<td>Jan-06</td>
<td>Jul-10</td>
<td>54</td>
<td>6</td>
<td>324</td>
</tr>
<tr>
<td>Ethiopia</td>
<td>Mar-11</td>
<td>Ongoing</td>
<td>9*</td>
<td>6</td>
<td>72</td>
</tr>
<tr>
<td>Kenya</td>
<td>Oct-08</td>
<td>Ongoing</td>
<td>38*</td>
<td>12</td>
<td>456</td>
</tr>
<tr>
<td>Malawi</td>
<td>Jul-05</td>
<td>Feb-07</td>
<td>19</td>
<td>5</td>
<td>95</td>
</tr>
<tr>
<td>Malawi</td>
<td>Apr-08</td>
<td>Jul-10</td>
<td>27</td>
<td>5</td>
<td>135</td>
</tr>
<tr>
<td>Malawi</td>
<td>Dec-11</td>
<td>Ongoing</td>
<td>0*</td>
<td>5</td>
<td>0</td>
</tr>
<tr>
<td>Tanzania</td>
<td>Jul-03</td>
<td>Jan-06</td>
<td>30</td>
<td>11</td>
<td>330</td>
</tr>
<tr>
<td>Tanzania</td>
<td>Aug-06</td>
<td>Dec-06</td>
<td>4</td>
<td>11</td>
<td>44</td>
</tr>
<tr>
<td>Tanzania</td>
<td>Jan-08</td>
<td>Oct-10</td>
<td>33</td>
<td>11</td>
<td>363</td>
</tr>
<tr>
<td>Tanzania</td>
<td>May-11</td>
<td>Oct-11</td>
<td>5</td>
<td>11</td>
<td>55</td>
</tr>
<tr>
<td>Zambia</td>
<td>Pre-2002</td>
<td>Jul-03</td>
<td>19*</td>
<td>5</td>
<td>95</td>
</tr>
<tr>
<td>Zambia</td>
<td>Mar-05</td>
<td>Jul-06</td>
<td>16</td>
<td>5</td>
<td>80</td>
</tr>
<tr>
<td>Zambia</td>
<td>May-08</td>
<td>Jul-09</td>
<td>14</td>
<td>5</td>
<td>70</td>
</tr>
</tbody>
</table>

2119

*overlaps with beginning or end of period.

Tariff rates are changed infrequently. Rate changes are typically either due to free trade agreements or short-term tariff waivers. Free trade agreements, including the Common Market for Eastern and Southern Africa (COMESA) Free Trade Agreement and the East African Community (EAC) customs union, have

\(^2\)In my structural model, I assume that all other unobserved policies affecting trade costs are constant over time and are therefore absorbed by my regression fixed effects. Export bans and tariff rates are certainly the primary policy instruments that have led to changes in trade costs during this period. Their large effects (e.g. abrupt reductions of tariff rates from 50% to 0%) are likely to dwarf those of any other policy changes, and I have not seen evidence to suggest that there are other policy changes that are correlated or coincide with export bans or changes in tariff rates.
been joined at different times by different countries, and joining an agreement has differential impacts on tariffs depending on whether the country of origin is also a member of the agreement. Tariff waivers also have differential impacts based on the pre-existing tariff that applied to the country of origin. I have identified the starting and ending dates of 5 tariff waivers implemented by 3 countries during the study period. These waivers range from 3 to 11 months in duration and affect 342 link-months in my dataset. Combining this information with free trade agreement implementation dates, base tariff rates on maize from country reports to the World Trade Organization, and tariff data obtained from other researchers, I have assembled a database of tariff rates for the countries in my dataset over the study period.

For fuel prices, I construct national monthly retail diesel price series for the 12 countries plus the breakaway republic of Somaliland by combining three sources of information. First, FEWS NET has partial monthly series for 7 of the 13 capital cities. Second, GTZ (the German technical cooperation) conducts a survey of capital city retail diesel prices once every two years. Third, I use the Dubai Fateh crude oil index, the most relevant for oil imports into East and Southern Africa. For periods not covered by the FEWS NET series, I calculate the markup in each country by comparing the GTZ retail diesel price with the Dubai Fateh index and then filling in the gaps between GTZ observations by inferring the markup using linear interpolation and then combining the Dubai Fateh price and the inferred markup. 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 national data series for all markets in the country, with the constant secondary markups to be captured by my regression fixed effects. The mean of the average country 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 average retail diesel 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...

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3For example, the EAC customs union was implemented between Kenya, Tanzania, and Uganda in January 2005, eliminating tariffs on maize between the three countries but raising their tariffs on maize from countries outside the customs union. Rwanda and Burundi subsequently joined the EAC in July 2009.

4The 5 waivers were implemented by Kenya (Jun-04 to Aug-04, Feb-09 to Dec-09, and Jun-11 to Ongoing), Tanzania (Jan-08 to May-08), and Zambia (Sep-05 to Mar-06).

5No tariff information was available for Somalia and South Sudan - their tariffs are assumed to be 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/

6To 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.
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 = 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.

| 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 5 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.
In the next section, I develop a simple structural model to illustrate how trade policies, fuel prices, and infrastructure quality affect trade costs and how trade costs in turn affect price differences.

3 Model

Trade policies, fuel prices, and infrastructure quality affect price differences (which I observe) indirectly through their direct effect on total trade costs (which are unobservable). My first modelling step will be to model how trade costs are determined by trade policies, fuel prices, infrastructure quality, and other factors. Then my second step will be to model how trade costs translate into price differences.

3.1 Step One: Trade Costs

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 total trade costs per unit of maize between the two markets at time \( t \) be \( \tau_{ijt} \) for trade from \( i \) to \( j \) and \( \tau_{jit} \) for trade from \( j \) to \( i \). Since there is significant anecdotal evidence from the region that export bans are never completely enforced (due to porous land borders), I model export bans as an increase (perhaps a very large increase) in trade costs and therefore assume that markets are never in complete autarky, i.e. \( 0 < \tau_{ijt} < \infty \forall i, j, t \).

I proceed to derive a detailed expression for the total trade costs \( \tau_{ijt} \). Although most terms will ultimately be absorbed into market pair fixed effects, the derivation is important to clarify the assumptions behind the ultimate fixed effect expressions. \( \tau_{ijt} \) will consist of the following seven components:

1) Wholesale-retail markups (local distribution costs). This includes storage, labor, local transport, store rent, local taxes, profit-taking in the case of local market power in the distribution sector, etc. In the case where I observe wholesale prices in \( i \) and \( j \), this component is 0. 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 the wholesale-retail markup in market \( i \) and \( m_j \) be the wholesale-retail markup in market \( j \). Then subtracting \( m_i \) from the retail price in market \( i \) gives the wholesale price in market \( i \) while adding \( m_j \) to the wholesale price on arrival in market \( j \) gives the retail price in market \( j \). Letting \( M_i \) be an indicator variable equalling 1 if the price data I have for market \( i \) is retail price data, the net effect of wholesale-retail markups on trade costs between retail markets \( i \) and \( j \) can be written as \(-M_im_i + M_jm_j\).

2) Truck rental rates. I assume that truck rental rates are symmetric for a given market pair (since traders may hire a truck based in either of the two markets). Rental rates \( r_{(ij)t} \) are determined by (i) the wear and tear the trip will put on the truck (determined by the distance between the markets \( D_{(ij)} \) and the road quality), (ii) the time the trip will take (determined by the distance, the road quality, the risk of breakdown, the average time of repair in case of breakdown \( T^K_{(ij)} \), and the average time spent at checkpoints \( T^{CP}(ij) \)), and (iii) the risk of losing the truck entirely (e.g. in an area of insecurity). I make the assumption that all these factors are constant for a given market pair \((ij)\) over the time period in my dataset except for the road quality \( s_{(ij)t} \), which may improve discretely with the completion of a new infrastructure project. The baseline road quality has 2 main components: 1) \( s^1_{(ij)t} \), the average time that a truck takes to travel one kilometer on the road, and 2) \( s^2_{(ij)t} \), the number of breakdowns per kilometer. For expositional simplicity, suppose there is just one infrastructure project between \( i \) and \( j \) completed at a specified time during the period of my data set with an indicator variable \( I_{(ij)t} \) equalling 1 in a time period where the project has already been completed and 0 otherwise. Let \( \Delta s^1_{(ij)} \) and \( \Delta s^2_{(ij)} \) represent the changes in the quality of the road after the project has been completed (new quality minus baseline quality). I proceed to write the following expression for the truck rental rate:

\[
r_{(ij)t} = (\delta^D_{(ij)} + C^K_{(ij)}s^2_{(ij)})D_{(ij)} + r^T_{(ij)}(D_{(ij)}s^1_{(ij)} + D_{(ij)}s^2_{(ij)}T^K_{(ij)}) + V_{(ij)}K_{(ij)}
\]

\[+ I_{(ij)t}[(C^K_{(ij)}\Delta s^2_{(ij)})D_{(ij)} + r^T_{(ij)}(D_{(ij)}\Delta s^1_{(ij)} + D_{(ij)}\Delta s^2_{(ij)}T^K_{(ij)})] \]

where the three terms on the first line correspond to the baseline values of (i)-(iii) above, \( \delta^D_{(ij)} \) is the depreciation rate per kilometer of the truck, \( C^K_{(ij)} \) is the cost per breakdown, \( r^T_{(ij)} \) is the time component of the truck rental rate based on the opportunity cost for using the truck for something else, \( V_{(ij)} \) is the average value of a truck used on the \( ij \) route, and \( K_{(ij)} \) is the probability of truck loss between \( i \) and \( j \).

3) Driver hire rates. As for truck rental rates, I assume that driver hire rates are symmetric for a given
market pair. Driver hire rates $w_{ijt}$ are based on the prevailing wage rates per unit time, $w_{ijt}$ (which I assume to be constant over time for a given market pair) and the time a trip takes (same as above):

$$w_{ijt} = w_{ijt}^T(D_{ij}\phi_{ij}^1 + D_{ij}^T K_{ij} + T_{ij}^{CP} + I_{ijt} w_{ijt}^T(D_{ij} \Delta s_{ij}^1 + D_{ij}^T \Delta s_{ij}^2 T_{ij}^K)$$

4) Fuel costs. Fuel costs $f_{ijt}$ for a truck will depend on the retail diesel price in market $i$ in period $t$, $g_{it}$, the average consumption in liters per kilometer, $\beta_g$, and the distance between markets. $f_{ijt} = \beta_g D_{ijt} g_{it}$.

5) Loading/unloading labor costs. Loading/unloading labor costs for a truck $l_{ijt}$ will be the sum of the prevailing rates in markets $i$ ($l_i$) and $j$ ($l_j$). $l_{ijt} = l_i + l_j$.

6) Taxes and bribes. Transport of maize is subject to official ad valorem international tariffs $t_{ijt}$ and unobserved taxes and bribes $t_{ij}^U$ (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 $\beta_X$. Total taxes and bribes per unit maize are therefore $\beta_X B_{ij} X_{it} + \beta_t B_{ij} t_{ijt} p_i + t_{ijt}^U$ 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).

7) Exchange costs. Consistent with the literature on the border effect (e.g. McCallum 1995), there may be additional costs associated with trade across borders, including currency exchange, the cost of cultural and linguistic barriers (e.g. the need to hire a translator or local agent), etc. Let these additional international trade costs per unit maize be $\beta_B$ (which will fit into the model as $\beta_B B_{ij}$).

Combining the costs from the seven components above and letting $\kappa$ be the fraction of a truck needed for a unit of maize, I obtain the following expression for total trade costs $\tau_{ijt}$:

$$\tau_{ijt} = [-M_i m_i + M_j m_j] + \kappa [r_{ijt} + w_{ijt} + f_{ijt} + l_{ijt}] + [\beta_X B_{ij} X_{it} + \beta_t B_{ij} t_{ijt} p_i + t_{ijt}^U] + \beta_B B_{ij}$$

Writing this expression out, rearranging terms, and adding directional market pair fixed effects $\phi_{ij}$, which absorb several terms at once:\footnote{Note that $\phi_{ij} \neq \phi_{ji}$ due to the addition and subtraction of the markup terms ($-M_i m_i + M_j m_j \neq -M_j m_j + M_i m_i$).}

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

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. This is the topic of the next section.

3.2 Step Two: From Trade Costs to Price Differences

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). In this section, I adapt the theoretical framework from the literature and suggest a simple inductive technique to help control for the possibility of segmented (non-traded) equilibria.

My starting point is a no-arbitrage condition. The competitive, zero-profit trading sector will bring maize from market \( j \) to market \( i \) at time \( t \) if \( \mathbb{E}(p_{it} - p_{jt}) \geq \tau_{jit} \) and will bring maize from market \( i \) to market \( j \) if \( \mathbb{E}(p_{it} - p_{jt}) \leq -\tau_{ijt} \). Competition ensures that the following no-arbitrage condition holds:

\[
\tau_{jit} \geq \mathbb{E}(p_{it} - p_{jt}) \geq -\tau_{ijt}
\]

Following Baulch (1997), Fackler and Goodwin (2001), and others, I identify three possible “regimes” based on the relative magnitude of actual observed price differences and the unobserved total trade costs:

1) In regime 1, \( p_{it} - p_{jt} = \tau_{jit} \) (case 1A) or \( p_{it} - p_{jt} = -\tau_{ijt} \) (case 1B), as was the case for demand curves 1 and 3 in Figure 1. The markets are in a competitive tradable equilibrium with no arbitrage opportunities. In case 1A maize is tradable from \( j \) to \( i \). In case 1B maize is tradable from \( i \) to \( j \).

2) In regime 2, \( \tau_{jit} > (p_{it} - p_{jt}) > -\tau_{ijt} \). 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\(^8\). Local prices are determined by local supply and demand as was the case for demand curve 2 in Figure 1.

3) In regime 3, \( p_{it} - p_{jt} > \tau_{jit} \) (case 3A) or \( p_{it} - p_{jt} < -\tau_{ijt} \) (case 3B). Here the markets are in disequilibrium following a shock in which the realized price difference is greater than the expected price difference. In case 3A there are arbitrage opportunities from market \( j \) to market \( i \); in case 3B there are arbitrage opportunities from market \( i \) to market \( j \). There may be an adjustment period back to a competitive

\(^8\)Notice that this runs counter to basic economic intuition that lower price differences are associated with trade and higher price differences with autarky. The key distinction here is that regime 2 is not autarky: trade is allowed but does not occur because traders would be operating at a loss.
The existence of these three possible regimes complicates my analysis of the effect of trade costs on price differences because the effect may be different according to the regime.

In regime 1, price differences have a one for one relation with trade costs. Assuming that the difference in retail markups is not greater than the other components of trade costs, a positive price difference \( p_{it} - p_{jt} \) indicates case 1A, in which \( p_{it} - p_{jt} \) increases one for one with an increase in \( \tau_{jit} \) but is not affected by a change in \( \tau_{ijt} \), whereas a negative price difference \( p_{it} - p_{jt} \) indicates case 1B, in which \( p_{it} - p_{jt} \) decreases (becomes more negative) one for one with an increase in \( \tau_{ijt} \) but is not affected by a change in \( \tau_{jit} \).

Of the other two regimes, regime 3 is the easiest to handle. I assume that any adjustment period is shorter than the one month time intervals in my price dataset. Consequently, I model regime 3 with a mean-zero normally distributed error term \( \epsilon \) 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 the following for cases 1A/3A and cases 1B/3B:

\[
p_{it} - p_{jt} = \tau_{jit} + \epsilon_{jit}
\]

\[
p_{it} - p_{jt} = -\tau_{ijt} - \epsilon_{ijt} \Rightarrow p_{jt} - p_{it} = \tau_{ijt} + \epsilon_{ijt}
\]

For each period \( t \) for each pair \( (ij) \), I now assign index \( j \) to the higher priced market and index \( i \) to the lower priced market and let \( \Delta p_{(ij)t} = p_{jt} - p_{it} \geq 0 \). If markets \( i \) and \( j \) are always in regimes 1/3 from my model, I could estimate the following basic specification based on equation (1) above:

\[
\Delta p_{(ij)t} = \tau_{(ij)t} + \epsilon_{(ij)t}
\]

\[
\Rightarrow \Delta p_{(ij)t} = \beta X B_{(ij)} X_{it} + \beta_{1} B_{(ij)} t_{jt} p_{i} + \beta_{2} D_{(ij)} g_{it} + \sum_{k} \beta_{k} I_{k(ij)t} + \phi_{(ij)} + \epsilon_{(ij)t}
\]

(2)

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 (2). In regime 2, price differences should be unaffected by changes in trade costs (unless such changes cause the market pair to enter one of the other regimes). To illustrate the complications this entails, suppose that market pair \( (ij) \) falls into regime 2 with probability \( \pi_{(ij)} \in [0, 1] \), which could be assumed to be an exogenous parameter but is more likely endogenously
determined by the trade costs, in which case it can be written \( \pi_{(ij)t}(\tau_{(ij)t}) \). Thus if I estimated equation (2) but some regime 2 observations were present in my dataset, my coefficient estimates might have some internal validity in a reduced form sense but would be conditional on the actual history of \( \pi_{(ij)t}(\tau_{(ij)t}) \)’s and therefore would lack external validity in that the coefficient estimates would be different given a different history\(^9\). The equation actually determining price differences is not (2) but rather:

\[
\Delta p_{(ij)t} = [1 - \pi_{(ij)t}(\tau_{(ij)t})] \left[ \tau_{(ij)t} + \epsilon_{(ij)t} \right] + \pi_{(ij)t}(\tau_{(ij)t}) \left[ E[\Delta p_{(ij)t} | \Delta p_{(ij)t} < (\tau_{(ij)t} + \epsilon_{(ij)t})] \right]
\]

Equation (3) 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 expectations term for the price differences in segmented equilibrium and are likely to also affect 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, developed by Sexton et al. 1991 and extended by Baulch 1997, 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 accommodate changes in trade costs over time.

In light of these shortcomings, I adopt a more inductive approach in the spirit of Fackler and Goodwin 2001 and more recent papers such as Moser et al. 2009. First, I observe that if no observations fell under regime 2 then \( \pi_{(ij)t} = 0 \ \forall (ij)t \), the second term of equation (3) would be 0, and equation (2) would be the correct specification. Now suppose that there are regime 2 observations and that they could be identified. Estimating equation (2) without the regime 2 observations would therefore give the “correct” structural

\(^9\)For example, during a period of high gas prices two markets might be in regime 2 in which case export bans might have no effect, whereas during a period of low gas prices they would have a significant effect. While equation (2) gives a reduced form estimate of the actual effect of a particular export ban on price differences under particular circumstances, equation (3) gives an estimate of the structural parameters, including the effect of an export ban conditional on being in a tradable equilibrium, which is relevant for predicting the effect of a similar ban in other circumstances.
coefficients corresponding to equation (1), while estimating it with all observations would give coefficient estimates 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 (2) as my basic specification but experiment with different ways of eliminating potential regime 2 data points from my data set and seeing how my results are affected. First, I use estimates of minimum feasible transportation costs in the region to establish a putative regime 2 price differences threshold for each market pair. I try eliminating data points with price differences below this threshold and experiment with increasing and decreasing the threshold. Second, I consider the case where no estimates of minimum feasible transportation costs are available. I try using a common threshold across markets and varying that threshold, comparing the results both to my baseline result and the results using the estimated minimum feasible transportation cost. For the case of my data, these techniques enable me to adequately control for the possibility of regime 2 and establish robust coefficient estimates. In other cases, they could at the very least be used to establish tighter bounds on the coefficient estimates. Given the importance of the policy issues (such as the ones considered in this paper) 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 may bias results in less transparent ways.

4 Empirical Results

4.1 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, gas prices, or infrastructure project completion. Gas prices, which are largely determined by prices in the international markets supplying the region, are clearly not affected by maize price differences. While infrastructure projects and trade agreements may be more likely to be put in place between markets with larger \textit{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 trade 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, price differences would only be affected if market pairs shifted from one regime to another as discussed earlier in the context of Figure 2.

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 et al. 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:

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

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>
<thead>
<tr>
<th>Table 4: Test of exogeneity of trade policies</th>
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<tbody>
<tr>
<td>(1)</td>
</tr>
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<td>1 month prior</td>
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<td></td>
</tr>
<tr>
<td>2 months prior</td>
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<td>4 months prior</td>
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</tr>
<tr>
<td>Policy</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>Links</td>
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</tbody>
</table>

Dependent variable: price differences. Absolute value of t statistics in parentheses, * significant at 10%; ** at 5%; *** at 1%. Robust standard errors clustered at link level.

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 equilibrium to a regime 1 importing equilibrium, as was the case for demand curve 2 in the right panel of Figure 2.10 A price spike in global markets, for instance, might cause prices in both origin and destination countries/markets to rise but should not affect the trade costs between them.
Figure 2. In contrast, export bans are typically implemented in countries that are always in a regime 1 exporting equilibrium in response to international price spikes, as was the case for demand curve 1 in the left panel of Figure 2. Thus these results are consistent with the theoretical hypothesis that trade policies are endogenous to price differences only if they are triggered by an event associated with a trading regime change, which does not appear to be the case for the export bans in my dataset. 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 (2):

\[
\Delta p_{(ij)t} = \beta X B_{(ij)} X_{it} + \beta_t B_{(ij)} t t p_t + \beta_g D_{(ij)} Y_{gt} + \sum_k \beta_k I_k_{(ij)} t + \phi_{(ij)} t + \epsilon_{(ij)1}
\]

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 \(E[\epsilon_{(ij)1}, \epsilon_{(ij)2}] \neq 0\). 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: \(E[\epsilon_{(ij)t}, \epsilon_{(ik)t}]) \neq 0\) and \(E[\epsilon_{(ij)t}, \epsilon_{(kj)t}] \neq 0\). Fafchamps and Gubert 2007 have derived formulas for consistent standard errors in dyadic regressions, but their technique is computationally intensive and cannot be directly 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 due to trade agreements leads me to fail to reject the null hypothesis that tariff changes have no effect on price differences.
Table 5: Basic specification

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<td>-0.0035</td>
<td>-0.00352</td>
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<td>0.0670</td>
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<td>(2.12)**</td>
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<td>0.0000356</td>
<td>0.0000356</td>
<td>0.0000340</td>
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<tr>
<td></td>
<td>(3.63)***</td>
<td>(3.77)***</td>
<td>(4.34)***</td>
<td>(3.78)***</td>
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</table>

Infrastructure | Yes | Yes | Yes | No |
Observations   | 12104| 11855| 11855| 11855|
Clustered errors | Origin & Destination | Link | Origin & Destination |
Clusters       | 79 & 79 | 79 & 79 | 139 | 79 & 79 |

Dependent variable: price differences. Absolute value of t statistics in parentheses, *significant at 10%; ** at 5%; *** at 1%. Robust standard errors clustered as indicated.

Using the standard errors from my preferred specification, there are some hypotheses that I can reject. Given the mean maize price of 26.7 cents from Table 1, for export bans to have an effect at least as great as that of a 5% export tax they would need to increase price differences by $0.05 \times 0.267 = 0.0134$ (1.34 cents). Using the standard error of 0.0126, I can reject this hypothesis at a 10% significance level. For tariffs, I can reject a hypothesis that a change of tariff rates of 1 percent results in an increase in price differences of more than 0.38 percent at a standard 5% significance level.

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 0.35 cents per kilogram (35 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 3.1. 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:

\[
k \Delta f_{(ij)t} = k \beta_g D_{(ij)} \Delta g_{it} = 0.0001 \times 0.4 \times 100 \times 1 = 0.004 = 0.4 \text{ cents}
\]

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

---

11 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. Thus the benchmark used here should be interpreted as the theoretical effect of a permanent 5% export tax.
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. 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

<table>
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<td>(1.85)**</td>
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<td>(3.89)***</td>
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<tr>
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Dependent variable: price differences. Absolute value of t statistics in parentheses, *significant at 10%; ** at 5%; *** at 1%. Robust standard errors clustered at origin and destination levels.

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,

\[12\] Similar results were obtained using year or month indicator variables, with or without the time trend.
but their exclusion does not affect my results. It does, however, reduce the standard error on my point estimate for export bans significantly (to 0.00570). This enables me to reject the hypothesis that the effect of export bans is as large as the theoretical effect of a 5% export tax at a 2% significance level.

In column (5) of Table 6, I explore whether the unbalancedness of the panel is affecting my results by trimming my dataset. I first exclude all data points before January 2005, reducing my dataset to 7 years. Then I exclude 10 markets (and the 29 market pairs of which they are members) with more than 12 months of missing data since January 2005. With these adjustments, of the 5796 possible price observations in my new panel, only 2.7% (212) 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 results.

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 to test 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 my estimates 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 Section 3.2. 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 my estimates. 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 downward bias due to regime 2 observations.

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.  

In the next section, I evaluate possible explanations of my surprising results and develop a qualitative

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13The 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.
description of what takes place during an export ban.

4.2 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. This allows me to rule out permanent segmented equilibrium as a possible explanation.

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 et al. 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 20-30% tariffs imposed by Kenya during their study period (1989-2004) had only a 4-5% effect on maize prices. In this environment of imperfect enforcement, changes in official tariff rates might not translate into changes in trade costs.

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. In addition, in contrast to tariffs, since export bans are all-or-none policies, even if border officials let some maize through during a ban in exchange for bribes they would be unlikely to collect an equivalent bribe in the absence of a ban, so the ban 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

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14For example, in 2005-2006, FEWS NET monitored crossborder maize flows averaging 15,000 metric tons (MT) per month at 24 border points between Malawi, Mozambique, Tanzania, Zambia, and Zimbabwe; in 2011, it monitored 9,000 MT per month at 26 border points between Burundi, Ethiopia, Kenya, Rwanda, Somalia, South Sudan, Tanzania, and Uganda.

15For 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 payments the trader makes at the border.
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:

$$SD_{(ij)X} = \alpha + \beta X + \phi_{(ij)} + \epsilon_{(ij)X}$$  \hspace{1cm} (5)$$

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 for lack of space) indicate that the standard deviation of price differences is 29% 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 50% and 48% higher in export ban periods than in non-ban periods (significant at 1% level). This increased variability may have a number of explanations (such as increased regime 2 frequency during export bans), and the results cannot be interpreted causally unless export bans are exogenous to volatility, which cannot be easily tested. However, the correlation between export bans and volatility at the very least suggests that export bans are in fact being implemented.

<table>
<thead>
<tr>
<th>Table 8: Price regressions</th>
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<tr>
<td></td>
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<tr>
<td>Export ban</td>
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<tr>
<td></td>
</tr>
<tr>
<td>Gas price</td>
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<tr>
<td></td>
</tr>
<tr>
<td>Dependent variable</td>
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<td>Observations</td>
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Absolute value of $t$ statistics in parentheses, *significant at 10%; ** at 5%; *** at 1%.
Robust standard errors clustered at market level.

To further clarify what happens during bans, I run suggestive 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:

$$p_{mt} = \beta_1 B_{(ij)X}X_{it} + \beta_2 g_{it} + \sum_k \beta_k I_{k(ij)X} + \phi_m + \epsilon_{mt}$$  \hspace{1cm} (6)$$

for $m = i$ and $m = j$. Export bans are undoubtedly endogenous to prices, so 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.

Finally, I undertake some simple graphical analysis to help visualize what goes on during export bans. In Figure 6, I combine data from all 27 marketing links affected by the 8 export bans for which I have complete start-to-finish data in my dataset to plot the average price in the origin market and the average price in the destination market for 3 months before and 3 months after the beginning (left panel) and end (right panel) of an export ban.

![Figure 6: Evolution of prices at the beginning (left) and end (right) of export bans](image)

The results are qualitatively striking. In the left graph, there is a marked discontinuity at the beginning of an export ban, with both origin and destination market prices increasing sharply and no significant change in the difference between the two. Comparing the two graphs, it appears that over the course of the ban the destination market price stays high but that by the time the ban is lifted the origin price has fallen significantly and the price difference has widened. Given my basic result that export bans do not have a statistically significant effect on price differences, this fall in the origin price likely happens near the end of the ban. To confirm this intuition, I re-estimate equation (4) 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 shown in column (1) of Table 9 confirm that price differences are significantly larger in the final few months of export bans than their average during the bans. In column (2), I rerun this regression with all data periods for the market pairs affected by the 8 export bans with start-to-finish data, using indicator variables for the first, second, third, and fourth quarters of the bans instead of the month indicator variables\(^{16}\). The

\(^{16}\)I use this quarter-based approach rather than a time trend since the bans vary greatly in length (from 4 to 54 months).
results confirm that there is a significant increase in price differences only in the final quarter of export bans.

Table 9: Price differences at the end of export bans

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<td>1 month before</td>
<td>0.0385</td>
<td>0.0071</td>
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<tr>
<td></td>
<td>(4.10)***</td>
<td>(0.46)</td>
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<tr>
<td>2 months before</td>
<td>0.0443</td>
<td>0.0195</td>
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<tr>
<td></td>
<td>(4.80)***</td>
<td>(0.99)</td>
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<tr>
<td>3 months before</td>
<td>0.0376</td>
<td>0.0136</td>
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<tr>
<td></td>
<td>(4.38)***</td>
<td>(0.85)</td>
</tr>
<tr>
<td>4 months before</td>
<td>0.0390</td>
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<tr>
<td></td>
<td>(5.47)***</td>
<td>(3.50)***</td>
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</tbody>
</table>

Observations 1668 Observations 3335

Dependent variable: price differences. Absolute value of t statistics in parentheses, *significant at 10%; ** at 5%; *** at 1%. Robust standard errors clustered at link level.

Taken together, the suggestive results in this section allow me to assemble a qualitative description of what happens during export bans that is consistent with anecdotal evidence from the region. 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 imports must be sourced from alternative, more costly sources. That origin country prices rise in tandem is consistent with optimizing behavior by traders in a rational expectations storage model (Gustafson 1958, Wright 2001). The increase in the destination country price causes a rise in the origin country traders’ expected future prices, since when the ban is lifted they will be able to sell maize to the destination country at a high price. This causes traders to increase stocks of maize, decreasing the supply in origin country markets and thereby increasing the domestic price. With rising domestic prices, governments keep the ban in place. Eventually, stocks become so large and continued storage so costly that traders start selling more maize and the domestic price begins to fall. 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 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 consistent with 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, to the consternation of Russian officials, 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.

5 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 dynamic description of what actually takes place during export bans 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, origin country 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. Markets remain integrated even when bans are in place. 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 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.

References


