Re-estimating the effect of heterogeneous standards on trade: endogeneity matters

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Abstract

Controlling for endogeneity-induced biases and accounting for the source of heterogeneity may both matter for the proper empirical estimation of the effect of heterogeneous standards on trade. However, existing literature on the trade effects of heterogeneity in pesticides Maximum Residue Levels (MRLs) does not directly address the problem of endogeneity in the standards-trade relationship. Using pesticides MRL data for 53 countries over 2005-2014, we thus re-examine the trade effects of stricter (than partner) standards accounting for endogeneity using panel data methods. We find that the direction of the estimated trade effects gets reversed, thereby underlining the greater demand-enhancing effect of more stringent regulation. We also discuss why endogeneity may bias the estimates downwards. In another original contribution, we examine the standards-trade relationship by the direction of imposition of stricter standards for a large panel. Our results suggest that stricter standards do not impede trade, irrespective of who imposes them.

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1 Introduction

The continual decline of tariffs through successive rounds of multilateral trade negotiations has increased the relative importance of non-tariff measures (NTMs). Sanitary and Phytosanitary (SPS) standards and technical barriers to trade (TBT) are two such NTMs, which though imposed for legitimate reasons such as alleviating information asymmetries, mitigating consumption risks and promoting environmental sustainability, can also be instruments of disguised protectionism.

Literature suggests that SPS and TBT measures can have both demand-enhancing and trade cost effects (Xiong and Beghin, 2014). Standards prescribe requirements for product characteristics, production processes and/or conformity assessment to address information problems, market failure externalities and societal concerns, which may assuage consumer concerns in importing countries. Standards can also convey positive information on product quality, again enhancing demand. Existing literature has documented the positive effects of standards on trade (Jaffee and Henson, 2004; Henson and Jaffee, 2008; Nimenya et al., 2012; Xiong and Beghin, 2013; Ishaq et al., 2016).

However, country-specific standards effectively create additional costs for foreign producers by forcing them to adjust their product and production process so as to meet individual national standards. Further costs emanate from the need for subsequent conformity assessment with these standards (for instance see Baldwin et al., 2000; Wilson and Otsuki, 2004; Chen et al., 2006; Chen and Mattoo, 2008) and inspection/testing at customs that lead to prolonged delivery times and even outright consignment rejection/entry denial (Xiong and Beghin, 2014). Other literature documenting the negative effects of standards on trade includes Otsuki et al., 2001; Disdier et al., 2008; Swinnen and Vandemoortele, 2011.

A commonly used standard in agricultural products restricts the maximum residue level (MRL) from pesticides. A pesticide residue is a tiny trace of pesticide that sometimes remains on the treated crop. An MRL is the maximum amount of residue legally permitted on food products. Once residues are demonstrated to be safe for consumption, MRLs are set by scientists, based on rigorous evaluation of each legally authorized pesticide. Countries choose the products they regulate, the pesticides they regulate for each product, as well as the MRL for a given product-pesticide pair.

The impact of differences in MRL regulation on trade has been widely studied in the standards literature, though without finding a consensus.[1] One of the possible reasons for the lack of consensus could be shortcomings in estimation. For instance, in their meta-analysis

[1]The following section provides a review of the relevant literature.

of estimates of the impact of TBT, Li and Beghin (2012) note that studies using MRLs do not directly address the problem of endogeneity in the standards-trade relationship. Empirically, this endogeneity could emanate from reverse causality, omitted variables and/or simultaneity (Wooldridge, 2010) and may result in the error term being correlated with the explanatory variables, leading to biased and inconsistent estimates of the impact of heterogeneous standards on trade. At the same time, accounting for the source of heterogeneity may also matter for the proper estimation of the effect of heterogeneous standards on trade.

In this paper, we therefore re-examine the effect of heterogeneity in MRL[2] regulation on bilateral trade using the Homologa data[3] on pesticide MRLs over 2005-2014 for 53 exporting and importing countries (details in Section 5). In doing so, we make two original empirical contributions to the impact assessment of standards literature.

To the best of our knowledge, we are the first to account for endogeneity in the standards-trade relationship using a random growth first difference model (RGFD; for instance see Baier and Bergstrand, 2007; Baier et al., 2014) with three-way fixed effects.[4] We regard this to be a significant contribution because the recent standards literature confirms endogeneity in the MRLs-trade relationship (for instance see Li et al., 2017) and accounting for endogeneity like we do reverses the direction of the estimated trade effects of heterogeneous standards.

In contrast, the existing impact assessment of standards literature either fails to account for endogeneity in the standards-trade relationship (Jongwanich, 2009; Engler et al., 2012; Drogué and DeMaria, 2012; Winchester et al., 2012; Xiong and Beghin, 2013; Ishaq et al., 2016) or does so using instrumental variables on cross-sectional survey data (Hansen and Trifković, 2014; Melo et al., 2014). Xiong & Beghin (2014) regard potential biases from endogeneity in their analyses to be limited while Ferro et al. (2015) do not include all appropriate fixed effects in their estimations.

Distinct from these approaches and following Baier et al. (2014), we estimate our empirical model in first differences including three-way fixed effects; this approach neutralizes reverse causality, omitted variables and simultaneity biases in the standards-trade relationship, be-

[2]We focus on MRLs to exploit the richness of our self-assembled database and because MRLs provide a continuous measure of relative stringency and are thus preferred in this literature (for instance see Melo et al., 2014) over the use of count or frequency measures.

[3]Drogué and DeMaria (2012), Xiong and Beghin (2014) and Ferro et al. (2015) also use the same base data obtained from Agrobase-Logigram, a private company that maintains Homologa, the Gobal Crop Protection Database, though there are significant differences in the underlying sample in each case. For instance Drogué and DeMaria (2012) only focus on apples and pears, while Xiong and Beghin (2014) only consider a cross-section of the Homologa data for the year 2008. We also have a longer panel than in Ferro et al. (2015).

[4]The use of Baier and Bergstrand's (2007) panel data approach to address endogeneity was also suggested by Li and Beghin (2012) in their meta-analysis.

sides controlling for sample selection and firm heterogeneity biases. We prefer this estimation strategy to using IV techniques to account for endogeneity as the former obviates the need to find valid instruments for endogenous variables, which is always a challenge, arguably more so in the context of this study. Significantly, our estimation strategy also yields strong, robust results.

In our second empirical contribution, we construct two indices of regulatory heterogeneity, which departing from existing literature, also examine the effect of heterogeneity on trade when the exporting country is bound to stricter regulation at home than in the destination market for a large sample of trading partners and years. As Xiong and Beghin (2013) point out, the direction of the own-export effect of a domestic standard depends on whether the domestic industry has a comparative advantage in meeting regulations and on whether consumer preferences in the importing country are pro-food safety.

However, studies analyzing the effect of SPS/TBT measures on trade either assume no effect from regulatory dissimilarity when the exporter is stricter (Burnquist et al., 2011; Xiong and Beghin, 2014; Ferro et al., 2015) or that all regulation heterogeneity leads to compliance costs for the exporter in the destination market, whether or not regulations are stricter in the exporter market (Achterbosch et al., 2009; Drogué and DeMaria, 2012; Winchester et al., 2012). The sole exception is Xiong and Beghin (2013) who study the effect of both relative importer and exporter stringency in the same estimating equation, but they only study effects on US and Canadian trade for one year, 2010.

Our results for a larger set of trading partners and years suggest that once endogeneity-induced biases in the standards-trade relationship are alleviated, stricter standards facilitate trade irrespective of who imposes them, thereby underlining the greater demand-enhancing effect of more stringent regulation. This result is in contrast to most other findings on the trade effects of pesticides MRLs in this literature, and can be explained as follows.

Relative dyadic stringency in standards can not only increase bilateral trade costs in a structural gravity framework (see Section 6 for details) but the information conveyed by more stringent regulation can also enhance consumer demand in the importing country (for instance see Xiong and Beghin, 2014). The RGFD estimates suggest that the bilateral trade costs are lowered or consumer demand enhanced, which results in a positive effect on exports. This can be further explained as follows. If consumers are more pro-food safety in the importing country and are well-informed about food standards, then more stringent importer regulation still generates a demand-enhancing effect (despite any higher prices from costs of meeting stricter importing country standards). Similarly, if exporting country firms already comply with stricter standards at home, it is less costly for them to meet importing country standards, again resulting in a positive effect on trade, which may be further accentuated by well-informed, pro-food safety importing country consumers.

The rest of the paper is structured as follows. The following section reviews some of the relevant literature. Section 3 discusses why endogeneity may arise in the standards-trade relationship and why endogeneity-induced biases may underestimate the trade effects of heterogeneous standards. Section 4 describes the measures of relative dyadic MRL stringency we construct to examine trade effects by source of heterogeneity. Section 5 looks at the data while Section 6 presents the empirical methodology. The main results are discussed in Section 7 along with sensitivity analysis in Section 8. Section 9 concludes.

2 Literature review[5]

Most work on the link between harmonization of standards and trade is empirical in nature and theoretical literature on this subject remains scant. Ganslandt and Markusen (2001) have modeled TBTs formally (though not their liberalization). Baldwin et al. (2000) and Chen and Mattoo (2008) have modeled both TBTs and their harmonization, cautioning against the discriminatory effects that the latter may entail. More recently, Xiong and Beghin (2014) have disentangled the demand-enhancing and trade cost effects of MRL regulation in a generalized gravity model setting. In what follows, we provide a select review of empirical studies assessing the trade effects of pesticides MRLs. In sum, this empirical literature finds regulatory harmonization of MRLs to be trade-facilitating and differences in MRL regulation to be trade-restrictive.[6]

Achterbosch et al. (2009) studied the impact of differences in pesticide MRLs on Chilean fruits exports to the EU15 over 1996-2007 and found a 5% reduction in the EU's regulatory tolerance levels for MRLs to lead to a 14.8% decline in export volumes, with grapes being twice as sensitive as the other fruits. Melo et al. (2014) examined regulatory harmonization in a range of SPS and quality (SPSQ) measures (including MRLs) on Chilean fresh fruit exports in 16 destination markets based on the number of regulations and exporters perception of the stringency of SPSQ measures over 2005-09. They find a rise in stringency to have

[5] Existing literature explicitly models MRLs/food standards in a political economy framework (for instance see Swinnen and Vandemoortele, 2011; Swinnen, 2016 and Li et al., 2017). There is more of a consensus in that literature to allow us to abstract from the determinants of MRLs and focus on their effects in this review.

[6] One recent exception to this is Ishaq et al. (2016), who use the Li & Beghin (2014) stringency indices to examine the effect of stricter (than Codex) importing country standards on Chinese exports over 2012-2013 and find this effect to be positive.

substantial negative effects on export volumes, with different dimensions of SPSQ measures having different effects on trade.

In different ways, papers similar to ours are Winchester et al. (2012), Drogué and DeMaria (2012), Xiong and Beghin (2013), Xiong and Beghin (2014) and Ferro et al. (2015). However, in addition to differences in sample and measures of regulatory heterogeneity used, with the exception of Xiong and Beghin (2013), none of these papers examines the trade effects of more stringent standards by the direction of their imposition.

Winchester et al. (2012) study the impact of regulatory heterogeneity on the EU's agri-food export intensity in the year 2009-10 by using the NTM-Impact database that was assembled under a European research framework programme. Their results indicate that differences in most regulations weakly reduce trade, but that stricter MRLs for plant products in one country relative to others reduces exports to that country.

Drogué and DeMaria (2012) construct an index of regulatory heterogeneity in MRLs following that in Vigani et al. (2010) to examine its effect on bilateral export intensity of fresh and processed apples and pears among 40 trading partners over 2000-09. They also find regulatory differences to hinder trade.

Xiong and Beghin (2013) use MRL data from the US Department of Agriculture to construct heterogeneity indices relative to Codex MRLs (following Li and Beghin, 2014) and examine the effects of relative importer and exporter stringency for US and Canadian trade in 2010. They find US exports to be adversely affected by relative importer stringency but Canadian exports to benefit from more stringent MRL regulation, irrespective of the source of such stringency. However, regulatory heterogeneity does not affect US or Canadian imports in their results.

Xiong and Beghin (2014) disentangle the cost-escalating and demand-enhancing effects of MRLs in 20 OECD importing countries in 2007-08 and 2011-12 at both margins of trade using the Li and Beghin (2014) stringency index. They find MRLs to jointly enhance import demand and hinder foreign export supply.

Ferro et al. (2015) use the same base data on pesticide MRLs as ours to study the effects of standards restrictiveness on agri-exports in importing countries over 2006-11. The authors find more restrictive standards in the destination market to adversely affect the probability of exporting, but unlike this study, they do not consider the case where the exporting country has more stringent regulation, assuming this to have no impact on importing country decisions.

With the exception of Xiong and Beghin (2013), all studies reviewed in this section conclude that relative MRL stringency hinders trade. But none of these studies accounts for endogeneity in the standards-trade relationship using the panel data methods we employ. As we show below, a significant outcome of accounting for endogeneity like we do is a reversal in the direction of the estimated trade effects.

3 Endogeneity in the standards-trade relationship

Empirically, endogeneity in the standards-trade relationship could emanate from reverse causality, omitted variables and/or simultaneity (Wooldridge, 2010) and may result in the error term being correlated with the explanatory variables, leading to biased and inconsistent estimates of the impact of heterogeneous standards on trade. We discuss these reasons for endogeneity below.

3.1 Reverse causality

Existing literature on the determinants of MRLs suggests that the level and stringency of MRLs may not depend solely on scientific and health concerns regarding the pesticide but also on economic and political determinants thereby leading to reverse causality in the standards-trade relationship. For instance, Li (2012) found tariffs and MRL stringency to be jointly determined alluding to the role of political decision-making in the design or implementation of MRL policies. In fact, since one of the determinants of MRLs is consumption, it is possible that countries set stricter standards on products that are more consumed and imported. Consistent with this argument, Li et al. (2017) show in a sample of 53 OECD and non-OECD countries including LDCs, that tariffs and MRLs are used as substitutes by policy-makers. In fact, with an elasticity of -5.45 at the mean of the dataset, the response of MRLs to variation in tariffs in their results was found to be the strongest amongst all political economy determinants. Their finding suggests that policy makers may adopt stringent standards to shield domestic industries from international competition in import-intensive sectors as the use of import tariffs is constrained and reduced by various trade agreements.

Analogously, exporting countries may deploy more stringent standards for products that are more likely to be exported, especially in cases where exporting firms have a comparative advantage in meeting such regulation and where such exports are destined for more pro-food safety markets (for instance see Xiong and Beghin, 2013). As the authors point out, two rationales justify this choice: one, meeting more stringent regulation at home saves exporters rejections at the border of the destination market (an extensive margin effect) and two, lower tolerance levels at home enhance exporter reputation abroad where food-safety may pay off in terms of higher premium or repeated purchase (an intensive margin effect).

3.2 Omitted variable

Li et al. (2017) show that MRL stringency is positively correlated with income per capita and population of the importing country. Both these factors are also important determinants of bilateral trade, suggesting that the standards-trade relationship is not exogenous. Thus, in terms of observable economic characteristics, countries with stricter standards also tend to have economic characteristics associated with more trade.

However, the error term may also include unobservable factors that tend to reduce bilateral trade and remain unaccounted for by standard gravity covariates but may be correlated with the decision to impose a more stringent standard. One likely omitted variable in the context of this study is consumer preferences. Consumers in an importing country with stricter standards are less likely to consume imported products (causing the error term to be negative). Such consumers are also more likely to lobby for more stringent regulation, yielding a positive correlation between consumer preferences and stricter standards, though the error term and consumer preferences may be negatively correlated. This suggests that relative importer stringency and the error term would be negatively correlated, and the coefficient of relative importer stringency will tend to be underestimated.

Similarly in an exporting country with stricter standards, consumers are more pro-food safety and most domestic production is likely to cater to these domestic preferences (causing the error term to be negative). Such consumers are also more likely to lobby for more stringent regulation, yielding a positive correlation between consumer preferences and stricter standards, though the error term and consumer preferences may be negatively correlated. This suggests that relative exporter stringency and the error term would be negatively correlated, and the coefficient of relative exporter stringency will also tend to be underestimated.

3.3 Simultaneity

Holding standard gravity covariates constant, two countries that trade more than their "natural" level as predicted by a typical gravity equation, may be induced to "lower" or harmonize standards, for instance in recognition of their long-established trading relationship. This would cause a negative simultaneity bias in the estimated relative stringency coefficients. An illustration of this is provided in existing literature via the "protection for sale" argument

(Grossman and Helpman, 1994) wherein import penetration and protectionism are inversely related. More recently, Li et al. (2017) also show that countries adopt less stringent MRLs in sectors where domestic producers are more competitive in the world market.

In sum, endogeneity in the standards-trade relationship clearly matters and the underlying biases are likely to underestimate the effect of heterogeneous standards on trade. In fact, Li and Beghin (2012) also point out that both Trefler (1993) and Lee and Swagel (1997) have shown that endogeneity could lead to an underestimation of NTMs' trade effects. In the empirical analysis that follows we thus control for (i) omitted variable biases by including time-varying importer-product and exporter-product fixed effects in estimation and (ii) the simultaneity bias by including dyadic fixed effects. The exogeneity test of our RGFD estimates (details in Section 8.1 below) provides evidence that our preferred estimation methodology also controls for reverse causality in the estimating equation.

4 The source of heterogeneity may matter in estimating the trade effect of heterogeneous standards

In addition to controlling for endogeneity-induced biases, accounting for the source of heterogeneity may also matter for the proper estimation of the effect of heterogeneous standards on trade. As Xiong and Beghin (2013) point out, the direction of the own-export effect of a domestic standard depends on whether the domestic industry has a comparative advantage in meeting regulations and on whether consumer preferences in the importing country are pro-food safety.

However, with the exception of Xiong and Beghin (2013), studies analyzing the trade effect of standards either assume no effect from regulatory dissimilarity when the exporter is stricter (Burnquist et al., 2011; Xiong and Beghin, 2014; Ferro et al., 2015) or that all regulation heterogeneity leads to compliance costs for the exporter in the destination market, whether or not regulations are stricter in the exporter market (Achterbosch et al., 2009; Drogué and DeMaria, 2012; Winchester et al., 2012).

These assumptions are clear from the quotations below:

"...if the MRL level is stricter in the exporter country than in the importer country, then the latter's MRL standard should have no effect on its imports from the exporter country." (Ferro et al., 2015, p.74)

"...exporters who have been subject to tougher MRLs in their domestic markets...are less likely to experience trade disruption..." (Xiong and Beghin, 2014, p.1193)

Another important motivation for our research is thus to empirically examine the hypotheses inherent in these quotes. Literature suggests that relative importer stringency may affect both fixed and variable costs (for instance see Ferro et al., 2015 and Xiong and Beghin, 2014, respectively). In contrast, a positive trade effect of stricter importing country standards could be due to an increased demand in the destination market emanating from the positive signaling effect of more stringent regulation, or due to more efficient and productive techniques used in markets where regulations are stricter (for instance see Blind and Jungmittag, 2005; Xiong and Beghin, 2013).

However, meeting stricter standards also involves higher costs which may get passed-through to consumers as higher prices. This said, the trade cost effects of more stringent domestic regulation are likely to be low if domestic industry has a comparative advantage in meeting such regulation (Xiong and Beghin, 2013). Moreover, especially in the case of pesticide MRLs, consumer preferences in the importing country may be less or more pro-food safety and this would also have a bearing on which effect would prevail in the end.

For all of these reasons, we consider it useful to examine the potential asymmetric impact of regulatory heterogeneity not just on exports destined for more stringent markets but even on exports coming from more stringent countries for a larger panel of trading partners and years than in Xiong and Beghin (2013).

To do so, we construct a modified version of the Achterbosch et al. (2009) heterogeneity index by taking the absolute value of the difference in MRL regulation between countries in a trading dyad and normalizing it by the sum of the levels of MRL regulation in that dyad.[7] Formally, we have:

$$r_{ijpkt} = \frac{abs(MRL_{ipkt} - MRL_{jpkt})}{MRL_{ipkt} + MRL_{jpkt}} \tag{1}$$

where MRL_{ipkt} is the maximum residue level of pesticide k allowed by the exporter i to remain on product p and MRL_{jpkt} is the maximum residue level of pesticide k allowed by the importer j to remain on product p.[8] Thus, the index, r, measures the degree of heterogeneity of MRL regulation between exporter i and importer j, regarding the maximum residue level

[7]Note that unlike the Li and Beghin (2014) index that measures MRL stringency of the importer relative to the Codex and is convex in protectionism, the Achterbosch et al. (2009) index (as well as the Winchester et al. (2012) index that we use in our sensitivity analyses) simply linearizes the difference in MRL regulation within a dyad. However, convexity in protectionism is a requirement in an index that is constructed relative to the Codex, which is an international standard, not in an index which measures stringency relative to the standard set by another country.

[8]Thus, the MRL_{ipkt} and MRL_{jpkt} are non-negative variables, that are theoretically unbounded but bounded from above in practice.

of pesticide k allowed to remain on product p. The value of the index ranges between 0 and 1, where $r = 0$ indicates that for the same pesticide and crop, the importer and exporter have equal MRLs and there is therefore no heterogeneity.[9]

To examine the assumptions inherent in the quotations mentioned at the beginning of this section - namely that regulatory heterogeneity affects trade only when the importer is stricter - we separate[10] the heterogeneity index into two sub-indices: s^m and s^x, the former corresponding to heterogeneity emanating from cases in which the importer has more stringent regulation, and the latter to cases in which the exporter is equally or more stringent.[11]

$$s^m_{ijpkt} = \begin{cases} \frac{abs(MRL_{ipkt} - MRL_{jpkt})}{MRL_{ipkt} + MRL_{jpkt}} & \text{if } MRL_{ipkt} > MRL_{jpkt} \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

$$s^x_{ijpkt} = \begin{cases} \frac{abs(MRL_{ipkt} - MRL_{jpkt})}{MRL_{ipkt} + MRL_{jpkt}} & \text{if } MRL_{ipkt} \leq MRL_{jpkt} \\ 0 & \text{otherwise} \end{cases} \tag{3}$$

Following standard practice in this literature (for instance see Li and Beghin, 2014), we construct aggregate indices for each product averaging over the number of pesticides used per product.[12] Thus we have:

[9]We use the modified Achterbosch et al. (2009) index over other measures of regulatory heterogeneity used in the literature as this index measures MRL stringency relative to the MRL set by another trading partner in our country sample and not relative to the Codex or another international standard *a la* Li and Beghin (2014). In that sense, the Achterbosch et al. (2009) index is dyadic and more pertinent to our research question. The similarity index used by Drogué and DeMaria (2012) only captures the linear relationship between the two MRL regulations and does not consider differences in levels: two countries might have perfectly collinear regulation but at different levels, thus having a similarity index of 0 (very similar) and yet be very different in terms of stringency. This said, in our sensitivity analyses, we also find our empirical findings to be robust to the use of the Winchester et al. (2012) index.

[10]We are aware of the problems associated with dichotomizing the expanatory variable and therefore examine the robustness of our "endogeneity" findings to the dichotomization of the relative MRL stringency index below (see Section 8.2).

[11]In another robustness check, we experimented with a stronger definition of relative exporter stringency, i.e. $MRL_{ipkt} < MRL_{jpkt}$. Our empirical findings were found to be robust to this change in definition.

[12]The number of pesticides regulated is found to vary by product. For instance, the US has 107 pesticide MRLs for apples but only 7 pesticide MRLs for coconut (Li and Beghin, 2014). By averaging the sum of the relative stringency index of each pesticide by the total number of pesticides, we make the indices invariant to regulation intensity *a la* Li and Beghin, 2014. The use of the simple average thus avoids assigning higher values to certain products simply because a greater number of pesticides are commonly applied to those products.

$$S_{ijpt}^{M} = \frac{1}{K}\sum_{k=1}^{K} s_{ijpkt}^{m} \tag{4}$$

$$S_{ijpt}^{X} = \frac{1}{K}\sum_{k=1}^{K} s_{ijpkt}^{x} \tag{5}$$

where K is the total number of pesticides for which there is an MRL on product p.

In contrast to the existing literature, we thus consider the effects of regulatory heterogeneity on trade by source of such heterogeneity by including these aggregate sub-indices together in our estimating equation in Section 6 (see equation 9). This distinguishes our approach from that of simply ignoring heterogeneity when the exporter is stricter (Burnquist et al., 2011; Xiong and Beghin, 2014; Ferro et al., 2015) and from the approach that heterogeneity always imposes compliance costs for the exporter in the importing country (Achterbosch et al., 2009; Drogué and DeMaria, 2012; Winchester et al., 2012).

Finally, we would like to point out a few cases in the construction of the heterogeneity indices. Not all countries set MRLs for the same pesticide/crop combination; it can therefore be the case that the importer country sets an MRL for a k, p pair for which the exporting country has not set a limit and we would therefore have to drop this observation as no comparison is possible. To minimize this from happening, and without imputing values arbitrarily, we resort to *default MRL values*.[13]

Some countries set default MRLs for any k, p combination that is not explicitly cited in their MRL regulation, such as the EU that sets an MRL of 0.01 mg/kg for any pesticide on any crop that is not listed in the European Commission Regulation No 396/2005. Table 1 summarizes the pertinent default MRL cases.

Thus, in cases where one of the partner countries was missing the MRL, we resort to the missing country's default value (if any) to compute the heterogeneity index. In cases where there is no default MRL in place as well, we replace the missing MRL with the sample's highest MRL following existing literature (for instance see Drogué et al. 2012 and Ferro et al. 2015).

In the empirical analyses that follow, we examine the sensitivity of our results both to the use of default MRL values and to replacements by the sample maxima by considering three different samples: Sample 1 (missing MRLs not replaced); Sample 2 (missing MRLs only

[13]Drogué and DeMaria (2012) and Xiong and Beghin (2014) also resort to default values, and to the best of our knowledge they are the only ones doing so apart from us.

replaced by default MRLs) and Sample 3 - "the full sample" (missing MRLs in Sample 2 replaced by sample maxima).

<Insert Table 1 here>

5 Data

We use data on MRL regulation covering the period between 2005 and 2014 in the following 53 importing and exporting countries: Argentina, Australia, Brazil, Canada, Chile, China, Colombia, Egypt, India, Israel, Japan, Korea, Mexico, Malaysia, Norway, New Zealand, Russia, Singapore, South Africa, Switzerland, Thailand, Turkey, Ukraine, USA, Vietnam and the EU-28 members. The data on MRL regulation were acquired from Agrobase-Logigram, a private company that maintains Homologa, the Gobal Crop Protection Database, using information from pertinent national ministries and legal publications.

However, the richness of the data received from Homologa that covers 2638 products[14] could not be fully exploited because a large amount of crops are too specific compared to the Harmonized System (HS) 6-level data. To enable an empirical trade analysis of these MRLs, it becomes impossible to use all the Homologa data since that would introduce MRL variation within the HS code that cannot be matched by trade variables. We therefore selected products that matched perfectly. These 31 products are reported in Table 9.

The analysis is conducted at the disaggregated HS6-digit product level, focusing on trade in HS Chapters 7 and 8 over 2005-14. These HS Chapters correspond to the agriculture fruit and vegetables sectors where pesticide MRLs are relevant. Fruits and vegetables in particular are interesting sectors to analyze because these are rejected more often than other products like meat or dairy products. For instance, the EU reports 2621 rejections by a member state of the EU from 2008 to 2015 with an increasing trend (Fiankor et al., 2016).

Export data come from the UN Comtrade database in current USD. Data on (simple average) applied tariffs are sourced from the International Trade Center. The bilateral trade cost variables are taken from CEPII (Head et al., 2010) and data for PTA-membership come from De Sousa.[15]

[14] Including subcategories of products at various levels of aggregation.

[15]http://jdesousa.univ.free.fr/data.htm

Descriptive statistics are provided in Table 2. The full sample has 811,850 observations. Trade values are positive for about 25% of these. Correlation between the restrictiveness indices S^M_{ijpt} and S^X_{ijpt} were found to vary beteween -0.04 (Sample 2) and -0.13 (Samples 1 and 3), which obviates concerns about multicollinearity in estimation and statistically supports our strategy to distinguish between relative importer and exporter stringencies in our estimating equations. The original dataset without any MRL replacements (Sample 1) has 580,154 observations; the sample size goes up to 731,634 with missing MRLs replaced with default values (Sample 2), and further to 811,850 with sample maxima used to replace missing MRLs in cases which did not even report default MRLs (Sample 3).

<Insert Table 2 here>

At the product level, the mean values of S^M_{ijpt} and S^X_{ijpt} by country averaged over 2005-2014 for Sample 1 (missing MRLs not replaced) are shown in Figures 1 and 2, respectively. Figure 1 shows that Ukraine, Brazil, Germany, South Africa, Czech Republic and Austria are the strictest importers (on average) relative to their exporters for our product and year coverage. In general, developed countries exhibit larger magnitudes of relative importer stringency compared to the developing world (Vietnam, India, China, Malaysia) in our product-level data averaged over 2005-2014. The ranking of countries by relative exporter stringency is also similar (see Figure 2). Most European countries are ranked in the middle of the distributions.

<Insert Figures 1 and 2 here>

Figure 3 shows the average number of pesticides regulated per product in each country at two points in time (2005 and 2014). Figure 3 reveals that developed countries (Germany, Austria, Japan) regulate a much larger number of pesticides per product and even though there have been significant changes within the overall distribution, the broad picture is fairly constant over time, with developing countries regulating far fewer pesticides per product. Figure 4, which shows the average number of products for which MRLs are set in each country (again across 2005, 2014), reveals the same pattern. Developed countries like Canada and the US are also far more active in setting pesticides standards.

<Insert Figures 3 and 4 here>

6 Empirical model

Our empirical analysis is conducted in the framework of the gravity model as laid down by Anderson (1979). Following Anderson and van Wincoop (2004), the value of exports from country i to country j of product p at time t can be written as follows:

$$X^p_{ijt} = \frac{E^p_{jt} Y^p_{it}}{Y^p_t} \left(\frac{\phi^p_{ijt}}{P^p_{it} \Pi^p_{jt}} \right)^{(1-\sigma^p)} \tag{6}$$

where X^p_{ijt} denotes the value of exports of product p from country i to j at time t, E^p_j is the expenditure in the destination country j of product p, Y^p_i denotes the total sales of exporter i towards all destinations, Y^p is the total world output of product p, ϕ_{ij} are the bilateral trade costs and σ^p is the elasticity of substitution across products. P^p_{it} and Π^p_{jt} are the Multilateral Resistance Terms (MRTs) i.e. the outward and inward relative resistance of a country's exports towards *all* destinations and from *all* origins.[16] Since these terms are difficult to construct directly as national price indices are needed, applications of the gravity model resort to using dummy variables to control for them instead. At the sectoral level, time-varying importer-product and exporter-product fixed effects control for the MRTs in a panel setting (Anderson and Yotov, 2012).

Bilateral trade costs in ϕ_{ijpt}[17] arise from different sources such as import tariffs, τ_{ijpt}; geographical distance between trading partners, $ln(Dist_{ij})$; cultural distance proxied by dummy variables identifying whether the trading partners share a common border, $Contig_{ij}$, had a colonial relationship, $Colony_{ij}$, and share a common language, $ComLang_{ij}$.;

These variables enter ϕ_{ijpt} as follows:

$$\phi^{1-\sigma}{}_{ijpt} = exp(\beta_1 ln(1 + \tau_{ijpt}) + \beta_2 ln(Dist_{ij}) + \beta_3 Contig_{ij} + \beta_4 ComLang_{ij}) \tag{7}$$

Substituting (7) into (6), adding an error term, and taking the log of the resulting multiplicative model, yields the following estimating equation:

$$ln(X_{ijpt}) = \beta_1 ln(1+\tau_{ijpt}) + \beta_2 ln(Dist_{ij}) + \beta_3 Contig_{ij} + \beta_4 ComLang_{ij} + \mu_{ipt} + \gamma_{jpt} + \epsilon_{ijpt} \tag{8}$$

where μ_{ipt} and γ_{jpt} are the time-varying exporter-product and importer-product fixed effects

[16] The MRTs are derived theoretically in Anderson and Van Wincoop (2003).

[17] The notation, regarding the subscripts, is slightly modified to accommodate the product dimension, p.

that proxy the MRTs and ε_{ijpt} is the error term.

In the context of this study, stricter importer and exporter regulation indices detailed in Section 4, S^M_{ijpt} and S^X_{ijpt}, can not only add to bilateral trade costs but the information disclosed by more stringent regulation can also enhance demand in the importing country by altering consumer preferences (for instance see Xiong and Beghin, 2014). Equation (9) is thus augmented to include the relative stringency indices as follows:

$$ln(X_{ijpt}) = \beta_1 S^M_{ijpt} + \beta_2 S^X_{ijpt} + \beta_3 ln(1+\tau_{ijpt}) + \delta_1 ln(Dist_{ij}) + \delta_2 Contig_{ij} + \delta_3 ComLang_{ij} + \mu_{ipt} + \gamma_{jpt} + \epsilon_{ijpt} \tag{9}$$

6.1 Estimation issues

Two stylized features of trade data that challenge the estimation of structural gravity models are sample selection and heteroskedasticity (Xiong and Chen, 2014). As seen in Table 2, X_{ijpt} equals zero in at least 84% of all observations across the three samples. Sample selection is therefore clearly a concern in our data.

Most recent papers in this literature (for instance see Xiong and Beghin, 2014; Ferro et al. 2015) resort to the two-stage Heckman (1979) to control for the sample selection bias. Notwithstanding the exclusion restriction issue in Heckman-type estimations emphasized in the heterogeneous firm trade literature (for instance see Head and Mayer, 2013), we replicate the estimation strategies in these papers and get qualitatively similar results that document the trade-impeding effect of stricter standards (see Appendix for details).

However, the two-stage Heckman does not account for endogeneity in the standards-trade relationship, which, for the reasons discussed in Section 3, is an equally significant challenge in the context of this study.[18]

[18] One possible estimation strategy which could address all three issues - sample selection, heteroskedasticity and endogeneity - is the Two-Step Method of Moments (TS-MM) estimator proposed by Xiong and Chen (2014). The first step of the TS-MM is identical to the Heckman proposed by Helpman et al. (2008) and thus, accounts for sample selection. The second step characterizes trade in levels to be estimated on the sample of positive exports using the Method of Moments; this accounts for heteroskedasticity-related concerns *a la* Poisson Pseudo-Maximum Likelihood or PPML (Silva and Tenreyro, 2006). Estimating the outcome equation of the TS-MM using Generalized Method of Moments (GMM) further accounts for endogeneity. However, the use of GMM requires the fixed effects to be time-invariant, which is not true in our case. Moreover, concerns remain regarding the choice of the exclusion restriction in the first stage of the TS-MM estimator.

We therefore decided to account for endogeneity using a random growth first difference (RGFD) model with three-way fixed effects in line with Baier and Bergstrand (2007) and Baier et al. (2014). Incidentally, the use of Baier and Bergstrand's (2007) panel data approach to address endogeneity was also suggested by Li and Beghin (2012) in their meta-analysis. Unlike IV estimation, the use of the RGFD model also obviates the need for finding valid instruments for endogenous variables, which was an additional challenge in the context of this study.

The RGFD model[19] entails the following empirical specification:

$$d.lnX_{ijpt} = \beta_1(d.S^M_{ijpt}) + \beta_2(d.S^X_{ijpt}) + \beta_3(d.ln(1+\tau_{ijpt})) + \mu_{d.ipt} + \gamma_{d.jpt} + \eta_{ij} + \epsilon_{d.ijpt} \quad (10)$$

where in addition to the variables already described above, η_{ij} are pairwise bilateral fixed effects and d is the difference operator.[20]

We believe that this estimation strategy goes a long way in neutralizing endogeneity-related biases in the standards-trade relationship. The use of time-varying importer-product and exporter-product fixed effects accounts for the omitted variables (for e.g. consumer preferences discussed in Section 3.2). Since any change in regulation may also be motivated by bilateral factors (as discussed in Section 3.3), the use of dyadic fixed effects additionally controls for such simultaneity bias. Finally, the exogeneity test of the RGFD estimates in Section 8.1 below provides evidence that the RGFD model also controls for reverse causality in the estimating equation.

An additional advantage of the RGFD model is that it also corrects for biases emanating

[19] Baier et al. (2014) provide for a choice between fixed effects (FE), first difference (FD) and randon growth first difference (RGFD) models. Following Wooldridge (2010, Ch. 10), the authors suggest that when t>2, the FE estimator (equation (9) estimated along with dyadic fixed effects) is more efficient than the FD estimator (equation (9) estimated in differences) if the errors from estimating the FE equation are serially uncorrelated while the FD estimator is more efficient than the FE estimator if the error term follows a random walk. In general, FD estimates are more efficient than FE estimates if the errors from the FE model are highly serially correlated and the dependent variable follows a unit root process. In our t>2 panel structure, we found the estimated error term from estimating equation (9) with dyadic fixed effects to be highly serially correlated and our dependent variable to be close to a unit root process. This suggested that the FD model would be more efficient than the FE model to account for endogeneity in the standards-trade relationship in our estimating equation. Finally, compared to the FD model, the additional use of dyadic fixed effects, η_{ij}, in the RGFD model gives it the added advantage of controlling for changes over time in pairwise unobservables, such as the experience acquired in exporting, that are unrelated to stricter standards.

[20] Baier et al. (2014) used differencing over five years as they had a panel over 1962-2000 but their results were robust to first differencing their data. While we use first differences in estimation, our results from the RGFD model were robust to differencing the data over two, three and four years. These results are available upon request.

from sample selection and firm heterogeneity by accounting for fluctuations across country pairs and over time in the latent variable z_{ijpt}[21] that reflects the ratio of variable export profits to fixed export costs for the most productive firm (for details see Baier et al., 2014, p.346).

Finally, to assuage concerns about biases emanating from heteroskedasticity and to accommodate zero trade flows in the sample more directly in the estimating equation, we also consider the PPML in our sensitivity analysis.

7 Results based on RGFD estimates: endogeneity matters

The results from estimating the RGFD model in equation (10) are reported in Table 4 for all three samples. Consistent with equation (10), all estimations include bilateral pairwise and first-differenced importer-product and exporter-product fixed effects. Note that unlike bilateral distance or common language, our relative stringency indices are dyadic only by construction. Since the indices are defined at the more disaggregated HS-6 product level, we construct the importer-product and exporter-product fixed effects at the more aggregate HS-4 product level. This is an important dimension of our identification strategy, which obviates concerns about fixed effects constructed at the HS-6 product level being collinear with the relative stringency indices. Since the relative stringency indices vary by dyad-HS6-product-year, standard errors are also clustered at that level.

<Insert Table 4 here>

The RGFD estimates reported in columns (1) and (3), for samples 1 and 3, respectively, suggest that growth in relative MRL stringency is positively correlated with growth rates of export, irrespective of the source of stringency. Thus accounting for endogeneity using the RGFD model with three-way fixed effects reverses the direction of the estimated trade effects compared to the Heckman results reported in Table 3, which is a significant departure from the findings in the existing MRL literature. In particular, results reported for the more complete sample in column (3) of Table 4 suggest that unit additional growth in relative importer stringency is associated with a 46.9% rise in the growth rate of exports while unit additional growth in relative exporter stringency is associated with a 72.3% rise in the growth rate of exports, *ceteris paribus* and on average.

[21] Helpman et al. (2008) show that accounting for z_{ijpt} accounts for both the Heckman selection bias (the inverse mills ratio is a monotonic function of z_{ijpt}) and the firm heterogeneity bias (the control for this is a function of z_{ijpt} and the inverse mills ratio (which in-turn is a function of z_{ijpt})).

Thus, once we alleviate endogeneity-related biases in the standards-trade relationship using the RGFD model with three-way fixed effects, the demand enhancing effect of stricter standards prevails over the trade cost effect, irrespective of the source of imposition. The findings in this sub-section are robust to using alternative samples (1 and 3), to substituting tariffs with membership of PTAs[22] in the estimating equation and to differencing over two, three and four years (the latter results are available upon request).

This result is in contrast to most other findings on the trade effects of pesticides MRLs in this literature and can be explained as follows. The indices S^M_{ijpt} and S^X_{ijpt} can not only increase bilateral trade costs but the information conveyed by more stringent regulation can also enhance consumer demand in the importing country. The RGFD estimates suggest that the bilateral trade costs are lowered or consumer demand enhanced, which results in a positive effect on exports. This can be further explained as follows. If consumers are more pro-food safety in the importing country and well aware of food standard regulation, then more stringent importer regulation still generates a demand-enhancing effect (despite any higher prices from costs of meeting stricter importing country standards). Similarly, if exporting country firms already comply with stricter standards at home, it is less costly for them to meet importing country standards, again resulting in a positive effect on trade, which may be further accentuated by pro-food safety importing country consumers.

8 Sensitivity analyses

8.1 Exogeneity test

Consistent with the RGFD estimates, endogeneity in the standards-trade relationship suggests that exports may increase in anticipation of a stricter importing or exporting country standard. However a simple exogeneity test requires that any leading of exports before the imposition of stricter standards needs to be well in advance of the estimated trade effects, needs to be economically small, and needs to diminish as the date of imposition of the stricter standard approaches (Baier et al., 2014).

Following the analyses in Baier and Bergstrand (2007) and Baier et al. (2014) and as suggested in Wooldridge (2010), we therefore re-estimated the RGFD model in equation (10) to include up to five-year leads of the heterogeneity indices and tariff variables. Results from these analyses, reported in Table 5, indicate statistically insignificant impacts of the leads

[22] Note that the use of three-way fixed effects also accounts for endogeneity in the PTA-trade relationships in estimation.

of the heterogeneity indices on exports. These findings confirm the absence of any feedback effects from changes in exports to changes in relative MRL stringency, thereby providing evidence that the RGFD model also controls for reverse causality in the standards-trade relationship.

<Insert Table 5 here>

8.2 Dichotomized explanatory variable

Existing literature on the dichotomization of explanatory variables (for instance see MacCallum et al., 2002) suggests that splitting the relative MRL stringency index into relative importer and relative exporter stringency indices would lend a downward bias to the magnitude of the esimates besides leading to loss of statistical power, even though it serves one of our research objectives.

We therefore examine the robustness of our "endogeneity" findings to the dichotomization of the relative MRL stringency index by replacing the relative importer and relative exporter stringency indices by the non-dichotomized relative MRL stringency index R^{Ach}_{ijpt} in the estimating equations, where

$$R^{Ach}_{ijpt} = \frac{1}{K}\sum_{k=1}^{K} r_{ijpkt} \tag{11}$$

and r_{ijpkt} is as defined in equation (1). For the robustness of our "endogeneity" findings, we would expect the coefficient of R^{Ach}_{ijpt} to be negative in the Heckman estimations (see Appendix for details) and positive in the RGFD estimations. Assuringly this is what we find: greater relative MRL stringency is associated with a decline in exports at both margins across the three samples in the Heckman results reported in Table 6, columns (1) to (6); however, the direction of the estimated trade effects is reversed across the three samples once we account for endogeneity using the RGFD model with three-way fixed effects, in the results reported in Table 7, columns (1) to (3).

<Insert Tables 6 and 7 here>

8.3 Alternative heterogeneity index from Winchester et al. (2012)

In another robustness check, we examine the sensititvity of our analyses to the use of an alternative regulatory heterogeneity index, the Winchester et al. (2012) index, which is defined, at the pesticide level, as follows:

$$r_{ijpkt}^{Win} = \frac{abs(MRL_{ipkt} - MRL_{jpkt})}{max(MRL_{pkt}) - min(MRL_{pkt})} \tag{12}$$

and at the product level, as follows:

$$R_{ijpt}^{Win} = \frac{1}{K}\sum_{k=1}^{K} r_{ijpkt} \tag{13}$$

Again, for our "endogeneity" findings to be robust, we would expect the coefficient of R_{ijpt}^{Win} to be negative in the Heckman estimations and positive in the RGFD estimations. And this is exactly what we find comparing the Heckman results reported in Table 6, columns (7) to (12) with the RGFD estimates in Table 7, columns (4) to (6).

8.4 Alternative estimation strategy: PPML

In a final robustness check, we estimate equation (9) using the PPML with three-way fixed effects, including R_{ijpt}^{Ach} and R_{ijpt}^{Win} in distinct specifications. The importer-product and exporter-product fixed effects are again constructed at the more aggregate HS4-product level while standard errors are clustered by dyad-HS6-product-year. The coefficients of both variables are found to be statistically indifferent from zero in two of the three samples in the results reported in Table 8. While weaker than the RGFD estimates, these results still confirm that stricter standards may not impede trade.

<Insert Table 8 here>

8.5 Relative exporter stringency and implementation

It is easier to associate relative importer stringency with demand-enhancing effects in the importing country. In contrast, consumers in the importing country may like to be assured that more stringent exporter regulation is actually binding for it to have any demand-enhancing effects. If relative exporter stringency were positively correlated with regulatory capacity, rule of law, governance and implementation, then the positive impact of more stringent exporter regulation on trade may be more tenable given that (i) the enforcement of strict public standards like MRLs requires good governance and well functioning public institutions; and

(ii) we expect countries with higher levels of governance to be also more concerned about food safety.

To relate these attributes to higher levels of relative exporter stringency, we use the Worldwide Governance Indicators (WGI, Kaufmann et al., 2011) of the World Bank as proxies for the quality of public institutions. These indicators include stability and effectiveness of the government, the extent of corruption, public violence, and – among others – freedom and democracy. Annex Table 1 provides an overview of all six WGI indicators.

As shown in Annex Table 2, all six indicators are highly correlated, which renders their simultaneous use inappropriate. We therefore employ principal component analysis (PCA), instead of choosing only one indicator or an arbitrary subset, to reduce the dimension of the WGI data. The PCA reveals that one component explains around 84% of the variation (see Annex Table 3). This component represents institutional quality in the context of our findings on the impact of relative exporter stringency and is denoted as WGI in Figure 5.

Figure 5 is a scatterplot of S^X_{ijpt} against WGI for our panel and shows that relative exporter stringency is positively correlated with the attributes embodied in the WGI, thereby lending further robustness to our findings on the positive trade effects of more stringent exporter regulation.

<Insert Figure 5 here>

9 Conclusion

Using two measures of MRL heterogeneity in the same estimating equation that, departing from existing literature, also include cases when the exporting country is stricter compared to the importing country, we re-examine the effect that dissimilarity in MRL regulation can have on bilateral trade for a large set of trading partners over 2005-14. In another significant contribution, we also alleviate endogeneity-related concerns in the standards-trade relationship using a random growth first difference model with three-way fixed effects in line with recent developments in the empirical trade literature (for instance Baier et al., 2014).

Stricter standards have both trade cost and demand enhnacing effects and the overall impact depends on which effect prevails (Xiong and Beghin, 2014). Without accounting for endogeneity in the standards-trade relationship, results from the Heckman estimations suggest that the trade cost effect prevails. Neutralizing endogeneity-related concerns using the

RGFD model with three-way fixed effects, empirical results suggest that the demand enhancing effect prevails. Thus, endogeneity-induced biases are found to underestimate the estimated trade effects in our empirical findings and we also provide an economic rationale of why this may be the case.

In sum, our results show that mitigating endogeneity reverses the direction of the estimated trade effects of pesticides MRLs, which is a significant departure from the findings in the MRL literature (Achterbosch et al., 2009; Burnquist et al., 2011; Drogué and DeMaria, 2012; Xiong & Beghin, 2014; Ferro et al., 2015). Specifically, stricter MRL standards are found to have a non-negative effect on trade in most of our results, irrespective of the source of stringency. While the positive trade effect of stricter standards has been documented in this literature (Blind and Jungmittag, 2005; Xiong and Beghin, 2013; Ishaq et al., 2016), these authors did not account for the endogeneity in the standards-trade relationship.

And this endogeneity clearly matters. If the domestic industry has a comparative advantage in meeting regulation and exports are destined for markets where consumer preferences are more pro-food safety, then the demand enhancing effect of more stringent regulation imposed on products more likely to be exported is likely to outweigh the trade cost effect. Similarly, if consumption through imports is sought to be curtailed by the imposition of stricter standards on products where the domestic industry is less competitive (and not because the imported product requires more stringent regulation on scientific or health grounds *per se*), then the imported product is likely to be more cost-competitive and/or of a better quality than the domestic substitute in the destination market. So, the demand enhancing effect may still outweigh the trade cost effect.

We realize that our findings challenge both conventional wisdom and much published work on this subject specific to pesticides MRLs. At the same time, this topic has been widely studied in the standards literature without finding a consensus and we show our results to be robust to a variety of sensitivity analysis. Illustratively, our results are robust to the choice of different estimation samples (based on dropping assumptions made in the construction of the heterogeneity indices), to using different sets of explanatory variables, to not splitting the regulatory heterogeneity index and to using two altogether different indices.

One shortcoming of this study, like the others that focus only on MRLs, is that it ignores other SPSQ measures that have a bearing on trade in agri-products. It would therefore be useful to examine the results from this research on a broader set of SPSQ measures as well as private standards.

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Appendix: Two-stage Heckman to replicate existing results in the literature

For the sake of replicating estimation strategies and some of the findings in recent papers using variants of the same dataset as ours (for instance Xiong and Beghin, 2014; Ferro et al. 2015), we estimated the baseline equation using the two-step Heckman as follows:

$$Pr(X_{ijpt} > 0) = \beta_1 S^M_{ijpt} + \beta_2 S^X_{ijpt} + \beta_3 ln(1+\tau_{ijpt}) + \gamma EV_{ijpt} + \delta_1 ln(Dist_{ij}) + \delta_2 Contig_{ij} + \delta_3 ComLang_{ij} + \mu_{ipt} + \gamma_{jpt} + \epsilon_{ijpt} \tag{14}$$

$$ln(X_{ijpt}|X_{ijpt} > 0) = \beta_1 S^M_{ijpt} + \beta_2 S^X_{ijpt} + \beta_3 ln(1+\tau_{ijpt}) + \Theta\eta_{ijpt} + \Lambda z_{ijpt} + \delta_1 ln(Dist_{ij}) + \delta_2 Contig_{ij} + \delta_3 ComLang_{ij} + \mu_{ipt} + \gamma_{jpt} + \epsilon_{ijpt} \tag{15}$$

Given the exclusion restriction issue in Heckman-type estimations emphasized in the heterogeneous firm trade literature (for instance see Head and Mayer, 2013), we closely followed Helpman et al. (2008) in our estimation strategy. Following Xiong and Beghin (2014), we used an indicator variable for common religion interacted with HS-4 chapter fixed effects as the exclusion variable, EV_{ijpt}, in the selection equation (14) to allow for heterogeneity across sectors in the self-selection process. We used the predicted probabilities, $\hat{\rho_{ijpt}}$, from the selection equation (14) to construct the inverse mills ratio[23], η_{ijpt}, which was included in the outcome equation (15) to control for the selection bias. Following (Helpman et al., 2008), we also controlled for biases emanating from firm heterogeneity in the outcome equation (15) by including a cubic polynomial of z_{ijpt} where $z_{ijpt} = \eta_{ijpt} + \hat{\rho_{ijpt}}$.[24] Finally, given concerns associated with incidental parameters (Lancaster, 2000) in the use of fixed effects in non-linear estimations, we estimated the selection equation (14) using the Linear Probability model (LPM).

Table 3 reports the results of the Heckman two-step estimations on all three samples. All estimations include only time-varying importer-product and exporter-product fixed effects, but not dyadic fixed effects.[25] We construct the importer-product and exporter-product fixed effects at the more aggregate HS-4 product level, as the relative stringency indices are defined at the more disaggregated HS-6 product level. Standard errors are clustered by dyad-HS6-product-year.

[23] $\eta(\hat{\rho}) = \frac{\phi(\hat{\rho})}{\Phi(\hat{\rho})}$, where $\phi(\cdot)$ and $\Phi(\cdot)$ are the standard normal density function and the standard normal cumulative function, respectively and $\hat{\rho_{ijpt}}$ are the predicted probabilities from the selection equation.

[24] Following (Helpman et al., 2008), we did not use the normality assumption to recover η_{ijpt} and z_{ijpt} from the selection equation and instead worked directly with the predicted probabilities, $\hat{\rho_{ijpt}}$

[25] The Heckman estimates were found to be qualitatively similar with bilateral gravity controls replaced by dyadic fixed effects. However, the sample selection and firm heterogeneity terms in the outcome equation were found to be collinear with the fixed effects in those results.

<Insert Table 3 here>

These results suggest that MRL heterogeneity decreases the probability of having positive exports when the importer is stricter than the exporter, implying compliance costs imposed on exporters - the coefficient of S^M_{ijpt} is negative and statistically significant in columns (1), (3) and (5). A similar negative effect is observed at the intensive margin in columns (2), (4) and (6). This result is consistent with existing literature (Achterbosch et al., 2009; Burnquist et al., 2011; Drogué and DeMaria, 2012; Ferro et al., 2015).

The coefficient of S^X_{ijpt} is found to be negative and statistically significant, especially at the extensive margin of trade in the results reported in columns (1), (3) and (5), and at the intensive margin in column (6). Thus, greater difference of MRLs between trading partners when the exporters have to comply with stricter regulations in their domestic market is found to diminish at least the probability of exporting conclusively, if not the value of exports. Thus, contrary to some findings in the existing literature (Blind and Jungmittag, 2005; Xiong and Beghin, 2013), relative stringency in exporter market is also negatively correlated with trade in the Heckman estimates.

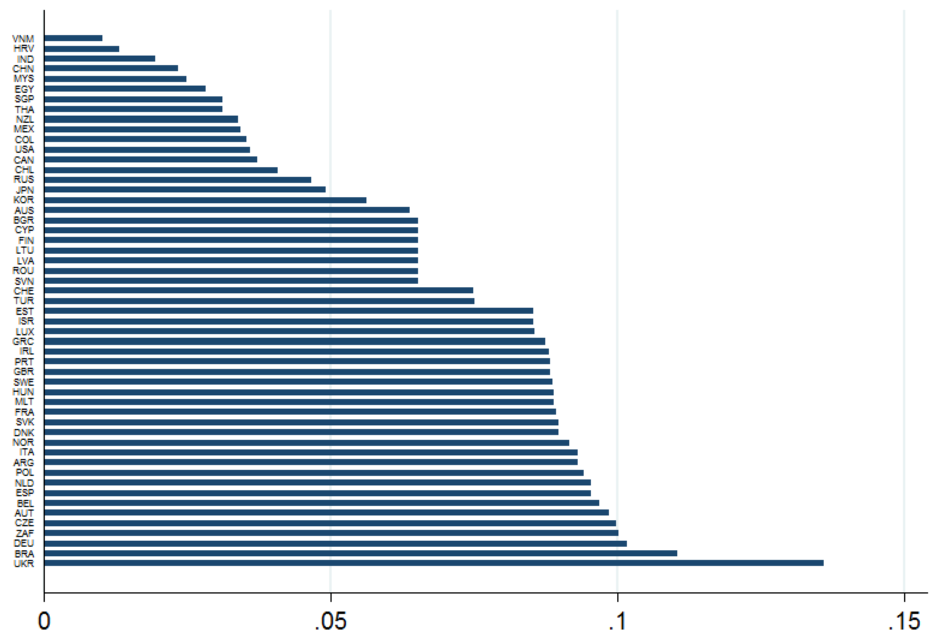
These results seem to suggest that irrespective of who imposes them, stricter standards impede trade. Thus, the trade cost effect of stricter standards prevails over the demand-enhancing effect in estimates that do not mitigate endogeneity in the standards-trade relationship and the results are found to be robust across the three samples. In unreported anaylses, these findings were also found to be robust to replacing tariffs with membership of preferential trade agreements (PTA_{ijt}).[26]

The coefficients of the gravity control variables are also consistent with existing gravity estimates. Countries with a common language or membership of a trade accord or which are adjacent to each other have higher probabilities of exporting to each other and also export larger values. Distance is found to reduce both the probability of trading and the value of trade between partners. We also find higher tariffs to reduce exports, both at the intensive and extensive margins, which is an expected result.

The exclusion variable, EV_{ijpt}, in the selection equation (14), and the sample selection, η_{ijpt}, and firm heterogeneity z_{ijpt} terms in the outcome equation (15), are also found to be statistically significant in these results. This confirms that countries in our sample are self-selected to trade and that firm heterogeneity matters.

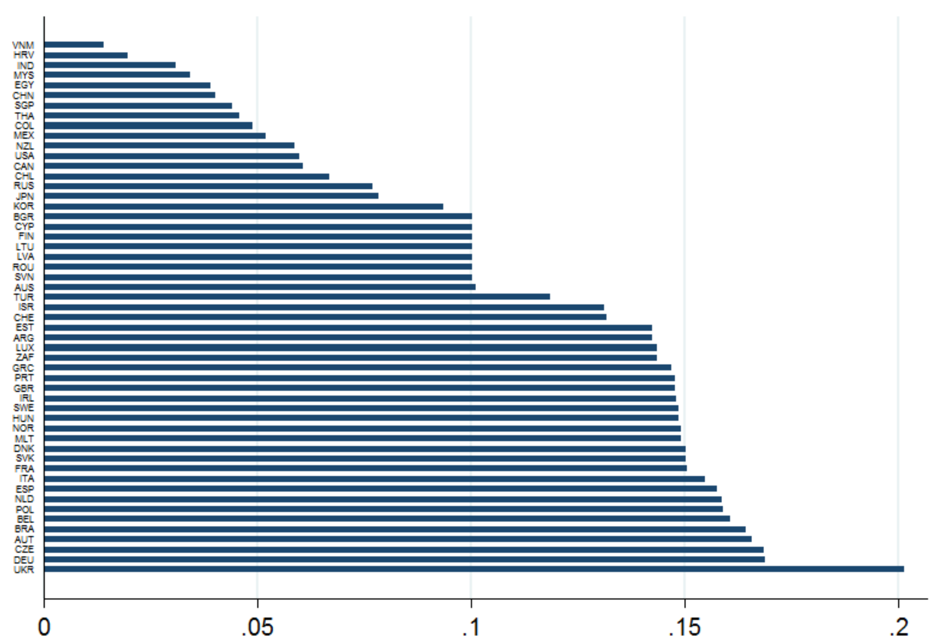
[26]These results are available upon request.

Figure 1: Mean S^{M}_{ijpt} (Sample 1: no missing MRL replacements)



Source: Authors' calculations based on data on MRL regulation from Homologa, S.A.

Figure 2: Mean S^{X}_{ijpt} (Sample 1: no missing MRL replacements)



Source: Authors' calculations based on data on MRL regulation from Homologa, S.A.

Figure 3: Average number of regulated pesticides per product by country of year 2005 and 2014

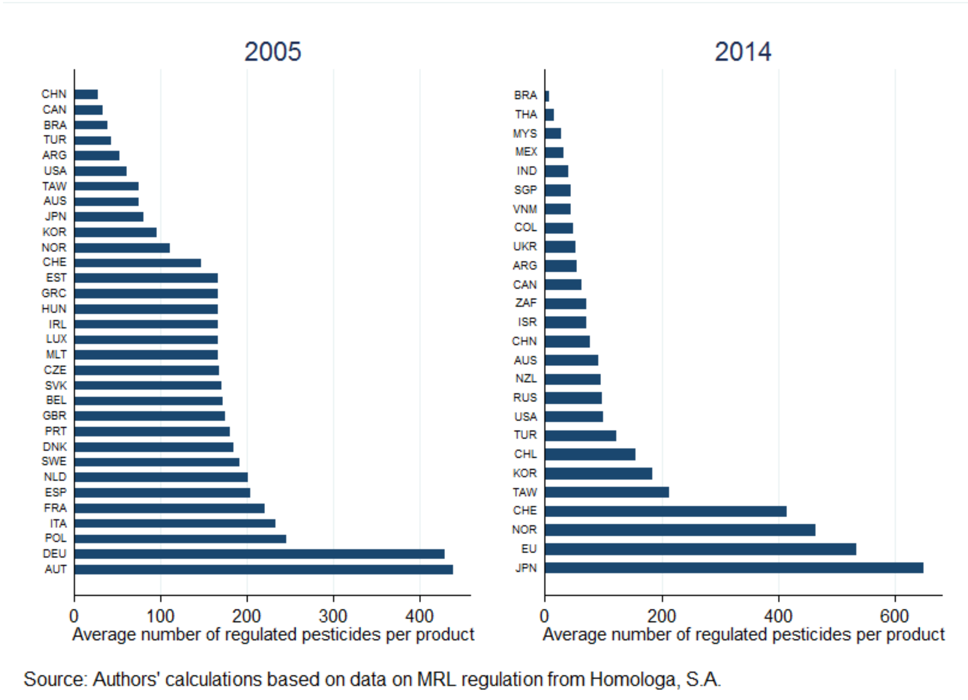


Figure 4: Average number of regulated products by country of year 2005 and 2014

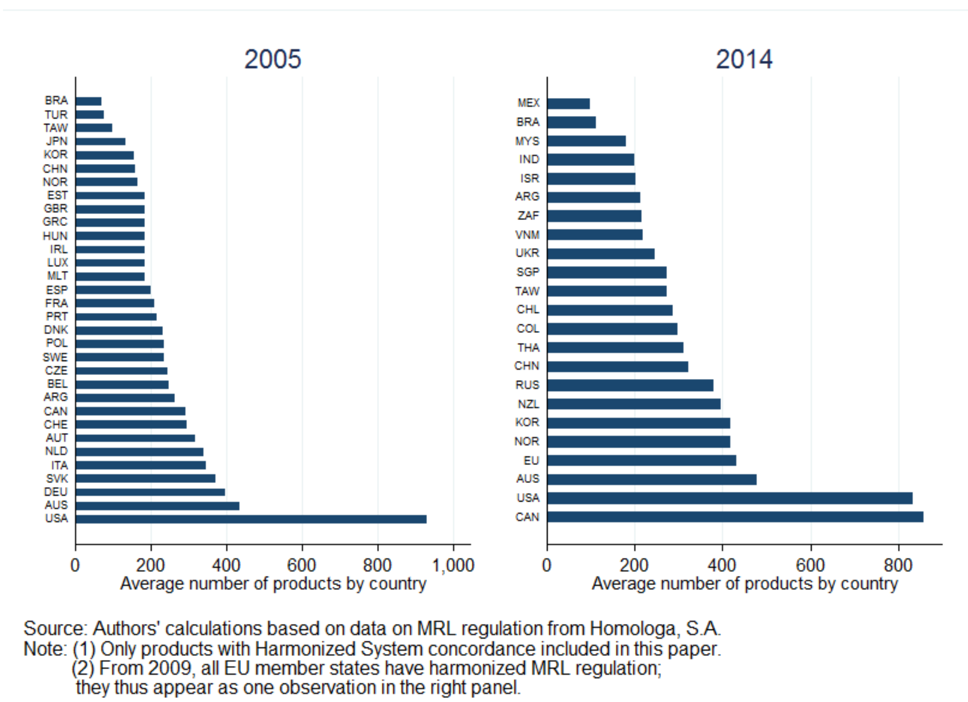
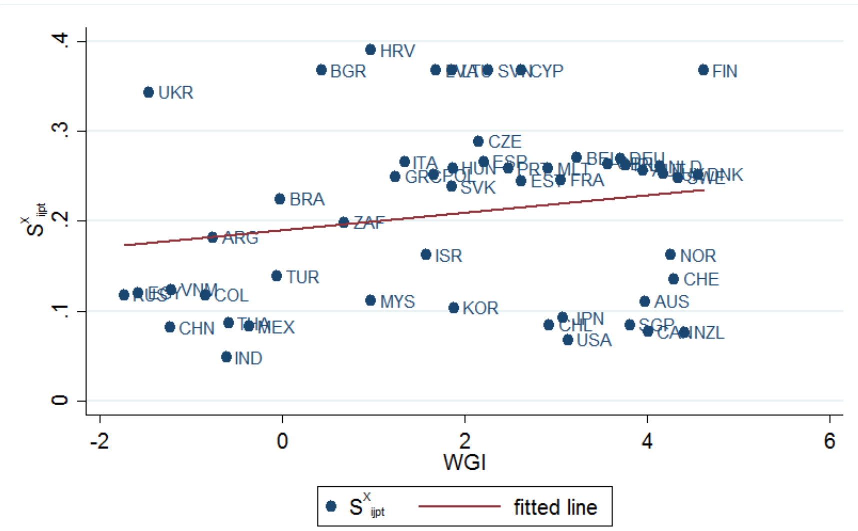


Figure 5: Relative exporter stringency is positively correlated with institutional quality



Source: Homologa and World Bank, own graph
Figure 5 plots the correlation between relative exporter stringency and institutional quality embodied in the World Bank's Worldwide Governance Indicators for our panel. The six WG indicators are comprised into one WGI variable using principal components analysis.

Table 1: Many countries use Codex MRLs as default values if national regulation is missing

Country	**First default**	**Second default**
Argentina	Codex	0.01
Australia	0.01	
Brazil	Codex	
Canada	0.01	
Chile	Codex	
China	Codex	
Colombia		
Egypt	Codex	
European Union	0.01	
India	Codex	
Israel	Codex	
Japan	0.01	
Korea	Codex	
Malaysia	Codex	0.01
Mexico	Codex	
New Zealand	0.01	
Norway	0.01	
Russia	Codex	
Singapore	Codex	
South Africa	Codex	0.01
Switzerland	EU	0.01
Thailand	Codex	
Turkey	Codex	
Ukraine	Codex	
USA	0.01	
Vietnam	Codex	0.01

Note: Default MRL information from mrldatabase.com (US FDA) except otherwise stated.

Table 2: Descriptive statistics

	Sample 1			Sample 2			Sample 3		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
R_{ijpt}	480564	.336	.285	660666	.345	.296	807710	.346	.272
S^M_{ijpt}	569172	.099	.163	704112	.137	.201	808271	.17	.219
S^X_{ijpt}	580154	.112	.179	731634	.137	.203	811850	.17	.219
Exports (USD '000s)	580154	405.3	5782.8	731634	373.8	5589.8	811850	359.2	5651.3
Distance (km)	580154	6166.2	5049.8	731634	6155.6	5039.6	811850	6220.8	5010.4
Contiguity	580154	.049	.217	731634	.049	.215	811850	.048	.214
Common language	580154	.073	.260	731634	.069	.253	811850	.0673499	.2506271
Common colony	580154	.032	.177	731634	.031	.175	811850	.031	.173
PTA membership	580154	.519	.5	731634	.52	.5	811850	.501	.5
Common religion	580154	.166	.372	731634	.158	.364	811850	.158	.364
Tariffs (simple avg. appd., %)	580154	1.066	.224	731634	1.062	.212	811850	1.065	.206
Share of zero exports	84%			85%			86%		
Correlation between S^M,S^X	-0.13			-0.04			-0.13		

Source of variables: Exports (UN Comtrade); R_{ijpt}, S^M_{ijpt}, S^X_{ijpt} (Homologa); Tariffs (ITC); Distance, Contiguity, Common language, Common colony (CEPII, Head et al. 2010); Common religion (Helpman et al. 2008); PTA membership (De Souza).
Sample 1 (S1) only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 (S2) = S1 + use of default MRLs to replace missing MRLs.
Sample 3 (S3) = S2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.

Table 3: Heckman results: stricter standards impede trade

	Sample 1		**Sample 2**		**Sample 3**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Pr(X_{ijpt} >0)	**ln(X_{ijpt})**	**Pr(X_{ijpt} >0)**	**ln(X_{ijpt})**	**Pr(X_{ijpt} >0)**	**ln(X_{ijpt})**
S^M_{ijpt}	-0.183***	-2.716***	-0.128***	-1.668***	-0.130***	-1.519***
	(0.00619)	(0.359)	(0.00449)	(0.347)	(0.00386)	(0.320)
S^X_{ijpt}	-0.189***	-0.348	-0.118***	-0.385	-0.107***	-0.689**
	(0.00522)	(0.398)	(0.00431)	(0.301)	(0.00379)	(0.282)
$ln(1+\tau_{ijpt})$	-0.0995***	-1.396***	-0.0966***	-1.865***	-0.0946***	-1.761***
	(0.00722)	(0.444)	(0.00672)	(0.443)	(0.00651)	(0.439)
$ln(Dist_{ij})$	-0.102***	-0.550***	-0.108***	-0.728***	-0.105***	-0.754***
	(0.00129)	(0.0843)	(0.00114)	(0.0852)	(0.00102)	(0.0834)
$Contig_{ij}$	0.157***	0.959***	0.163***	1.050***	0.147***	1.103***
	(0.00369)	(0.137)	(0.00336)	(0.136)	(0.00320)	(0.123)
$Colony_{ij}$	0.0284***	-0.233***	0.0250***	-0.161***	0.0187***	-0.174***
	(0.00419)	(0.0579)	(0.00386)	(0.0528)	(0.00370)	(0.0508)
$ComLang_{ij}$	0.0699***	0.643***	0.0625***	0.699***	0.0682***	0.704***
	(0.00283)	(0.0757)	(0.00263)	(0.0669)	(0.00249)	(0.0702)
EV_{ijpt}	0.00557***		0.00490***		0.00494***	
	(0.000310)		(0.000274)		(0.000254)	
η_{ijpt}		744.3***		732.4***		519.8***
		(209.2)		(174.6)		(195.0)
z_{ijpt}		12,355***		12,209***		8,175**
		(3,833)		(3,277)		(3,639)
z^2_{ijpt}		-12,782***		-12,637***		-8,121**
		(4,231)		(3,663)		(4,055)
z^3_{ijpt}		4,911***		4,847***		2,953*
		(1,753)		(1,537)		(1,697)
N	569,172	34,606	704,112	40,801	808,271	42,018
r2	0.395	0.677	0.388	0.681	0.375	0.680
Method	LPM	OLS	LPM	OLS	LPM	OLS
Fixed effects	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt

Sample 1 only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 = Sample 1 + use of default MRLs to replace missing MRLs.
Sample 3 = Sample 2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.
The exclusion variable used in the selection equation is a dummy variable for common religion interacted with HS-4 product fixed effects.
Product dimension in the fixed effects is at the HS-4 digit level.
LPM = Linear Probability Model.
Robust standard errors, clustered by dyad-product-year, included in parentheses.
Levels of signicance: *p<0.1 **p<0.05 ***p<0.01

Table 4: RGFD results: stricter standards facilitate trade

	Sample 1	**Sample 2**	**Sample 3**
	(1) $d.ln(X_{ijpt})$	**(2)** $d.ln(X_{ijpt})$	**(3)** $d.ln(X_{ijpt})$
$d.S^M_{ijpt}$	0.952***	0.295	0.469***
	(0.345)	(0.200)	(0.142)
$d.S^X_{ijpt}$	1.145***	0.323	0.723***
	(0.347)	(0.200)	(0.128)
$d.ln(1+\tau_{ijpt})$	-1.701***	-0.461	-0.322
	(0.638)	(0.749)	(0.760)
N	22,669	27,251	27,950
r2	0.072	0.054	0.054
Method	RGFD	RGFD	RGFD
Fixed effects	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij

Sample 1 only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 = Sample 1 + use of default MRLs to replace missing MRLs.
Sample 3 = Sample 2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.
d is the first difference operator.
Product dimension in the fixed effects is at the HS-4 digit level.
Robust standard errors, clustered by dyad-product-year, included in parentheses.
Levels of signicance: *p<0.1 **p<0.05 ***p<0.01

Table 5: Exogeneity test of RGFD estimates

	Sample 1	**Sample 2**	**Sample 3**
	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$
$d.S^M_{ijpt}$	0.087	0.771**	0.891***
	(0.547)	(0.301)	(0.199)
$d.S^X_{jpt}$	-0.900*	0.836***	0.942***
	(0.482)	(0.264)	(0.171)
$d.ln(1+\tau_{ijpt})$	-1.675*	-2.042**	-1.756*
	(0.891)	(0.909)	(0.953)
S^M_{ijpt+1}	0.710	1.172**	0.246
	(0.828)	(0.578)	(0.301)
S^X_{ijpt+1}	0.759	0.758	0.572*
	(0.671)	(0.552)	(0.297)
$ln(1+\tau_{ijpt+1})$	1.152	0.585	0.677
	(2.317)	(2.199)	(2.164)
S^M_{ijpt+2}	-0.465	-0.150	0.218
	(1.024)	(0.698)	(0.358)
S^X_{ijpt+2}	-0.085	0.758	-0.356
	(0.788)	(0.719)	(0.354)
$ln(1+\tau_{ijpt+2})$	0.257	1.340	1.132
	(1.440)	(1.542)	(1.486)
S^M_{ijpt+3}	-0.890	-0.863	-1.620***
	(2.217)	(0.699)	(0.520)
S^X_{ijpt+3}	0.438	-0.568	-0.640
	(1.771)	(0.647)	(0.433)
$ln(1+\tau_{ijpt+3})$	-1.179	0.434	0.895
	(1.799)	(1.821)	(1.808)
S^M_{ijpt+4}	1.120	0.610	0.688
	(2.321)	(0.497)	(0.438)
S^X_{ijpt+4}	-2.045	-0.812	-0.157
	(1.851)	(0.555)	(0.364)
$ln(1+\tau_{ijpt+4})$	-0.598	-2.897	-2.707
	(2.936)	(1.789)	(1.804)
S^M_{ijpt+5}	-0.184	0.467	0.656*
	(1.539)	(0.441)	(0.366)
S^X_{ijpt+5}	2.301**	1.157**	0.868***
	(1.108)	(0.523)	(0.330)
$ln(1+\tau_{ijpt+5})$	0.676	1.607	1.252
	(1.937)	(1.715)	(1.656)
N	7100	11019	11549
r2	0.145	0.109	0.114
Method	RGFD	RGFD	RGFD
Fixed effects	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij

Sample 1 only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 = Sample 1 + use of default MRLs to replace missing MRLs.
Sample 3 = Sample 2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.
d is the first difference operator.
Product dimension in the fixed effects is at the HS-4 digit level.
Robust standard errors, clustered by dyad-product-year, included in parentheses.
Levels of signicance: *p<0.1 **p<0.05 ***p<0.01

Table 6: Heckman estimates robust to non-dichotomization of relative stringency (R_{ijpt}) and to the use of the Winchester et al. (2012) index

	Achterbosch						Winchester					
	Sample 1		Sample 2		Sample 3		Sample 1		Sample 2		Sample 3	
	(1) $\mathbf{Pr}(X_{ijpt}>0)$	(2) $\ln(X_{ijpt})$	(3) $\mathbf{Pr}(X_{ijpt}>0)$	(4) $\ln(X_{ijpt})$	(5) $\mathbf{Pr}(X_{ijpt}>0)$	(6) $\ln(X_{ijpt})$	(7) $\mathbf{Pr}(X_{ijpt}>0)$	(8) $\ln(X_{ijpt})$	(9) $\mathbf{Pr}(X_{ijpt}>0)$	(10) $\ln(X_{ijpt})$	(11) $\mathbf{Pr}(X_{ijpt}>0)$	(12) $\ln(X_{ijpt})$
R_{ijpt}	-0.160***	-0.994***	-0.112***	-0.686***	-0.120***	-1.051***	-0.363***	-8.682***	-0.128***	-1.985**	-0.057***	-0.438
	(0.004)	(0.273)	(0.003)	(0.234)	(0.003)	(0.220)	(0.023)	(1.385)	(0.013)	(0.992)	(0.008)	(0.549)
$ln(1+\tau_{ijpt})$	-0.106***	-1.533***	-0.107***	-1.945***	-0.094***	-1.771***	-0.136***	-1.830***	-0.126***	-2.113***	-0.112***	-1.951***
	(0.008)	(0.462)	(0.007)	(0.452)	(0.007)	(0.440)	(0.008)	(0.466)	(0.007)	(0.453)	(0.007)	(0.438)
$ln(Dist_{ij})$	-0.1***	-0.619***	-0.108***	-0.717***	-0.105***	-0.760***	-0.129***	-0.757***	-0.126***	-0.787***	-0.121***	-0.828***
	(0.002)	(0.084)	(0.001)	(0.085)	(0.001)	(0.08)	(0.001)	(0.106)	(0.001)	(0.16)	(0.001)	(0.09)
$Contig_{ij}$	0.151***	1.107***	0.160***	1.047***	0.147***	1.115***	0.128***	1.222***	0.147***	1.104***	0.136***	1.177***
	(0.004)	(0.136)	(0.004)	(0.133)	(0.003)	(0.123)	(0.004)	(0.115)	(0.004)	(0.121)	(0.003)	(0.113)
$Colony_{ij}$	0.0271***	-0.211***	0.028***	-0.162***	0.019***	-0.174***	0.013***	-0.274***	0.018***	-0.219***	0.009**	-0.250***
	(0.005)	(0.069)	(0.004)	(0.054)	(0.004)	(0.051)	(0.005)	(0.055)	(0.004)	(0.051)	(0.004)	(0.049)
$ComLang_{ij}$	0.067***	0.746***	0.061***	0.679***	0.068***	0.707***	0.071***	0.836***	0.064***	0.696***	0.071***	0.735***
	(0.003)	(0.074)	(0.003)	(0.066)	(0.003)	(0.07)	(0.003)	(0.076)	(0.003)	(0.067)	(0.003)	(0.071)
EV_{ijpt}	0.006***		0.005***		0.005***		0.005***		0.005***		0.005***	
	(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)	
η_{ijpt}		833.6***		686.4***		511.7***		711.0***		554.4***		431.5**
		(222.7)		(177.1)		(196.9)		(162.2)		(152.3)		(177.4)
z_{ijpt}		14,043***		11,484***		8,009**		13,819***		9,959***		7,355**
		(4,074)		(3,348)		(3,664)		(3,422)		(3,122)		(3,542)
z^2_{ijpt}		-14,668***		-11,896***		-7,933*		-15,525***		-10,655***		-7,590*
		(4,493)		(3,757)		(4,078)		(4,068)		(3,657)		(4,093)
z^3_{ijpt}		5,699***		4,562***		2,874*		6,480***		4,199***		2,856
		(1,859)		(1,583)		(1,703)		(1,818)		(1,610)		(1,777)
N	480,564	33,324	660,666	40,346	807,710	42,011	480,564	33,324	660,666	40,346	807,710	42,011
r2	0.407	0.675	0.394	0.680	0.375	0.680	0.404	0.677	0.393	0.681	0.373	0.681
Method	LPM	OLS	LPM	OLS	LPM	OLS	LPM	OLS	LPM	OLS	LPM	OLS
Fixed effects	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt	ipt, jpt

Sample 1 only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 = Sample 1 + use of default MRLs to replace missing MRLs.
Sample 3 = Sample 2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.
d is the first difference operator.
Product dimension in the fixed effects is at the HS-4 digit level.
Robust standard errors, clustered by dyad-product-year, included in parentheses.
R_{ijpt} refers to the Achterbosch-index R^{Ach}_{ijpt} in columns (1)-(6) and to the Winchester-index R^{Win}_{ijpt} in columns (7)-(12).
Levels of signicance: *p<0.1 **p<0.05 ***p<0.01

Table 7: RGFD estimates robust to non-dichotomization of relative stringency (R_{ijpt}) and to the use of the Winchester et al. (2012) index

	Achterbosch			**Winchester**		
	Sample 1	**Sample 2**	**Sample 3**	**Sample 1**	**Sample 2**	**Sample 3**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$	$d.ln(X_{ijpt})$
$d.R_{ijpt}$	0.788***	0.426***	0.608***	4.251***	0.467*	0.157
	(0.180)	(0.148)	(0.1000)	(1.552)	(0.273)	(0.231)
$d.ln(1+\tau_{ijpt})$	-1.784***	-0.803	-0.314	-1.866***	-0.892	-0.531
	(0.628)	(0.727)	(0.761)	(0.657)	(0.720)	(0.752)
N	22,702	27,028	27,948	21,982	27,028	27,948
r2	0.071	0.054	0.054	0.071	0.054	0.052
Method	RGFD	RGFD	RGFD	RGFD	RGFD	RGFD
Fixed effects	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij	d.ipt, d.jpt, ij

Sample 1 only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 = Sample 1 + use of default MRLs to replace missing MRLs.
Sample 3 = Sample 2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.
d is the first difference operator.
Product dimension in the fixed effects is at the HS-4 digit level.
Robust standard errors, clustered by dyad-product-year, included in parentheses
R_{ijpt} refers to the Achterbosch-index R^{Ach}_{ijpt} in columns (1)-(3) and to the Winchester-index R^{Win}_{ijpt} in columns (4)-(6).
Levels of signicance: *p<0.1 **p<0.05 ***p<0.01

Table 8: PPML results: stricter standards do not impede trade

	Achterbosch			**Winchester**		
	Sample 1	**Sample 2**	**Sample 3**	**Sample 1**	**Sample 2**	**Sample 3**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	X_{ijpt}	X_{ijpt}	X_{ijpt}	X_{ijpt}	X_{ijpt}	X_{ijpt}
R_{ijpt}	-0.229	-0.140	-0.433***	-5.032***	-2.285	0.467
	(0.176)	(0.199)	(0.155)	(1.103)	(1.533)	(0.408)
$ln(1+\tau_{ijpt})$	3.574***	3.937***	3.981***	3.561***	3.932***	3.989***
	(0.518)	(0.503)	(0.397)	(0.522)	(0.505)	(0.400)
N	286,875	400,083	489,833	286,889	400,100	489,817
Pseudo-r2	0.796	0.797	0.797	0.796	0.797	0.797
Method	PPML	PPML	PPML	PPML	PPML	PPML
Fixed effects	ipt, jpt, ij	ipt, jpt, ij	ipt, jpt, ij	ipt, jpt, ij	ipt, jpt, ij	ipt, jpt, ij

Sample 1 only includes observations where the importer and the exporter had an explicit MRL.
Sample 2 = Sample 1 + use of default MRLs to replace missing MRLs.
Sample 3 = Sample 2 + use of sample maxima to replace those missing MRLs which also lacked a default MRL.
X_{ijpt} is in USD millions.
Product dimension in the fixed effects is at the HS-4 digit level.
Robust standard errors, clustered by dyad-product-year, included in parentheses
R_{ijpt} refers to the Achterbosch-index R^{Ach}_{ijpt} in columns (1)-(3) and to the Winchester-index R^{Win}_{ijpt} in columns (4)-(6).
Levels of signicance: *p<0.1 **p<0.05 ***p<0.01

Table 9: List of included products

HS Code	Product	HS Code	Product	HS Code	Product	HS Code	Product
080211/2	Almonds	080920	Cherries	080710	Melons	080430	Pineapples
080810	Apples	080240	Chestnuts	100820	Millet	080940	Plums
080910	Apricots	070320	Garlic	071120	Olives	081020	Raspberries
070920	Asparagus	080221/2	Hazelnuts	070310	Onions	070970	Spinach
070930	Aubergine	081050	Kiwi	080510	Oranges	081010	Strawberries
080440	Avocados	080530	Lemons	080720	Papayas	080231/2	Walnuts
070410	Broccoli	080520	Mandarins	080930	Peaches	080711	Watermelons
070940	Celery	080450	Mangos	080820	Pears		

Annex Table A1: Definition of WGI, Source: Kaufmann, Kraay and Mastruzzi (2011).

Indicator	**Brief Definition**
(1) Control of corruption	Extent to which public power is exercised for private gain
(2) Government effectiveness	Perceptions of the quality of public services
(3) Political stability	Likelihood that the government will be destabilised or overthrown
(4) Regulatory quality	Ability of the government to formulate and implement sound policies
(5) Rule of law	Confidence in enforcement of contracts, property rights
(6) Voice and accountability	Participation in government, freedom of expression, association

Annex Table A2: Correlation-matrix of WGI

Indicator	**CoC**	**GE**	**PS**	**RQ**	**RoL**	**VaA**
Control of corruption (CoC)	1.000					
Government effectiveness (GE)	0.928	1.000				
Political stability (PS)	0.733	0.676	1.000			
Regulatory quality (RQ)	0.864	0.934	0.618	1.000		
Rule of law Confidence (RoL)	0.940	0.934	0.773	0.892	1.000	
Voice and accountability (VaA)	0.776	0.765	0.676	0.778	0.816	1.000

Annex Table A3: Principal component rotation

Component	**Eigenvalue**	**Difference**	**Proportion**	**Cumulative**
Comp1	5.054	4.608	0.842	0.842
Comp2	.446	.161	0.074	0.917
Comp3	.285	.166	0.048	0.964
Comp4	.12	.0691	0.02	0.984
Comp5	.051	.006	0.008	0.993
Comp6	.044		0.007	1.0000

Annex Table A4: Principal components (eigenvectors)

Indicator	**Comp1**	**Unexplained**
Control of Corruption	0.425	.086
Government Effectiveness	0.425	.085
Political Stability	0.358	.351
Regulatory Quality	0.413	.137
Rule of Law	0.434	.048
Voice and Accountabbility	0.388	.239