IMF Economic Review
Vol. 62, No. 1

Crisis Protectionism: The Observed Trade Impact

CHRISTIAN HENN and BRAD MCDONALD

This paper investigates how new discriminatory measures implemented since the start of the global financial crisis are affecting global trade flows. Newly available data on border measures and behind-the-border measures (for example, bailouts and subsidies) implemented through April 2010 are matched to monthly 4-digit bilateral trade data using a first-differenced gravity equation. The estimation strategy exploits within-product variation and utilizes extensive time-varying fixed effects and duad/tetrad ratio estimation to isolate as far as possible the impact of protectionist measures from that of other factors to estimate trade policy impacts. The estimates suggest that trade in country pairs subject to new border measures decreased by 5–8 percent relative to trade in the same product among pairs not subject to new measures. Identification of trade impacts of behind-the-border measures proves to be considerably more challenging, but, also for these measures, evidence points toward a negative impact, possibly of roughly the same magnitude. These results imply a decline in global trade of about 0.2 percent on account of crisis protectionism. [JEL F10, F13]
IMF Economic Review (2014) **62,** 77–118. doi:10.1057/imfer.2014.4;
published online 25 March 2014

The original research for this paper was conducted while the authors were economists in the International Monetary Fund (IMF). Christian Henn is currently also affiliated with the World Trade Organization, and Brad McDonald with the U.S. Department of the Treasury. The paper does not represent the position or opinions of the IMF or its Executive Board, the WTO or its Members, the United States Treasury, or the United States Government, or the position of any of these organizations' staff members, apart from the authors. Any errors are the responsibility of the authors. The authors thank seminar participants at the European Trade Study Group's 2011 Annual Conference in Copenhagen; the July 2011 WTO Symposium on "The Financial and Economic Crises and the Role of the WTO"; the July 2011 EcoMod conference in Ponta Delgada, Portugal; and the December 2011 Joint IMF-World Bank-WTO Trade Conference. They also thank Richard Baldwin, Cosimo Beverelli, Joe Francois, Alexander Keck, Rod Ludema, Håkan Nordström, Chris Papageorgiou, Ranil Salgado and two anonymous referees for suggestions on this research; and Tushara Ekanayake, Emmanuel Hife, Yoichiro Kimura, Ioana Niculcea, and Nicolas Young for outstanding research assistance.

With the global economic crisis of 2008–09 came concerns that some countries may resort to protectionism to try to deal with its consequences. In response, the World Trade Organization (WTO) and the independent watchdog Global Trade Alert (GTA) began to compile detailed information on new trade and trade-related measures. These monitoring exercises brought welcome transparency to governments' trade-related policy actions by documenting the wide variety of measures taken. What Evenett (2010) has called the "diversity in contemporary protectionism" is manifest as tariffs, export restrictions, "buy national" procurement provisions, bailouts, domestic subsidies, and other types of measures. But the value of these new databases goes beyond transparency: combined with detailed monthly bilateral trade statistics, they also allow for the first direct and comprehensive ex post quantification, based on a gravity equation, of the trade impact of various trade restrictive measures. This paper strives to provide this analysis.

The literature on the quantification of the trade impact of trade frictions follows two strands, the first "direct" and the second "indirect." Using the gravity equation as its workhorse, the first strand relates trade-related measures directly to trade to determine their impact. The major challenge faced in this research is that direct measures of trade costs are sparse and inaccurate (Anderson and van Wincoop, 2004). Consequently, applications have focused on the impacts of preferential trade agreements (Cipollina and Salvatici, 2010, provide a meta analysis), currency unions (Rose, 2000), and GATT/WTO membership (Rose, 2004; Eicher and Henn, 2011a). These studies typically do not identify the depth of trade opening achieved through these arrangements and resort instead to a dummy variable, taking the value of 1 after accession.[1] They generally use aggregate trade data and do not exploit product-specific data, although there are exceptions for certain types of measures. For example, Essaji (2008) combined product-level trade data and official WTO notifications of technical regulations. By identifying the proportions of products or value of trade subject to these regulations within any product category, he then estimates the impact of technical barriers to trade.

Beyond this, studies are rare that directly quantify the trade impacts of other measures, which are less easily quantified but may affect trade flows. That is despite the apparent importance of such measures as licensing requirements, sanitary and phytosanitary measures, government procurement measures and domestic subsidies, for which direct data are sparse or unreliable. Nontariff measures are nowadays likely much more important in restricting trade than tariffs (Hoekman and Nicita, 2011). Kee, Neagu, and Nicita (2010) estimate their ad valorem tariff equivalent to be 39 percent. However, trade impacts of these measures have hardly been identified in a direct way.

The second strand of the literature proceeds in an indirect way. This literature starts by using domestic trade to obtain a benchmark for external trade values in a frictionless world. The difference between observed and benchmark external trade

[1]Some papers address this issue to some extent by disaggregating into individual arrangements currency unions (Eicher and Henn, 2011b) and preferential trade agreements (Eicher, Henn, and Papageorgiou, 2012).

is then used to obtain an estimate of total trade costs scaled by an industry-specific elasticity of substitution. Next, elasticities of substitution (obtained from separate estimation) are used to isolate total trade costs. Finally, these total trade costs can be decomposed, for instance into the effects of tariffs and other trade costs (Head and Ries, 2001).[2] A simulation literature builds on this second strand. It takes elasticities of substitution as given and then varies different types of trade costs to gauge their trade impact. These simulation models have been used in a wide range of applications to analyze policy changes on an ex-ante basis. The collection of studies in Francois and Reinert (1997) is broadly representative of the earlier literature, and UNCTAD/WTO (2012) provides a more recent overview. Large scale empirically based simulation models have been used to estimate the trade and welfare impacts of the actual Uruguay Round provisions (Francois, McDonald, and Nordstrom, 1996, provide a review) and to examine alternative scenarios in the Doha Round (Decreux and Fontagne, 2011). As useful as these studies are, their downside is that their results are not verifiable ex-post within their framework, because trade elasticities are estimated from past data and have to be assumed constant during the evaluation period.

This paper's main motivation is that the new databases on protectionist actions, together with high-frequency bilateral trade statistics now allow for the first time an ex-post gravity equation estimation of impacts of a broad range of trade policy measures. Impacts of many of these could previously only be estimated, if at all, in simulation studies and some in a rather vague fashion by varying indices of overall trade restrictiveness, for instance. Our paper also relates to a small literature analyzing protectionist actions in the aftermath of the 2008 global crisis. A study that takes a similar approach to ours is Shingal (2009), but he limits his analysis to Japan. Finally, a few studies dedicate themselves to analyzing particular types of trade measures. They include Kee, Neagu, and Nicita (2010), who simulate trade impacts of tariff responses to the global crisis. Bown (2010) assesses crisis-related developments in antidumping, safeguards, and countervailing duties. Bussiere and others (2011) use simulations to gauge the impact that a protectionist surge may have on broad macroeconomic variables.

Despite the richness of the new databases on protectionist measures, comprehensive ex-post gravity equation-based estimation of protectionist impacts faces several challenges. Given the wide variety of measures implemented, no comparable metric of the magnitude of protection afforded by each measure can be constructed. Our estimation strategy addresses this by focusing exclusively on *whether* a product has been subject to a new measure—an approach that nonetheless allows us to compare average protectionist impacts across *types* of measures, while also sidestepping aggregation issues. This approach is essentially analogous to that in the literature mentioned above on trade effects of trade agreements and currency unions.

[2]These decompositions of total trade costs, often undertaken through regressions, are again only limited by availability of quantifiable trade cost proxies. For instance, Chen and Novy (2011) analyze the impact of technical barriers to trade in such a framework.

Our analysis first pairs this dummy variable on protectionist measures with trade flow data. The first upshot, which has also been highlighted in various WTO reports (for example, WTO, 2013), is that protectionist measures only affected a small fraction of world trade flow values (3.57 percent in our data).[3] This immediately confirms that protectionism could not explain a significant fraction of the trade collapse of late 2008 and early 2009, when global trade value fell by more than 30 percent.[4] More probable causes of the great trade collapse have been well documented by the literature and include foremost amplified demand shocks (Bems, Johnson, and Yi, 2010), inventory adjustments (Alessandria, Kaboski, and Midrigan, 2010), and trade finance channels (Ahn, Amiti, and Weinstein, 2011).

Magnitudes of product-level impacts remain nonetheless of critical interest, including to judge what damage protectionism could do, if it were to become widespread. Our results indicate that new measures are substantially decreasing trade in country-pair/product combinations to which they apply (the “affected trade flows”). The typical border measure implemented between mid-2008 and early 2010 is associated with an average 5–8 percent drop in affected trade flows.[5] The evidence we are able to gather on behind-the-border measures also points toward a negative impact, possibly of roughly the same magnitude. However, identification issues related to limited data variation in a key dimension keep us from drawing strong conclusions on these behind-the-border impacts. Nevertheless, protectionist measures are far from innocuous, although their aggregate impact—estimated at 0.2 per cent of world trade—was muted by their limited adoption in the immediate aftermath of the global crisis covered by our sample (July 2008–April 2010). Crisis protectionist measures have been more harmful for exports of developing countries, particularly affecting those of the poorest nations. Interestingly, border restrictions hurt the developing world much more than trade partners’ bailouts of domestic firms. Rather, advanced economies’ behind-the-border measures mainly affect their peers.[6]

Our estimates have been derived from data covering the immediate postcrisis period, a very special juncture for world trade characterized by the big initial collapse and subsequent recovery. In this sense, it is unfortunate that the data on trade restrictive measures do not cover any period before the global crisis. But by carefully isolating the protectionist impacts on trade from those of other

[3]The 2013 WTO report also reports protectionist measures to cover 3.6 percent of world trade at the HS 8-digit level, but refers to a longer period, 2008 to mid-2013, during which more measures have been implemented than captured in our data. The divergence is caused by our trade data being more aggregate (HS 4-digit level), so that more trade is shown to be affected for any measure in our data.

[4]Other sources that have pointed out that protectionism could not have been a significant driver of the global trade collapse include Baldwin and Evenett (2009) and OECD (2010).

[5]Because many of the measures cover only certain subcategories of a 4-digit trade category (our level of analysis), these product-level estimates would probably be larger and more precise if 6-digit or 8-digit trade data were used; however, this bias does not imply that our approximation of aggregate trade impacts, discussed below, is also biased.

[6]See also Henn and McDonald (2011) for further details on these results by income levels and regions.

factors, we are confident that our estimates nonetheless provide insights into the effects of trade restrictions also during more normal times. Firstly, we control for nonprotectionist-related factors using extensive time-varying fixed effects, which have the power to address the large fluctuations in trade observed during the global crisis. Although large numbers of fixed effect controls are crucial in addressing omitted variable and simultaneity bias, in some specifications we find that they inadvertently also absorb much of the variation in trade needed to identify impacts of measures relatively uniformly across trade partners, such as of behind-the-border measures. We therefore devise an alternative estimation strategy relying on Duad and Tetrad ratios, which returns some further—but ultimately not conclusive—evidence that behind-the-border measures indeed decreased trade.

We believe that our estimates are also informative for noncrisis periods because of our use of extensive sets of controls to account for nonprotectionist trade determinants and endogeneity concerns. We take additional comfort from a recent WTO study (WTO, 2013), which follows our baseline methodology to analyze the impact of protectionist measures during the somewhat less turbulent 2011–12 period.[7] It also finds consistently negative protectionist impacts across various categories of measures.

The remainder of this paper is organized as follows. Section I presents the data. Section II illustrates graphically the impact of new discriminatory measures on product-level trade data. Section III sets out the baseline estimation strategy, and Section IV discusses the econometric results and extensive robustness checks. Section V concludes.

I. Data

Monthly bilateral 4-digit HS merchandise trade data were obtained under subscription from Global Trade Information Services (GTIS), a commercial service that compiles data from national statistical authorities. The data used here include the value of external imports and exports reported by the European Union (EU) and 14 other major G20 reporting countries.[8] The data cover some 80 percent of global merchandise trade, missing only those bilateral flows for which neither the exporter nor importer is among those 15 major reporters. For many reporters, the trade data in levels cover the period of July 2007–April 2010; data for all reporters were available through December 2009.[9] After year-over-year log differences in trade flows are constructed for our dependent variable, the data series starts in July 2008.

[7]It is furthermore noteworthy that comparable results emerge, despite the WTO relying on their own monitoring database for data on protectionist measures, instead of GTA. However, a drawback of this study is in our view that no product-specific controls are introduced, though these may be somewhat less important during a noncrisis period when trade growth across products does not vary as dramatically as during 2008–09.

[8]As Germany, France, Italy, and the United Kingdom are EU members, all G20 countries except Saudi Arabia are covered.

[9]For most reporters, the trade flow data extend through either March or April 2010. Exceptions are India (December 2009) and the EU-27, Indonesia, and Mexico (February 2010).

The regressions thus draw on 9.9 million monthly observations of import values in country-pair-product combinations.

The information on discriminatory measures is drawn from GTA. In assembling its database, GTA allows policymakers, government officials, exporters, the media, and analysts to report discriminatory measures and draws upon expertise and analysis from seven independent research institutions around the world. Measures are then verified by GTA as far as possible and classified into three categories: red, amber, and green. “Red” measures are those judged by GTA as almost certainly discriminating against foreign interests. Measures for which there are doubts about the discriminatory impact are classified as “amber,” and liberalizing or transparency-improving measures are classified as “green.” GTA started recording measures in mid-2008 and virtually all measures in the GTA data were implemented after July 2008.

With some exceptions, the GTA database provides, for each measure: (i) the implementing country, (ii) affected trade partners, (iii) affected 4-digit Harmonized System (HS) product categories, (iv) month of implementation (and removal, if any), and (v) a description of the measure. The determination of affected trade partners is a unique and useful feature of the GTA data.[10] To maintain a close focus on protectionism, the analysis here includes only “red” measures.[11] It is based on measures reported by GTA as of the beginning of June 2010, at which time 508 GTA measures met these criteria. Of these, 314 featured all information necessary to be included in our analysis. The 194 excluded protectionist measures (38 percent of the total) were mostly behind-the-border measures. Although the GTA database provides numerous behind-the-border measures, they were mostly financial sector bailouts, for which there is no corresponding merchandise trade sector assigned. Consequently only 40 behind-the-border measures met our requirement of providing data on all of (i)–(v).

For this study, the 314 included measures were classified as import restrictions, export restrictions, export support measures, or behind-the-border measures, based on the GTA descriptions. Among import restrictions, subclassifications used are (a) tariffs and import bans, (b) trade defense measures, consisting of anti-dumping, countervailing duty, and safeguard measures, (c) nontariff barriers (NTBs), and (d) discriminatory purchasing policies, such as government procurement

[10]To determine affected trade partners for import policy changes, the GTA team relies as far as possible on information available on the measure directly. If no concrete information on trade partners to which the measure applies can be retrieved in this fashion, GTA reports all trading partners that export one million dollars or more of the product in question to the implementing jurisdiction (based on UN COMTRADE import data for the latest year available before the measure’s implementation).

[11]“Amber” measures are excluded given the uncertainty of their nature. “Green” measures were explored in initial stages of this research but were dropped from the analysis to maintain a close focus on protectionism. We do not believe that this decision introduces much selection bias for several reasons. First, there were relatively few (51) green measures during the sample period with complete data on (i)–(v). Second, like export measures, they normally affected most other trade partners, so are captured in time-varying importer-product fixed effects (in Table 2, specifications 4–6). Finally, the impact coefficients on GTA green measures obtained in our early research were close to zero and statistically insignificant throughout and did not impact any other measures.

Table 1. Summary of Measures Used in the Study

	Total	By Region of Implementing Country[1] Africa	Asia	Europe	LAC	North America
Protectionist measures reported by GTA[2]	508	68	181	163	75	21
Protectionist measures used in study[3]	314	50	132	47	70	15
Import restrictions	239	42	97	23	65	12
Tariffs and import bans[4]	99	29	41	4	22	3
Trade defense	102	4	45	13	33	7
Nontariff barriers	16	5	4	0	7	0
Discriminatory purchasing	22	4	7	6	3	2
Behind the border measures[5]	40	2	16	18	3	1
Bailouts	27	0	14	11	1	1
Domestic subsidies	7	0	1	5	1	0
Investment subsidies	6	2	1	2	1	0
Export restrictions	19	4	14	0	1	0
Export support[6]	16	2	5	6	1	2

[1]Countries categorized into regions using World Bank classification. Africa includes the Middle East. Asia includes Russian Federation. Latin America and Caribbean (LAC) includes Mexico. For convenience, Appendix Table A2 in the working paper version (Henn and McDonald, 2011) provides the regional classification of all countries in the data set.

[2]Implemented measures reported as of end-June 2010 by Global Trade Alert as almost certainly discriminating against foreign commercial interests ("red" measures).

[3]Number of measures with complete data on implementing jurisdiction, affected jurisdiction, and affected products among those described under footnote 2.

[4]Also includes import quotas and competitive devaluations.

[5]For our purposes we define behind-the-border measures as those consisting of direct discriminatory assistance to domestic firms. A priori, these measures could be expected to cause a decrease in imports and/or an increase in exports.

[6]Largely consists of export subsidy measures.

provisions and consumption subsidies tied specifically to the purchase of domestically produced products. Behind-the-border measures are separated into bailouts, domestic subsidies, and investment subsidies. Import restrictions account for over three-quarters of all measures used here, with behind-the-border measures accounting for about half of the remainder (Table 1).[12] Export restrictions and export support measures are fewer than 20 each, which, in light of the volatile disaggregate trade data, makes it difficult to establish significant impacts for these measures. We exclude them from much of our analysis, but return to them when discussing robustness.

The stock of protectionist actions grew over time in our sample, with few measures being removed (Tables 10 and 11 in Henn and McDonald, 2011, provide summary statistics). Of our total 279 import restrictions and behind-the-border measures, most were implemented after the great trade collapse, when global trade flows

[12]Appendix Table A1 in Henn and McDonald (2011) contains detail on each individual measure included and its classification.

were stabilizing during February–May 2010 (76 measures) and thereafter as trade was recovering (170 measures).[13] This gradual implementation of measures underpins our motivation in using trade flows of the last quarter of 2009 for our approximation of aggregate trade impacts: Most protectionist measures are in force at the end of our sample period and the last quarter of 2009 is the last to cover all reporters in our data set. In our discussion of aggregate impacts, we concentrate on the percentage of world trade affected, because, given the turbulent times under study, corresponding dollar values varied considerably.

II. A First Peak at the Protectionist Impact

Consider how trade evolved in a particular 4-digit product category in the months after a new import restriction was imposed. In particular, compare trade in affected country-pair combinations to global trade in the same product. Comparing affected and unaffected country-pairs *within* a product separates the impact of the new restriction from worldwide product-specific influences. This is particularly important for the sample period because of large product heterogeneity: for instance, trade in durables fell much more than did trade in nondurables after the onset of the crisis (Baldwin and Taglioni, 2009; Levchenko, Lewis, and Tesar, 2009). Data presented in this literature show that trade developments were also heterogeneous across countries, although to a lesser extent (Baldwin, 2009; Martins and Araujo, 2009). The ratio of trade in the affected country-pairs to global trade in the same product is normalized to 100 on average for the 12 months ($T-12$ to $T-1$) preceding the imposition of a new import restriction. We expect that ratio to fall when a new measure is implemented.

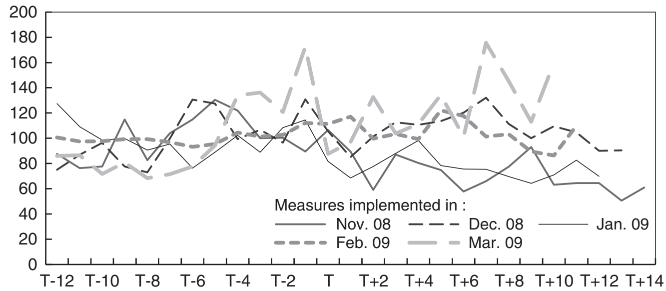
Figure 1 presents the raw data organized this way, aggregated by the month in which a new import restriction was implemented, and charts these series for implementation months through March 2009.[14] The series are volatile, as would be expected in such detailed trade data. For certain months of measure implementation, such as November 2008 and January 2009, a clear downward trend in the market share of protected country-pair product combinations is visible, with all months after T showing lower values than in the year before measure imposition. For other implementation months, the evidence is more mixed.

To obtain a better notion, the series in Figure 1 are averaged over implementation months. Figure 2 presents the results for two weights used in averaging: (i) the number of observations affected, and (ii) the value of trade affected. With (i) there seems to be evidence of market share declines in protected country-pair product combinations after measures were imposed. With (ii), however, the series is very volatile and the evidence is less clear; this results from the March 2009 implementation month being assigned a high weight, because these measures affected large trade flows (but few observations). From the

[13]The measures implemented during February–May 2010 were those that affected the largest trade flows.

[14]For visual clarity, Figure 1 shows only the (early) implementation months for which the longest time series are available in our database.

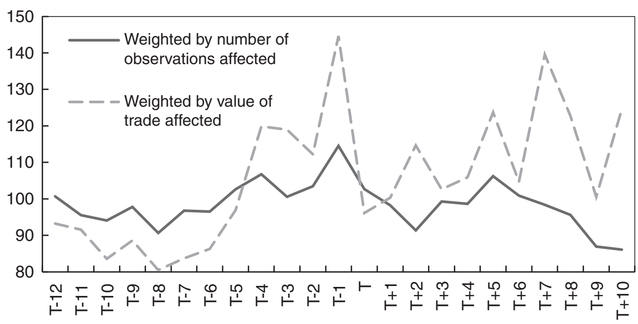
Figure 1. Relative Performance of Trade Affected by Import Restrictions (by month import restrictions were implemented; avg. T–12 to T–1 = 100)[1]



Source: Authors' calculations.

[1]Value of trade in countrypair-product combinations subject to import-restricting border measures implemented in month T divided by world trade in the same 4-digit HS product categories and normalized to equal 100 for the average during $T-12$ to $T-1$. Only series with observations through $T+10$ are included. The series for October 2008 (the first month with implemented measures) is omitted because measures affected few country pair-product combinations, resulting in an excessively volatile series.

Figure 2. Averaged Relative Performance of Trade Affected by Import Restrictions (averages over implementation months; avg. T–12 to T–1 = 100)[1]



Source: Authors' calculations.

[1]The graph shows different weighted averages (over implementation months) of the series shown in Figure 1. Measures included in this graph (implemented up to March 2009) cover more than 80 percent of total trade affected by measures in the study.

standpoint of our regression analysis, which will follow and minimizes the sum of squared residuals across all observations, the observation-weighted measure is likely more informative.

Notwithstanding, both series deliver the same information once analysis is taken one step further. Whether trade fell after a new measure was implemented naturally depends on the base period used for comparison. Month T–1, just before imposition of the measure is arguably the most relevant, because it gives the most up-to-date market share before measure implementation. In such volatile data, however, trade may have been abnormally high (or low) at T–1. It is therefore informative to compare average postprotection imports (during T through $T+10$) also to $T-2$, $T-3$, and so on. Doing so shows that postprotection imports were lower relative to the months just before imposition of the measure (any one of $T-1$ through $T-4$), regardless of the averaging technique used. When instead any month of $T-5$ to $T-12$ is used as the reference period, postprotection trade may have been about the same as preprotection trade (in case of the observations-weighted averaging) or higher (in case of the trade-weighted averaging). We draw two conclusions: (i) market shares of country-pair products hit by new import restrictions likely fell upon their implementation compared with the months immediately before and (ii) new measures were targeted, perhaps intentionally (and endogenously), at those exporters which had been gaining market share in the run up to the imposition of measures. Our regression results further support these conclusions.[15]

Behind-the-border measures show a somewhat different picture. Behind-the-border measures are defined as direct assistance to domestic firms that is discriminatory, that is, not available to foreign firms exporting to the domestic market. To cover the most relevant behind-the-border measures in the sample without unduly shortening available time series, measures implemented up to June 2009 are included in the figure. These cover 80 percent of the imports affected by behind-the-border measures during the entire sample.

Figure 3 shows that exports affected by domestic governments' discriminatory aid to home producers indeed fell relative to world trade in the same products. However, it is not clear whether this decline occurred mainly after T, that is, in response to the protectionist measure, or it already started earlier (during $T-12$ to $T-1$) and may actually have accelerated already before measure implementation.[16] If such declines had already been ongoing, we may be mistaking a particularly weak import market for a protectionist effect. With the crisis causing large product-specific demand shocks, weak import markets are likely correlated with weak domestic demand overall for the same product.[17] To the extent that this left the domestic industry in a precarious state, local policymakers may have reacted to aid these industries. This underlines the importance of controlling in our regressions for importer-product specific effects to avoid overestimation of behind-the-border impacts. In specifications where these controls are not in place, for example, to maximize identifying variation, behind-the-border impacts will need to be interpreted with particular caution.

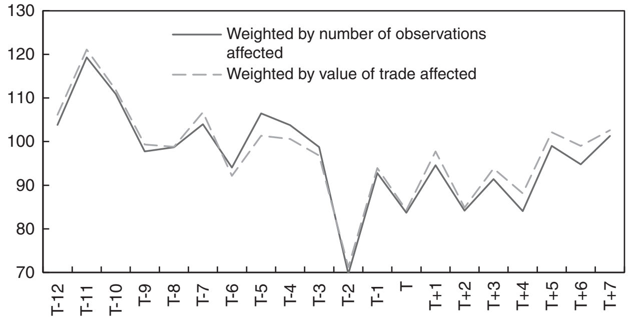
---

[15]The econometric results in Section IV give qualitatively similar results. Table 3, Regression 1 reports most analogous results to the figures presented here. These econometric results are superior to the graphical analysis because the regression's minimization of squared residuals provides a consistent way of weighing over (i) implementation months and (ii) products affected within each implementation month.

[16]The strong decline at $T-2$ is driven by measures in one particular implementation month, but nonetheless it cannot be ruled out that an acceleration of declines already began before month T.

[17]In the figures, whether falls in market shares of exporters hit by protectionism are due to the protectionist actions themselves or general collapses of certain country-specific markets cannot be well distinguished, because the market shares of exporters hit by protectionism are calculated relative to the *global* market in the same product. The regression analysis addresses this.

Figure 3. Averaged Relative Performance of Imports Affected by Behind-the-Border Measures (averages over implementation months; avg. *T*–12 to *T*–1 = 100)[1]



Source: Authors' calculations.
[1]The graph shows different weighted averages (over implementation months). Measures included in this graph (implemented up to June 2009) cover more than 80 percent of total trade affected by measures.

Unreported analogous figures for export restrictions and export support measures suggest that trade affected by export support measures increased, as expected.[18] Contrary to initial intuition, though, trade in products subject to export restrictions actually increased. This latter observation is based on a small sample, and may reflect a tendency to use export restrictions in periods of regional or global supply shortages.

III. Estimation

Econometric analysis improves on the graphical analysis above by allowing for more extensive control of variations in trade that are unrelated to policy changes. The regressions provide estimates of the trade impact of the discriminatory measures at the 4-digit product level. In a second step, we combine these coefficient estimates and the information on the value of trade flows to obtain a first approximation of the aggregate impact of the discriminatory measures on global trade.

The Estimation Equation

Given the context of the global crisis, it is crucial to identify the protectionist impact at the product level. The scope of new protectionist measures was not widespread, so attempting to identify any impact of protectionism directly at the aggregate level would be problematic. In the 4-digit data, new protectionist measures were applied only to 3½ percent of global trade, making it difficult to identify any effect in aggregate figures.[19] Furthermore, the collapse in global

[18]These figures are included in Henn and McDonald (2011).

[19]This figure is for 2009:Q4, the last period for which trade data are included for all reporters (see footnote 9 “For most reporters, the trade flow data extend…”).

trade at the onset of the global crisis was largely because of a demand shock and inventory adjustment rather than to protectionism, but coincided with the implementation of many protectionist measures in the sample.[20] Digging down to the product level affords a better chance to separate the effects of protectionism from those of these other factors through the extensive use of time-varying fixed effects.

In deriving our estimation equation, we start with the following gravity equation. This is a multiplicative version of a standard gravity equation, specified at the product level, which anticipates that distance, income, and other non-protectionist determinants will ultimately be controlled for by differencing and fixed effects:[21]

$$Imports_{ijpt} = \exp\left(\omega_{ijp} + \delta_{ipt} + \delta_{jpt} + \gamma_{ijt} + \beta Prot_{ijpt} + \varepsilon_{ijpt}\right), \tag{1}$$

where $Imports_{ijpt}$ is bilateral import value in U.S. dollars and i, j, p, and t index importers, exporters, 4-digit HS product categories, and months, respectively. Time-constant country-pair-product-specific determinants are captured by ω_{ijp}, whereas δ_{ipt} and δ_{jpt} denote respectively time-varying importer-product and exporter-product-specific determinants, including multilateral resistance terms (Anderson and van Wincoop, 2003). Time-varying country-pair-specific determinants are depicted by γ_{ijt} (and are constant across products). $Prot_{ijpt}$ is a count variable of the number of protectionist measures by which an observation is affected. The error term ε_{ijpt} has a mean of zero.

As our aim is to quantify short-run trade responses, a gravity equation in first differences is the appropriate approach and has several advantages with regard to addressing potential endogeneity bias as well as computational constraints. We focus on setting out the equation here and then discuss its advantages further below. We divide Equation (1) by its 12-month lagged version to obtain:

$$\begin{aligned}\frac{Imports_{ijpt}}{Imports_{ijpt-12}} = \exp(&\delta_{ipt} - \delta_{ipt-12} + \delta_{jpt} - \delta_{jpt-12} + \gamma_{ijt} - \gamma_{ijt-12} \\ &+ \beta\left(Prot_{ijpt} - Prot_{ijpt-12}\right) + \varepsilon_{ijpt} - \varepsilon_{ijpt-12}).\end{aligned} \tag{2}$$

Taking logs then yields our estimation equation (3):

$$\Delta_{12} \ln\left(Imports_{ijpt}\right) = \Delta_{12}\delta_{ipt} + \Delta_{12}\delta_{jpt} + \Delta_{12}\gamma_{ijt} + \beta\Delta_{12}Prot_{ijpt} + \Delta_{12}\varepsilon_{ijpt}, \tag{3}$$

[20]Recent research suggests that the trade collapse of 2008–09 was attributable to three main factors: (i) a sharp decline in the production and trade of durable goods (which account for a much larger share of global trade than of production), (ii) supply chain and inventory adjustment effects, and (iii) somewhat tighter trade finance. See Baldwin (2009); Levchenko, Lewis, and Tesar (2009); and Anderton and Tewolde (2011). Henn and McDonald (2010) and Ahn, Amiti, and Weinstein (2011) provide overviews.

[21]Equation (1) is closely analogous to the specification used in Baier and Bergstrand (2007). See, for example, Anderson and van Wincoop (2003) for a derivation of the gravity equation.

where Δ_{12} indicates differences between month t and month $t-12$ values and $\Delta_{12}\delta_{ipt}$, $\Delta_{12}\delta_{jpt,}$ and $\Delta_{12}\gamma_{ijt}$ represent changes in importer-product-time, exporter-product-time, and country-pair-time-specific determinants, respectively. The residual $\Delta_{12}\varepsilon_{ijpt}$ would absorb any remaining trade determinants unrelated to protectionism that vary in all i, j, p, and t dimensions. In our baseline specification, the $\Delta_{12}\delta_{ipt}$, $\Delta_{12}\delta_{jpt}$, and $\Delta_{12}\gamma_{ijt}$ terms will then be controlled for through different sets of fixed effects to reign in potential endogeneity bias; more on this below.

Our protectionist variable can only exploit information on *whether* a product has been affected. This is because we aim to deliver comprehensiveness across measures and due to constraints imposed by GTA data recording. For instance, information on the magnitude of a new measure (for example, a tariff change) is not exploited.[22] However, splitting the protectionist dummies by types of measures will give a sense of the importance of those measures that could be reliably quantified (such as tariff changes) relative to others. Our protectionist indicator is a count variable that identifies the number of protectionist measures affecting each observation.

Once initially constructed in a levels data set, the protectionist indicator variable is 12-month differenced just like the log import flows.[23] For ease of description, assume initially that any given importer-exporter-product combination were only affected by one protectionist measure (and the protectionist variable would thus be a pure 0–1 dummy). Then the protectionist variable $\Delta_{12}Prot_{ijpt}$ takes the value of 1 for the first 12 months after imposition of the measure and 0 thereafter. In the rare cases where a measure is again removed during our sample period, the protectionist variable will then take the value of -1 for 12 months after removal of the measure and 0 thereafter.[24] Given that the protectionist variable counts all the protectionist measures affecting each observation, it may take integer values larger than 1 (or smaller than -1) in cases where more than one measure is introduced (removed). This coding facilitates the interpretation of β coefficients as the average trade impact of a single measure.[25]

---

[22]Baier and Bergstrand (2007) highlight that this may induce measurement error bias which could lead to underestimation of protectionist impacts (in absolute terms). However, they judge this type of potential bias much less important in estimating the impact of trade policy variables than omitted variable and simultaneity bias—on which we extensively comment below.

[23]Note that 12-month differencing of fixed effects dummies is not necessary, but interpretation of their coefficients changes as they come to quantify impact of *changes* in determinants.

[24]This description is correct if the measure has been in place at least for one year before removal, which is the general case in the data. In cases of earlier removal, the differenced protectionist dummy first takes the value of 1 for some months, then the value of 0 for the first months after removal, and the value of -1 for those months one year after those with "1" values.

[25]Restricting this to binary dummies does not have an appreciable effect on the results (see Table 3 in Henn and McDonald, 2011), largely because very few affected country-pair-product observations are subject to more than one protectionist measure. See footnote 49 "Import restrictions, our main variable of interest, break down as follows. Observations affected…".

Two Types of Potential Endogeneity Bias

Baier and Bergstrand (2007) highlight two major sources of endogeneity bias in gravity equations.[26] These two sources are omitted variable bias and simultaneity bias.

Omitted variable bias in the protectionist coefficient arises if any trade flow determinants omitted from the right-hand side are correlated with protectionism. An extensive literature documents the difficulties of controlling for the multitude of determinants that impact trade flows, many of which are unobservable or not quantifiable, and may be correlated with protectionism.[27] For instance, a country may impose protectionist actions primarily on partners with which it does not have strong cultural affinities or overriding political objectives. To the extent that those affinities and objectives do not vary over time, they would be controlled for by the differencing in Equation (3). Furthermore, any protectionist action also affects multilateral trade costs in addition to bilateral trade costs (Anderson and van Wincoop, 2003), and the former need to be properly accounted for to avoid omitted variable bias. Given that these multilateral trade cost terms vary over time, comprehensively accounting for them requires additional fixed effects in Equation (3).

Simultaneity bias can arise, because imposition of a protectionist action is a political choice and cannot necessarily be expected to be randomly assigned with regards to trade flows. For instance, protectionist actions will be correlated with trade flow *levels*, if countries target protectionist actions primarily at exporters selling large amounts into the domestic market.[28] This would be addressed by the differencing in Equation (3). Moreover, protectionist actions could be correlated with trade flow *changes*, if countries target trade flows in those products, where domestic imports have recently surged or collapsed. This can be addressed by adding fixed effects to Equation (3).

We address these sources of endogeneity bias in two steps. First, differencing our gravity equation as illustrated in Equation (3) takes care of *time-constant* trade determinants. Second, extensive fixed effects are added to this differenced gravity equation to control for *time-varying* trade determinants.

---

[26]With this contribution, Baier and Bergstand started as well a literature on the endogenous formation of Preferential Trade Agreements based on preexisting trade flow patterns. See also Egger and others (2011).

[27]Since Hummels and Levinsohn (1995) first emphasized large unobserved bilateral heterogeneity in gravity equations, a considerable literature has pointed out that gravity estimates may suffer from considerable omitted variable bias if these time-invariant country-pair-specific unobservables are not controlled for via first differencing or country-pair fixed effects (for example, Baldwin and Taglioni, 2006).

[28]To be exact, and as Baier and Bergstrand (2007) point out, these trade flow levels would have to exceed “natural” levels, as predicted by the gravity equation.

Controlling for *Time-Constant* Trade Determinants Unrelated to Protectionism

The 12-month differencing undertaken in Equation (3) controls for any time-constant country-pair-product-specific determinants captured by ω_{ijp}.[29] These comprise many long-run determinants of trade commonly included in gravity equations such as distance and other geography variables, common language, and colonial relationships, but also time-invariant unobservables. In light of the short sample used in our estimation, differencing also addresses most slow-moving trade determinants such as the general trade policy stance, transport infrastructure, costs of doing business, and political relationships, which may also influence decisions to impose protectionism.[30] Therefore, differencing is a crucial first step to limit potential omitted variable bias. As highlighted above, differencing likewise also eliminates any simultaneity bias arising from political decisions to impose protectionist actions based on existing trade levels.

Twelve-month differencing has two further advantages in our application. First, it addresses seasonality. Second, it is the best way to control for country-pair-product determinants ω_{ijp} also from a computational perspective because (a) the estimation still needs to include various sets of time-varying fixed effects and (b) the number of sets of fixed effects that can be included in the estimation is limited to two. The limitation to two fixed effects arises because the panel (i) is unbalanced, (ii) includes a large number of observations and (iii) has high-dimensional fixed effects. High dimensionality implies that thousands of dummy variables would have to be created (such as for time-varying product fixed effects), because many different products are observed at many different points in time. As each dummy would be 9.9 million observations long, computer memory constraints bind. In an unbalanced panel, traditionally these constraints implied that only one high-dimensional fixed effect could be considered by transforming the estimation equation (Greene, 2003).[31] Labor economists have devised solutions to the challenges of multiple high-dimensional fixed effects, starting with approximations in Abowd, Kramarz, and Margolis (1999). Guimaraes and Portugal (2010) provide an iterative technique to obtain exact estimates of equations with high-dimensional fixed effects in a computationally manageable way. Thus, after using first-differencing to eliminate time-constant country-pair-product-specific

---

[29]The consistency of the estimate for β is unaffected by whether differencing or *ijp* fixed effects are used as controls for these determinants. However, if serial correlation in the error terms of the gravity equation in levels is likely, as in our case, estimation in differences increases the efficiency of the estimator vis-à-vis country-pair-product fixed effects (Wooldridge, 2002, chapter 10.6; Baier and Bergstrand, 2007).

[30]If these determinants vary considerably over time, time-varying country-pair controls to proxy for γ_{ijt} become important, as included in our specification 3 below. Controlling for these time-varying country-pair-specific influences is only possible in product-level data, because for aggregate trade data the *ijt* level is the level of individual observations.

[31]In a balanced panel, two fixed effects could be stripped algebraically.

determinants, their technique allows the inclusion of two sets of time-varying fixed effects.[32]

The 12-month differencing does introduce an MA(11) process into the estimation (Hansen and Hodrick, 1980). Coefficient estimates will remain consistent, but OLS standard errors would be biased downward. We address this by using robust standard errors clustered by our panel identifier (Rogers, 1993), that is, at the country-pair-product level. These robust clustered standard errors address both autocorrelation and heteroskedasticity across clusters and are designated as the preferred choice in Stock and Watson (2008) for any application with a possibility of autocorrelation. Fortunately, the routine by Guimaraes and Portugal (2010) for handling simultaneously two high-dimensional fixed effects allows for the computation of these Rodgers standard errors. For robustness, in our regression specifications that only have one fixed effect, we also computed two alternative standard errors that correct for autocorrelation, Newey West (1987) and Driscoll Kraay (1998).[33] Reassuringly, results remained unchanged.

Controlling for *Time-Varying* Trade Determinants Unrelated to Protectionism

Our baseline estimation strategy relies greatly on time-varying fixed effects to capture the time-varying $\Delta_{12}\delta_{ipt}$, $\Delta_{12}\delta_{jpt,}$ and $\Delta_{12}\gamma_{ijt}$ determinants in differenced equation (3).[34] Given that the global crisis was marked by large fluctuations in global trade, and particularly in certain durable products, these time-varying fixed effects are crucial to isolate the impact of protectionism. For instance, in the absence of time-varying (importer-)product effects, large falls in demand for certain products could lead to overestimation of protectionist trade diversion on account of omitted variable bias. Furthermore, *ipt* and *jpt* effects are necessary to capture time-variation in multilateral price terms, although potential bias from this source is likely much less than that from demand swings during the global crisis.[35]

[32]In our estimations, we are mindful that Guimaraes and Portugal's (2010) methodology can only accommodate two fixed effects at a time. Working around this does not impose a major constraint, however, because two different sets of fixed effects can often be easily combined into a more general one, as is the case for TVCP effects as mentioned below.

[33]The Newey West (1987) and Driscoll Kraay (1998) standard errors were implemented with a 12-month bandwidths for both regressions 1 and 4. Driscoll Kraay (1998) standard errors in addition allow for cross-sectional dependence (across country-pair-product categories). Among the three types of standard errors, the Rodgers (1993) standard errors were the largest in most cases.

[34]Aside from that, there are no available analogues of some variables traditionally used in gravity equations (for example, GDP) at the product level and at monthly frequency, making fixed effects indispensable. In addition, the time-varying country-pair fixed effects used to capture the term $\Delta_{12}\gamma_{ijt}$ are allowed to differ between flows from country *i* to *j* and country *j* to *i*, because trade policies may vary in the different directions.

[35]Furthermore, controls along the *ijt* dimension would absorb any changes in bilateral trade costs, which could possibly induce bias. For example, if exchange rate swings or better transport connections lead to higher imports and occurred around the same time as a protectionist action between trade partners, the protectionist coefficient would be biased toward zero.

The time-varying fixed effects also serve an important role in addressing potential simultaneity bias. As will be seen in the baseline results discussion (Section IV), policymakers' decision may take into account changes in imports in determining protection. For instance, increases in imports could trigger higher demand for protection by the corresponding domestic industry—this would be controlled for by *ipt* effects. Similarly, with firms increasingly acting on the global stage, some may seek to persuade policymakers to aim protectionist actions at competing exporters whose sales have been increasing—this would be controlled for by *jpt* effects.

Identification

There is a trade-off involved in our attempts to comprehensively control for $\Delta_{12}\delta_{ipt}$, $\Delta_{12}\delta_{jpt}$, and $\Delta_{12}\gamma_{ijt}$ through fixed effects, because the more comprehensive the controls, the less variation could be available to identify protectionist impacts. How stark this trade-off is depends on how much identifying variation exists in particular dimensions. Although not an issue for protectionist measures with strong variation in all i, j, p, and t dimensions, evaluating measures that do not have strong variation along one of the dimensions can be challenging. Of particular concern is the variation among exporters within importer-product-time combinations affected by protectionist actions. The reason is that importer-product-time controls to account for $\Delta_{12}\delta_{ipt}$ are important for asserting the reliability of estimates, given that demand shocks varied considerably among importing countries and products during the global crisis. Once these controls are included, identification relies solely on variation among exporters within these importer-product-time combinations.

We have therefore devised some summary statistics to shed light on the amount of variation among exporters within those *ipt* combinations affected by protectionism. As expected, these statistics suggest that there is relatively ample variation among import restrictions imposed at the border.[36] However, variation is considerably more limited among measures that by their nature apply uniformly to most or all exporters. These include behind-the-border measures, which inter alia comprise bailouts of domestic firms, and also some subcategories of import restrictions, such as "buy local" public procurement policies.[37]

[36]In devising these summary statistics, we first obtain a ratio of nonzero observations over total observations of the protectionist variable for each importer-product-time combination affected by protectionism. This ratio will always be positive and reaches its maximum value of unity when a measure is uniformly applied to all trade partners. Given that we have such a measure for each importer-product-time combination, we can then evaluate frequency distributions. This distribution has the following mean, first quartile and median for import restrictions respectively: 0.756; 0.667; and 0.800. Behind-the-border measures are considerably more uniformly applied among exporters with analog values here being 0.888; 0.857; and 0.947. Public procurement measures are a subcategory within import restrictions with relatively little variation (0.854; 0.750; 0.917).

[37]There may also be an issue related to the data recording by Global Trade Alert, described in footnote 10 "To determine affected trade partners for import policy changes, the GTA team relies as far as possible… ." Their imposition of a threshold for listing affected exporters in some cases may cause measures that are uniformly applied across trade partners to be falsely identified as nonuniform.

As will be seen now in the results discussion, this relative dearth of variation seems to reflect also in the form of insignificant behind-the-border estimates with values very close to zero in our baseline fixed effect specifications including *ipt* controls. Although we subsequently manage to gather further evidence pointing to negative behind-the-border effects—like those found in baseline specifications without *ipt* controls—we are unable to resolve this trade-off sufficiently to draw strong conclusions on behind-the-border measures' impact.

IV. Results

Baseline Results

Our baseline regressions focus on *average* effects of all import restrictions and all behind-the-border measures, respectively, by introducing a separate protectionist count variable for each of these two.[38] At this point, the protectionist dummy is not yet split into subcategories of measures such as tariffs, antidumping duties, and NTBs. The regression specifications introduce different sets of fixed effect controls to capture time-varying trade determinants unrelated to protectionism. These controls address either partially or completely $\Delta_{12}\delta_{ipt}$, $\Delta_{12}\delta_{jpt}$, and $\Delta_{12}\gamma_{ijt}$. We introduce these fixed effects gradually, moving toward increasing comprehensiveness. This can provide us with clues regarding the importance of different types of shocks in driving changes in trade and their overlap with protectionist measure implementation. It can also inform us about the potential for bias in certain specifications.

Trade impact of import restrictions

Table 2 presents the results for our six different fixed effects specifications. We first focus on the impact of import restrictions.

Specification 1, the most basic specification, includes time-varying product fixed effects (TVP). The reaction of different products during the great trade collapse was highly heterogeneous. TVP controls are therefore crucial in disentangling protectionist influence in light of our sample period and are maintained throughout all regressions. The TVP regression is also the closest econometric analogue to the graphical analysis in Section II. In this specification, estimates of β thus evaluate whether, for a given product and month, those country-pair relationships affected by an import restriction experienced a relatively larger decline in trade.[39] The TVP results identify a statistically significant average trade decline of around 4.7 percent ($=e^{-0.048}$) in response to such a protectionist

[38] Results for regressions that also include export measures are reported in Table 3.

[39] Including TVP fixed effects implies that the estimation of β in Equation (3) relies exclusively on cross-sectional variation. This strategy is similar to that of Amiti and Weinstein (2009), who study the impact of the 1992/93 Japanese banking crisis on Japanese exporters. Their estimation also only uses cross-sectional information at each point in time, comparing whether the performance of exporters in the same sector varied depending on how much their main bank was impacted by the banking crisis.

Table 2. Baseline Fixed-Effect Results

Estimation of Product-Level Trade Impact[1]

Time-Varying Fixed Effects	Product	Product & Importer	Product & Country-Pair	Importer-Product	Imp.-Prod. & Exporter	Imp.-Prod. & Exp.-Prod.
Regression No.	1	2	3	4	5	6
Import restrictions	−0.048***	−0.050***	−0.051***	−0.076***	−0.084***	−0.083***
	(−5.09)	(−4.46)	(−4.77)	(−3.08)	(−2.94)	(−2.69)
Behind-the-border measures[2]	−0.165***	−0.092***	−0.073***	0.010	−0.005	−0.004
	(−10.86)	(−5.37)	(−4.53)	(0.16)	(−0.05)	(−0.03)
Hausman statistics (*p*-values in per cent) versus: [7]						
Regression 1		0.0	0.0	0.5	0.6	1.3
Regression 2			0.0	11.2	10.9	15.2
Number of time-varying fixed effects	27,896	32,910	128,833	2,574,781	2,579,648	3,819,552
Number of observations	9,878,481	9,878,481	9,878,481	9,878,481	9,878,481	9,878,481
Adj. R^2 (percent)	1.80	2.36	3.12	5.20	5.44	8.97

Table 2: (Continued)

Calculation of Aggregate Trade Impact [3,6]

	Number of Measures[4]	Affected Observations[5]	Affected Quarterly Trade[6]	Aggregate Quarterly Trade Impact Implied by Regression Number: *1*	*2*	*3*	*4*	*5*	*6*
Total	279		$77,668	–$7,313	–$5,177	–$4,568	–$2,794	–$3,605	–$3,537
		1.65%	3.58%	–0.34%	–0.24%	–0.21%	–0.13%	–0.17%	–0.16%
Import restrictions	239		$42,722	–$1,983	–$2,099	–$2,105	–$3,136	–$3,424	–$3,410
		1.11%	1.97%	–0.09%	–0.10%	–0.10%	–0.14%	–0.16%	–0.16%
Behind-the-border measures[2]	40		$34,946	–$5,330	–$3,078	–$2,462	$342	–$181	–$127
		0.54%	1.61%	–0.25%	–0.14%	–0.11%	0.02%	–0.01%	–0.01%

[1]*, **, *** denote 10, 5, 1 percent significance levels. *T*-statistics in parentheses, based on robust standard errors clustered by country-pair-product combinations. Underlying data are at monthly frequency. Regression coefficients express impacts in log units, which are very similar to percentage changes for values close to zero. The exact percentage change implied by any coefficient b can be calculated as exp(b)-1.

[2]Refers to the impact of behind-the-border measures on imports.

[3]Aggregate trade impacts are expressed as the change in trade due to protectionism *per quarter*. Impacts are calculated by multiplying product-level regression coefficients by the amount of trade in country-pair-product combinations affected by protectionist measures ("affected quarterly trade"). Calculations are based on 2009: Q4 data, the last quarter with data available from all reporters. As protectionist measures were implemented at different times but generally remained in place until end-2009 or longer, 2009:Q4 data are best suited to approximate the steady-state impact of protectionism on trade.

[4]"Red" measures from Global Trade Alert database for which complete data were available. See Appendix Table A1 in Henn and McDonald (2011).

[5]In percent of total observations in our data set. Calculations are based on trade flows covered by the data set in 2009:Q4.

[6]Expressed in U.S.$ millions and in percent of total trade. Calculations are based on trade flows covered by the data set in 2009:Q4. Aggregates in some tables may not equal the sum of their components, because the same trade flows may be affected by more than one measure.

[7]The null hypothesis is that the more parsimonious model, of regressions 1 or 2, is the correct model. Other Hausman tests not reported in the table suggest that model 4 cannot be rejected in favor of models 5 or 6. Results are broadly comparable if instead *F*-statistics are used for model comparison.

action.[40] However, the TVP fixed effects control only for global shocks to specific products, and thus control only for the product-specific components of $\Delta_{12}\delta_{ipt}$ and $\Delta_{12}\delta_{jpt}$. Therefore they would be biased if there were in addition time-varying importer, exporter or country-pair-specific determinants that are correlated with protectionist actions.

Specification 2 remedies this issue partly by adding time-varying importer fixed effects (TVIM). These control for any (observable or unobservable) change in an importer's trade determinants that affects all products equally. This setup controls for a larger part of $\Delta_{12}\delta_{ipt}$, leaving only its product-importer interaction uncontrolled for. For instance, import demand may have fallen more strongly among importers particularly exposed to the global crisis, because of high debt levels. These fixed effects also control for importer-specific multilateral trade costs (Anderson and van Wincoop, 2003), that is, general equilibrium effects that could otherwise bias the estimate of β. The coefficient on import restrictions remains virtually unchanged relative to specification 1. Therefore, when viewed across all products, importers that imposed border restrictions do not seem to have been much different from others.

Specification 3 adds time-varying country pair fixed effects (TVCP) to the TVP effects. TVCP effects are more general than TVIM and time-varying exporter effects (TVEX) taken together; they control completely for country-specific multilateral resistance. Therefore this specification addresses the country-specific components in $\Delta_{12}\delta_{ipt}$ and $\Delta_{12}\delta_{jpt}$, and controls completely for $\Delta_{12}\gamma_{ijt}$. In particular, TVCP effects control for any changes in *bilateral* trading costs that affect all products, such as changes in exchange rates, political relationships, or transport connections.[41] As Haddad, Harrison, and Hausman (2010) highlight, exchange rates fluctuated substantially for some nations during the crisis, so these additional controls may be quite important. However, we again obtain an unchanged import restriction coefficient, implying that exporters and country pairs affected by this type of protectionism were not particularly different from those not affected.

Specification 4 returns to just one type of fixed effect. The time-varying importer-product fixed effects (TVIMP) combine and generalize TVIM and TVP effects. They thereby allow product fixed effects to vary depending on the importer, capturing the notion that consumers in different countries may have reacted to the crisis differently by cutting expenditure on different items. This specification controls completely for $\Delta_{12}\delta_{ipt}$ and for the product-specific portion of $\Delta_{12}\delta_{jpt}$. These more detailed fixed effects are particularly useful in reducing the simultaneity bias that results if policymakers in importing countries impose protectionist measures in a way systematically related to changes in trade

[40]Note that the protectionist coefficients are semielasticities, because changes in log imports are on the left-hand side of the equation, while the protectionist variables are dummies and not expressed in logs.

[41]In specifications 5 and 6, below, the joint use of importer- and exporter-specific effects is sufficient to control for bilateral exchange rates, because they can be decomposed into exchange rates of importer and exporter relative to a reference currency.

developments.[42] TVIM effects can take account of some of this—the country-specific part—but if trade policies are motivated by developments in certain industries, TVIMP effects will be needed as controls.

The import restrictions estimate in specification 4 suggests a considerably larger protectionist impact (exceeding 7 percent) than those of regressions (1)–(3). This change in estimates can inform us about the direction of possible simultaneity bias in specifications 1–3, if we assume for the moment that specification 4 is the true model. Protected trade contracted by around 5 percent relative to global trade in the same product (specifications 1–3), but specification 4 shows that protected trade contracted by more when compared with other exporters' sales *in the same import market* (specification 4).[43] This also corroborates the graphical evidence in Figure 2, which suggested that targeted import markets may have increased in size relative to the global import market for the same product.[44] In other words, it seems that border restrictions were imposed in those markets where imports had risen disproportionately.[45] Therefore, our import restriction estimates in specifications 1–3 have to be interpreted with caution, but we take comfort that they are on the conservative side (that is, biased toward zero), if there is indeed such simultaneity bias.

Specification 5 adds time-varying exporter fixed effects (TVEX) to specification 4. This controls additionally for exporter-specific supply shocks, extending the coverage of $\Delta_{12}\delta_{jpt}$. Jointly with the importer effects, it also controls completely for exchange rate swings. The import restriction estimate increases slightly further to an 8 percent trade decline, indicating that protectionist actions affected more those exporters that performed comparatively well during the crisis period. Policymakers in importing countries may have an incentive to pick those well-performing exporters as targets, particularly if their global outperformance also implies that they are expanding sales in the domestic market in question. If that is indeed the case, then specification 5 (or even 6) would be the correct model and the import restriction coefficient in specification 4 could be underestimated on account of simultaneity bias.

---

[42]As pointed out above, large declines in imports of a product likely signal a large demand shock affecting this market and also impacting adversely domestic producers of the product.

[43]Effectively, specification 4 is identified only within importer-product-time combinations affected by protectionism, with other observations adding no further information. There are 9,021 of those combinations for import restrictions with 84,727 observations. For behind-the-border measures, there are 3,194 such combinations with 30,882 observations. In specifications 5 and 6, however, the other observations play a role as they help in tying down the coefficient values of the TVEX and TVEXP effects, respectively.

[44]This conclusion assumes that, relative to the world market in the given product, sales of nontargeted exporters in the protected market also increased in line with that of targeted exporters (or at least did not offset the increase in sales of targeted exporters). Also, in context of the great trade collapse, these markets probably *shrank* by less than the world market in the same product.

[45]In Grossman and Helpman's (1994) political economy model of protection, in contrast, equilibrium tariffs are *higher*, ceteris paribus, for politically organized industries with *low* import penetration, because industry shareholders gain from an increase in the domestic price, while consumers incur fewer inefficiency losses with lower imports. Our evidence suggests that instead higher import penetration would lead to higher protection, at least in the short term.

Specification 6, finally, is of particular interest due to its comprehensiveness, although estimates do not change further. It includes time-varying exporter-product fixed effects (TVEXP) on top of TVIMP effects. TVEXP effects allow product fixed effects to vary depending on the exporter, capturing the notion that crisis-induced supply shocks may have differed in each exporting sector in each country. This is the only specification controlling completely for multilateral resistance, if it varies across products. Both $\Delta_{12}\delta_{ipt}$ and $\Delta_{12}\delta_{jpt}$ are entirely controlled for in this specification. However, because of the high dimensionality of fixed effects, we can include only two types of fixed effects in the estimation—we cannot control for $\Delta_{12}\gamma_{ijt}$ at the same time. Therefore, if a nation experiences asymmetric trade cost changes across its trading partners, for instance by joining a preferential trade agreement, it may bias the protectionist coefficient. We judge any potential bias emanating from this to be small, in light of the limited trade coverage of protectionist actions and the short time span of our sample. Also, we are not aware of reasons why such asymmetric trade cost changes would have a high correlation with new protectionist measures.

Trade Impact of Behind-The-Border Measures

In contrast to our findings for import measures, for behind-the-border measures the largest coefficient estimate (about 15 percent) is found in specification 1. Estimates are reduced already by more than half in specification 3, which, however, still reports a statistically significant impact of 7 percent. Simultaneity may come into play here, but possible bias would now work in the opposite direction.

The reduction in estimates in response to adding importer fixed effects in specification 2 suggests that particularly policymakers in those countries tended to use behind-the-border measures where imports fell particularly strongly. In light of home bias in consumption (Trefler, 1995), particularly strong falls in imports are likely also a good proxy of a large domestic demand shock and a downtrodden state of the corresponding domestic industry. The most immediate way to assist these ailing domestic industries would be behind-the-border measures, such as bailouts. Thus, it is not too surprising that endogeneity bias likely inflates our behind-the-border estimate in the more basic specifications.

Perhaps more surprising is that, when introducing TVIMP effects (from specification 4 forward) the behind-the-border impact disappears completely; estimates lose both economic and statistical significance. TVIMP effects are more specific than the combination of TVP and TVIM effects of specification 2 and control for the possibility that policymakers may target specifically those sectors where imports declined particularly badly in their country. Such a behavior seems quite intuitive, so that simultaneity bias concerns are reduced when TVIMP effects are included.

As previously highlighted, this unfortunately presents a double bind for our fixed effects estimation strategy. If TVIMP effects are excluded (as in specifications 1–3), we cannot be sure whether estimates are purely driven by policymakers' nonrandom selection of which sectors to protect. If instead we do include TVIMP effects, we face the identification problem described earlier.

In specifications 4–6, identification relies on the variation between different exporters *j* only (within any importer-product-time category affected by protectionism). This is very problematic for bailout measures, which do discriminate between domestic and foreign firms but not among trading partners, so that variation in the data is low in this dimension.[23] The uniformly very small estimates indeed suggest that any impact of behind-the-border measures is absorbed completely into the TVIMP effects. Ultimately, the estimates in specifications 4–6 may then be underestimates of the true impact of behind-the-border measures. This is problematic also for our approximation of the aggregate impact of protectionism, because any behind-the-border measure, given its broad application across trade partners, affected considerable amounts of trade.

Finally, statistical measures of model comparison could help solve this conundrum, if they rejected model 4 soundly, thereby instilling confidence that endogenous selection at the importer-product level is not a strong possibility. However, the Hausman tests displayed in Table 2 do not offer strong conclusions.[46] Specifications 3 and 4 are nonnested and cannot be compared directly, but we can compare both to specification 2. Specification 3 is strongly preferred to specification 2. Meanwhile, specification 2 is rejected in favor of specification 4 only at the 12 percent level. This indicates that within products, importer-specific demand shocks were not much different than nationwide demand shocks and corroborates the evidence presented in Martins and Araujo (2009).

Nonetheless, specification 4 is not soundly rejected, so that we preliminarily conclude that there is weak evidence favoring specification 3 over specification 4. We explore this in the robustness section to shed more light on the impact of behind-the-border measures, for which the graphical evidence of Figure 3 suggested that TVIMP effects may be particularly relevant. There we switch to a different estimation strategy, which preserves more variation while still accounting for TVIMP determinants and its results indeed point in the direction of negative behind-the-border impacts.

Before discussing approximate aggregate impacts of protectionism, it is worthwhile noting that product-level estimates may also be subject to bias resulting from data reporting.[47] Many measures affect only a portion of a 4-digit

---

[46]Meanwhile, there seems to be strong evidence of specification 4 being preferred to 5 and 6, but we nonetheless include specification 6 in our robustness tests because of its comprehensiveness. Previous versions of this paper instead employed *F*-tests for model comparison, with very similar results. *F*-tests have some drawbacks in requiring normality of errors, but are asymptotically valid (Greene, 2003). Adjusted R^2 values of our models are low relative to gravity equations in levels. The dependent variable here—monthly product-level bilateral trade—is much more volatile. Not surprisingly then, even large sets of fixed effects do not have enormous explanatory power, as measured by adjusted R^2, because even within-group idiosyncratic fluctuations are high. Although the estimated coefficients on the protectionist dummies are highly statistically significant, these discrete variables take nonzero values for a small share of observations. They cannot be expected to boost the overall R^2 substantially, even though they do a good job in explaining the (few) observations affected by protectionism.

[47]In calculating aggregate trade impacts, any such bias would offset against trade flow values, resulting in unbiased aggregate impacts.

product category. This is particularly true for import restrictions imposed at the border, such as antidumping measures. To the extent that this is the case, our coefficient estimates likely underestimate product-level effects. Use of more detailed trade data (such as at the 6-digit level) would probably have led to more precise coefficient estimates, but was not possible given that GTA specifies affected products at the 4-digit level. If there is such bias, it would offset with trade flow amounts in calculating aggregate impacts. Another type of possible bias leading to underestimation of coefficients relates to intermediate goods. To the extent that intermediate goods were affected by protectionism, there may be a multiplier effect on trade flows, which cannot be quantified here, but would go beyond the estimated "static" effects.[48]

Finally, there may be selection bias resulting from incomplete data on some GTA measures, which dictated their exclusion from the analysis. This bias could lead to overestimated protectionist coefficients, if these missing measures were to affect the same country-pair-product combinations as measures included in the analysis. We judge this risk to be small because, in the considerable number of measures that we do include, hardly any affect the same observations.[49] However, incomplete data recording could have been a considerable issue if protectionism had been widespread.

Aggregate Results

With product-level coefficients of protectionist trade impacts in hand, we can now calculate a first approximation of the aggregate trade impact of new trade-related measures. First, for each type of measure, the estimated percentage reduction in product-level trade is multiplied by the value of trade affected. Then these measure-specific total impacts are summed to obtain one aggregate impact. In keeping this calculation straightforward, it is again helpful that the large majority of observations affected by protectionism are affected by one measure only. Any downward bias on protectionism's aggregate trade impact resulting from this simplification is thus minimal. The reported aggregate impacts are a first approximation on overall trade effects, given that the gravity coefficients constitute average treatment effects and neglect general equilibrium considerations.

In deriving approximate aggregate impacts, we rely mainly on specification 3 product-level estimates, in light of our results above and anticipating the conclusions of the robustness section. Our calculation combines these with trade values affected in the last quarter of 2009, because it is the last quarter with complete country coverage in our data set and at that time the largest number

[48]We thank an anonymous referee for pointing this out.

[49]Import restrictions, our main variable of interest, break down as follows. Observations affected by one import restriction numbered 56,050 (0.57 percent of the total sample). There were 4,780 observations, 218 observations, and 2 observations contemporaneously affected by two, three, and four import restrictions, respectively. The remainder of the sample (9,817,431 observations) was unaffected by import restrictions.

of protectionist restrictions was in place.[50] Based on that quarter, we calculate that new protectionist measures are estimated to have been reducing world trade by 0.21 percent.[51] In our sample covering 80 percent of world trade, based on 2009:Q4 quarterly trade flows, this translates into a quarterly trade loss of $4.6 billion.[52] Border and behind-the-border measures contributed about equally to the total impact, according to specification 3. However, there were only 40 behind-the-border import restrictions, compared with 239 border measures. Thus, based on these estimates, a typical behind-the-border measure distorted trade about seven times more than a typical border measure, because it affected more trading partners and products.

The results indicate that aggregate trade could increase by some $30–$35 billion a year if the crisis protectionist measures implemented up to early 2010 were removed.[53] Data limitations in the estimation imply that the actual benefits of removing crisis protectionist measures could be higher. The analysis here was handicapped by the exclusion (due to inadequate information) of 38 percent of the GTA "red" measures that were implemented during the study period. The extent to which this gap distorts the base estimate of 0.21 percent depends on how the excluded measures would affect product-level estimates. For example, if the restrictiveness of the excluded measures matched those in the sample, and again assuming specification 3 is the true model, the impact would rise to 0.34 percent.[54]

Robustness

Our robustness section consists of two parts. The first part centers around Table 3. It presents two sets of robustness tests, but adheres to our baseline fixed effects estimation strategy. Underlying data remain at monthly frequency. The second part centers around Table 4 and mainly aims to shed further light on behind-the-border impacts. Here we use a ratio-based estimation strategy on quarterly

[50]The data section elaborates further on the motivation for choosing the last quarter of 2009 as a basis for calculating aggregate trade impacts.

[51]If we instead used specification 4 estimates, the impact would be 0.14 percent of world trade, based solely on import restrictions, which are now attributed a larger impact. Specifications 5 and 6, which feature even more comprehensive controls, return slightly larger estimates, again based on import restrictions only.

[52]Table 2, lower panel, summarizes these impacts for all specifications in percent of world trade and in values. Impacts are calculated by multiplying the product-level coefficient estimates by the "affected quarterly trade" (the value of trade in country-pair-product combinations that were affected by new measures). Table 2 also provides the number of measures implemented and number of observations affected.

[53]The lower panel of Table 2 illustrates dollar values of the impact based on quarterly trade flows in 2009:Q4. Recognizing that trade in 2009 was depressed by the crisis, the $30–$35 billion impact stated here is calculated based on annual global trade in 2008 and 2010.

[54]Calculated as 0.21*(508/314), where 508 is the total number of measures in GTA and 314 is the number of measures that could be included in our estimation sample, including export measures (Table 1). However, excluded measures were tilted toward behind-the-border measures, whose effect is surrounded by somewhat more uncertainty. Therefore such extrapolation is difficult.

Table 3. Robustness of Product-Level Trade Impacts (Monthly Data)[1]

Time-Varying Fixed Effects	Includes Regressors for Export Measures			Interaction with 2007 Tariff Levels in Percent		
	Product & Country-Pair	Imp.-Prod.	Imp.-Prod.& Exp.-Prod.	Product & Country-Pair	Imp.-Prod.	Imp.-Prod.& Exp.-Prod.
Regression No.	R1	R2	R3	R4	R5	R6
Import restrictions	−0.051***	−0.076***	−0.083***	−0.023	−0.111**	−0.108**
	(−4.77)	−3.08	(−2.70)	(−1.27)	(−2.50)	(−2.31)
Behind-the-border measures	−0.074***	0.010	−0.008	−0.055***	0.067	0.034
(impact on imports)	(−4.56)	0.16	(−0.07)	(−2.68)	(0.89)	(0.41)
Export restrictions	0.017	0.025	−0.007			
	(0.46)	0.51	(−0.06)			
Export support	−0.016	−0.010	−0.032			
	(−1.30)	−0.82	(−0.87)			
Behind-the-border measures	−0.026	−0.005	0.066			
(impact on exports)	(−1.56)	−0.27	(1.43)			
Import restrictions				−0.003*	0.003	0.002
*Initial tariffs				(−1.76)	(0.89)	(0.54)
Behind-the-border measures				−0.004	−0.010	−0.007
*Initial tariffs[2]				(−1.27)	(−1.13)	(−0.75)
Number of time-varying fixed effects	128,833	2,574,786	3,819,552	116,158	2,150,646	3,351,199
Number of observations	9,878,481	9,878,481	9,878,481	9,072,142	9,072,142	9,072,142
Adj. R^2 (percent)	3.12	5.20	8.97	3.11	5.27	9.05

[1,2]Please see corresponding notes in Table 2.

data to circumvent the identification problems encountered in the fixed effects strategy. In both Tables 3 and 4, we present robustness checks for three different specifications. Given the uncertainty surrounding behind-the-border estimates and whether specification 3 or 4 may be the more appropriate model, we present all robustness checks for both. In addition, we report robustness results for analogs to specification 6, the model with the most comprehensive set of controls.

The first robustness test of Table 3 adds dummies for protectionist export measures, which had been excluded in the baseline regressions. Including export measures has little impact on the coefficients for import restrictions, and neither new export restrictions nor new export support measures are shown to significantly affect trade flows. Neither export support nor export restrictive measures were common in the sample period, with less than 20 registered by GTA. In light of the high volatility of product-level trade data, the low statistical significance of the export coefficients may be partly attributable to this.[55] Meanwhile, their insignificance is not primarily driven by lack of identifying variation. Although it is true that they varied little across importers, regression R2 relies on variation across exporters so that variation available for identification should be plentiful for export measures. Regression R1 does include some controls for exporter-specific determinants through the TVCP effects. Nonetheless, it would have identified any impacts, if they existed, based on exporter-specific variation within any product.

The second robustness test explores whether the impact of crisis protectionism varied depending on the amount of preexisting protection in place, with preexisting protection proxied by applied most-favored-nation tariffs in 2007.[56] We interact our two protectionist variables with these tariff levels. The results remain unchanged relative to the baseline in R5 and R6, which represent analogues to specifications 4 and 6, respectively. The only exception concerns R4, the analogue to specification 3, where there is some indication of nonlinearity in the impact of import restrictions, with crisis protection being more damaging in cases were protection was already high.[57]

Duads and Tetrads Estimation Strategy

Ratio approaches such as Duads and Tetrads estimation have the potential to address the identification issue surrounding the impact of behind-the-border

[55]We also tested the significance of green measures in initial stages of this research with similar results. See footnote 11 “Amber” measures are excluded given the uncertainty….

[56]These data were obtained from the UN TRAINS database with missing observations filled with data from the WTO’s Integrated Database (IDB).

[57]Henn and McDonald (2011) report two additional robustness tests. The first restricts the protectionist variable in Equation (1) to binary (0, 1) values, which leaves baseline results unchanged. The second uses changes in log trade volumes as the dependent variable and finds that results still broadly hold, although precision and magnitudes of estimates are reduced.

measures by preserving more variation in the estimation equation.[58] They carry an added advantage in addressing concerns about selection bias induced by the elimination of zero trade flows in our log gravity equation, on which we will comment after discussing their results.

We first derive the Duads Specification. It is the ratio analogue to specification 4 in the fixed effects baseline but accounts in a different way for time-varying importer-product-specific determinants (Head and Mayer, 2000; Martin, Mayer, and Thoenig, 2008). To derive the Duads specification, equation (2) is first divided by an analogue of itself, which instead of exporters *j* features a reference exporter *k*. Taking logs, eliminates any importer-product-time-specific determinants $\Delta_{12}\delta_{ipt}$, and yields the Duads estimation equation (4):

$$\begin{aligned}\Delta_{12}\ln\left(Imports_{ijpt}\right)-\Delta_{12}\ln\left(Imports_{ikpt}\right) &= (\Delta_{12}\delta_{jpt}-\Delta_{12}\delta_{kpt})+(\Delta_{12}\gamma_{ijt}-\Delta_{12}\gamma_{ikt})\\ &\quad+\beta(\Delta_{12}Prot_{ijpt}-\Delta_{12}Prot_{ikpt})\\ &\quad+(\Delta_{12}\varepsilon_{ijpt}-\Delta_{12}\varepsilon_{ikpt}). \qquad (4)\end{aligned}$$

All terms in the Duad estimation are relative to a reference exporter *k*. Therefore, in identifying any protectionist impacts, the estimation equation judges whether relative to the reference exporter (in any importer-product-time category), protected or unprotected exporters fared better. As in baseline specification 4, exporter-product-time-specific determinants ($\Delta_{12}\delta_{jpt}$–$\Delta_{12}\delta_{kpt}$) remain uncontrolled for in the Duads regression. To the extent that these determinants (i) vary between exporters *j* and the reference exporter *k*, and (ii) are correlated with protectionism, they may bias the protectionist coefficients β. Similarly, country-pair-time-specific influences, ($\Delta_{12}\gamma_{ijt}$–$\Delta_{12}\gamma_{ikt}$) are not addressed. Note that the error term is now composed of two components, of which $\Delta_{12}\varepsilon_{ikpt}$ appears repeatedly across observations within any *ipt* category. This correlated error structure implies that clustering is now more complex and nonnested. We address this by using the method of Cameron, Gelbach, and Miller (2011) to implement multiway clustering and add *ikp* clusters to the *ijp* clusters already used throughout previous specifications.

The range of values that the protectionist dummy ($\Delta_{12}Prot_{ijpt}$–$\Delta_{12}Prot_{ikpt}$) can take expands in the Duads regression. For the illustrative case of just one protectionist measure being imposed and never removed, it will take the value of 1 (−1) if the measure is imposed against exporter *j* (*k*), but not exporter *k* (*j*). It will take the value of 0 if the measure is imposed on both exporters *j* and *k*.

We also set out a Tetrads Specification, building on the work of Head, Mayer, and Ries (2010). It is a close analogue to the TVIMP-TVEXP fixed effects specification 6. The derivation of Tetrads builds on the multiplicative Duads equation (that is, the equation before we take logs to obtain equation (4)). This multiplicative Duads equation is divided by an analogue of itself where importers *i*

[58]We thank an anonymous referee for highlighting this. Variation would be preserved relative to fixed effects particularly in cases where the reference exporter is not affected by protectionism in a certain import market, while most other exporters are subject to such protectionism.

are replaced by a reference importer l throughout. Thus the tetrad comprises two importers, $\{il\}$, and two exporters, $\{jk\}$. Then we take logs. This eliminates any exporter-product-time-specific determinants in addition to the importer-product-time-specific determinants already eliminated in the Duad. Our Tetrads estimation equation (5) is:

$$\begin{aligned}&\Delta_{12}\ln\left(Imports_{ijpt}\right)-\Delta_{12}\ln\left(Imports_{ikpt}\right)-\Delta_{12}\ln\left(Imports_{ljpt}\right)\\&\quad+\Delta_{12}\ln\left(Imports_{lkpt}\right)=\left(\Delta_{12}\gamma_{ijt}-\Delta_{12}\gamma_{ikt}-\Delta_{12}\gamma_{ljt}+\Delta_{12}\gamma_{lkt}\right)\\&\quad+\beta\left(\Delta_{12}Prot_{ijpt}-\Delta_{12}Prot_{ikpt}-\Delta_{12}Prot_{ljpt}+\Delta_{12}Prot_{lkpt}\right)\\&\quad+\left(\Delta_{12}\varepsilon_{ijpt}-\Delta_{12}\varepsilon_{ikpt}-\Delta_{12}\varepsilon_{ljpt}+\Delta_{12}\varepsilon_{lkpt}\right)\end{aligned}\tag{5}$$

Here we cluster the errors along three dimensions—ijp, ipt, jpt—again using the Cameron, Gelbach, and Miller (2011) method. Meanwhile, $\Delta_{12}\varepsilon_{lkpt}$ takes one constant value for each product-time combination and is accounted for by de-meaning all "tetraded" variables across the product-time dimension before estimation.[59]

Despite their advantages in our application, the Duads and Tetrads estimations also have two downsides. First, the number of observations will be reduced if a given product is not traded with the reference importer or the reference exporter in the same time period.[60] We mitigate this problem by (i) choosing as reference importers and exporters the trade partners with the largest number of observations and (ii) aggregating our monthly data to quarterly figures. Quarterly aggregation preserves observations if reference trade flows are missing for certain months but there are nonzero flows with the reference importer and exporter in the same quarter.

The second drawback of Duads and Tetrads estimations is that results can vary depending on the reference importer and exporter chosen. We address this by calculating averages over estimates obtained based on different reference exporters in the Duads case, and different reference exporter-importer combinations in the Tetrads case. As reference exporters and importers, the trading nations with the largest amounts of observations are chosen, in order to avoid losing many observations and thereby running the risk of introducing selection bias. We require reference importers and exporters to have least as many observations as our largest trader on both the import and export sides, which is the EU in our data set. We end up with three reference exporters (China, the EU, and the United States) and five

[59]Head, Mayer, and Ries (2010) study on aggregate trade data proceeds in the analog way, by introducing time dummies. In light of the high dimensionality of the product-time dimension and our large data set, we cannot introduce a set of dummy variables into estimation without reaching computational constraints. Preestimation demeaning delivers the same results after standard errors are adjusted for degrees of freedom. A downside of having to demean for product-time is that we can only do such demeaning for one effect given that our panel is unbalanced. This prevents us from introducing time-varying country-pair fixed effects into the tetrads equation. Note that for tetraded variables, the protectionist dummy here may take integer values between 2 and −2 for the simplest case of only one protectionist measure affecting each observation and no measures being removed.

[60]Although this could introduce selection bias, if observations are lost in a fashion correlated with incidence of protectionist actions, we believe this possibility to be remote.

reference importers (Canada, China, the EU, Russia, and the United States). This results in a total of 12 reference importer-exporter pairs for the Tetrads specification.

Duads and Tetrads Results

Table 4 presents the results. It first includes quarterly analogues to our fixed effects baseline specifications 3, 4, and 6. These serve as a baseline for the quarterly Duads and Tetrads regressions, but also constitute an additional robustness check themselves to confer whether the volatility of monthly trade data may have driven previous results. This does not seem to be the case: the results of regressions R7-R9 are closely comparable with the monthly baseline regressions. Most notably, significance of behind-the-border measures disappears abruptly between specifications R7 and R8, parallel to our baseline results.

The three quarterly Duads regressions, broadly confirm our baseline results on import restrictions, although the number of usable observations drops by around 40 percent. Estimates are significant for the specifications in which China and the EU are used as the reference exporters. Both economic and statistical significance drop off when the United States is chosen as a reference exporter in the Duads regression with the lowest number of observations. In all, these results confirm that impacts of import restrictions do exist and their size may indeed be on the order of 5–8 percent, as indicated in the baseline specifications earlier.

The Duads regressions help to address the identification problem surrounding the impact of behind-the-border measures. Although estimates remain statistically insignificant throughout, their magnitude is now on the order of 10–13 percent trade declines. This suggests that the Duads setup does not suffer to the same extent from the shortfall of its fixed effects analogue, which absorbed all variation into the TVIMP controls and returned point estimates of almost exactly zero.

Specification R13 presents the Tetrads analogues to baseline specification 6, which featured our most comprehensive set of controls. Averaged results over our 12 Tetrads regressions are shown.[61] The requirement of contemporaneous flows to reference importer and exporter reduces the number of observations considerably further. Therefore estimates, and particularly their magnitudes, need to be interpreted with caution against the possibility of selection bias. The estimates are also very sensitive to the choice of reference importer and exporter. Nonetheless, the Tetrads results seem to broadly confirm our conclusions.

With regards to import restrictions, the Tetrads regressions apparently confirm a negative impact of new measures on trade, with the average Tetrad estimate suggesting a 13 percent trade decline. Moreover, most individual model estimates are negative and statistically significant (8 out of 12), and the two positive estimates are close to zero and statistically insignificant.

[61]Results are unchanged if, instead of simple averages, we use medians or weighted averages with the number of observations as weights.

Table 4. Robustness of Product-Level Trade Impacts (Quarterly Data)[1]

Type of Time-Varying Controls	Fixed Effects			Duads			Tetrads
Level of Controls	Product & Country-Pair	Importer-Product	Imp.-Prod. & Exp.-Prod.	Importer-Product	Importer-Product	Importer-Product	Imp.-Prod. & Exp.-Prod.
Regression No.	R7	R8	R9	R10	R11	R12	R13
Import restrictions	–0.040***	–0.055**	–0.081***	–0.104***	–0.070*	–0.047	–0.143**
	(–3.66)	(–2.39)	(–3.41)	(–2.72)	(–1.65)	(–0.95)	(–2.28)
Behind-the-border measures [2]	–0.063***	0.009	–0.033	–0.139	–0.107	–0.120	–0.276**
	(–3.52)	(0.17)	(–0.57)	(–1.03)	(–1.13)	(–0.95)	(–2.15)
Reference exporter			—	EU	China	U.S.	Average[4]
Number of time-varying fixed effects [3]	48,572	1,154,485	1,694,855	—	—	—	6,423.3
Number of observations	5,374,256	5,374,256	5,374,256	3,239,407	3,362,627	2,921,866	1,796,277.1
Adj. R^2 (percent)	3.19	4.40	8.50	NA	NA	NA	NA

[1] *, **, *** denote 10, 5, 1 percent significance levels. *T*-statistics in parentheses, based on robust standard errors clustered by country-pair-product combinations (for Regressions R7-R9). Multiway clustering applies for Regressions R10-R12 (country-pair-product and time-varying importer) and R13 (country-pair-product, time-varying importer and time-varying exporter). Regression coefficients express impacts in log units, which are very similar to percentage changes for values close to zero. The exact percentage change implied by any coefficient b can be calculated as exp(b)-1. The protectionist dummy in quarterly data takes the value of one, if it took the value of one in the last month of the quarter in the monthly data. When instead the decision is made based on the first month of the quarter, the results remain broadly unchanged.

[2] Refers to the impact of behind-the-border measures on imports.

[3] Time-varying importer-product are controlled for through "duading" in specifications R10-R12. In addition, time-varying exporter-product-specific determinants are controlled for through "tetrading" in specification R13. In addition, following Head, Mayer, and Ries (2010), product-time fixed effects are introduced in R13 to account for *lkpt* specific component of the error term, which is constant for each product at each point in time.

[4] Simple average over 12 pairs of reference exporters and importers, given the sensitivity of estimates to this choice. Selected as reference countries are those with at least half as many observations in our sample as the largest trader (EU). This gives us three reference exporters (China, EU, United States) and five reference importers (Canada, China, EU, Russia, United States), resulting in 12 pair combinations. The results remain unchanged when instead of the simple average either the median or a weighted average, with number of observations as weight, are used. Notably, all point estimates for the behind-the-border coefficient are negative and 6 of 12 are moreover statistically significant at the 5 percent level. Of the import restriction coefficients, 8 of 12 are negative and statistically significant at the 5 percent level; only 2 of 12 are positive (and statistically insignificant).

With regards to the behind-the-border measures, Tetrads regressions return negative point estimates in all 12 models, but only six of these are also statistically significant. Moreover, estimate magnitudes fluctuate widely and include some very large coefficient estimates. The smallest Tetrad point estimates (in absolute value) suggest an 8 percent trade diversion. Although this cannot conclusively confirm the existence of behind-the-border impacts, especially in light of the insignificance of Duads estimates, we take these results as additional evidence that such impacts are likely negative. Uncertainty also remains with regards to the magnitude of behind-the-border impacts, though, when considering all the evidence and conservatively speaking, it may be on the same magnitude as that of border restrictions (5–8 percent). The duads and tetrads results also provide us sufficient comfort to use our baseline specification 3 (which returned a 7 per cent behind-the-border impact) in illustrating detailed impacts of specific types of measures in the following subsection.

First, however, we conclude this robustness section by discussing why, in light of the Duads and Tetrads estimates, selection bias resulting from elimination of zero trade flows does not seem to be a major issue in our application.

Zero Trade Flows

The existence of zero trade flows is a pervasive problem in log gravity equations.[62] Eliminating the zero flows by taking logs can induce selection bias (Helpman, Melitz, and Rubinstein, 2008). One common way to handle this is to avoid taking logs altogether, thereby preserving the zero trade flows, and estimate the gravity equation in multiplicative form by Pseudo Poisson Maximum Likelihood (PPML) (Santos Silva and Tenreyro, 2006). This method has mainly been used in applications with aggregate trade flows. PPML is impracticable in our product-level application because: (i) convergence of PPML is usually not achieved with fixed effects of a dimensionality as high as ours, (ii) adding all the zeros to a data set with already close to 10 million observations would severely aggravate computational constraints, and (iii) our estimation strategy relies on a *differenced* log gravity equation to eliminate country-pair-product-specific effects and seasonality.[63] Employing PPML would require estimating the gravity equation in levels,

[62] It is not straightforward to devise metrics to evaluate the importance in our data set of trade flows falling to zero in response to new protectionist measures. This is because high frequency product-level trade data often show zeros for certain months or quarters given lumpiness of shipments. Therefore, more intricate decision rules, such as in Asmundsen (2012), would have to be devised to define how many months of zero values are necessary to consider that trade flows in a country-pair-product category have ceased. This would be further complicated by the differencing in our estimation equation. Given that Duads and Tetrads results are broadly in line with our baseline findings, we do not explore this issue further.

[63] In addition to these limitations, Martin and Pham (2009) have shown PPML to yield "severely biased estimates" when large values of zeros have been generated by a limited dependent variable process. For aggregate trade data, Head, Mayer, and Ries (2010, p.4) document a number of further important shortfalls of PPML.

reintroducing country-pair-product-specific fixed effects and making the problem computationally intractable.[64]

Head and Mayer (2014) find that Tetrad regressions constitute an alternative solution. They run Monte Carlo simulations on gravity equations, comparing how a variety of estimation approaches perform in estimating distance and trade policy effects in the presence of missing observations.[65] In their simulations the Tetrads approach performs best if zero and small trade flows are deleted. Meanwhile, the fixed effects approach of our baseline performs best if trade flows are deleted randomly. With Tetrads results generally corroborating our baseline results, this provides some evidence that selection bias resulting from zeros is limited in the baseline specification.

Effects by Measure Types

Table 5 presents results in which the protectionist dummies are disaggregated by type of measure. The table and the discussion are centered on our preferred TVP-TVCP specification 3, but we highlight the limited areas in which results based on TVIMP specification 4 deviate.[66]

Regression 7 breaks down import border measures into (i) tariffs and import bans; (ii) trade defense measures; (iii) NTBs, mainly made up of licensing requirements and sanitary and phytosanitary restrictions; and (iv) discriminatory purchasing measures, including local content provisions, public procurement, and consumer subsidies. Behind-the-border measures are broken down into (v) bailouts, (vi) domestic subsidies, and (vii) investment subsidies. Bailouts are distinguished from domestic subsidies by being directed to specific firm(s). Investment subsidies provide investment incentives to domestic firms in a discriminatory fashion. Regression 8 further categorizes (i), (iii), and (iv).

Tariffs and other traditional trade barriers do not seem to have been particularly harmful; this result underpins a growing interest in nontraditional barriers as main retardants of trade (Minor and Tsigas, 2008). The impact of tariff measures is not statistically significant, perhaps because tariff increases on average were not large enough for our dummy variable approach to reveal an impact. Correspondingly, they contribute little to the overall impact, although the number of implemented tariff measures (67) is high relative to total measures taken. Relatedly, Kee, Neagu, and Nicita (2010) find that changes in countries' tariff schedules and their use of antidumping (AD) measures only modestly increased overall protection levels during the global crisis. In contrast, those traditional trade measures that by design should be more restrictive, such as quotas and import bans, showed this also in the

[64]Otherwise the only way to eliminate the country-pair-specific effects in a PPML setup would be to divide the contemporaneous multiplicative gravity equation by its 12-month lagged equivalent. This would result again in missing values for imports where the 12-month lagged value is zero.

[65]For the latter, they use a regional agreement dummy in their study.

[66]This subsection explores the effects by different types of measure and summarizes results by country groupings. Henn and McDonald (2011) provide more detail on the latter and also include additional results by sectors and time periods.

Table 5. Detailed Results, by Type of Measure

	Estimation of Product-Level Trade Impact[1]			Calculation of Aggregate Trade Impact[3,6]					
Time-Varying Fixed Effects	Product & Country-Pair	Product & Country-Pair	Product & Country-Pair	Agg. Quarterly Trade Impact Implied by Regression No.:			No. of Meas.[4]	Affected Obs.[5]	Affected Quarterly Trade[6]
Regression No.	3	7	8	3	7	8			
Total				–$4,568	–$4,352	–$4,134	279		$77,668
				–0.21%	–0.20%	–0.19%		1.65%	3.58%
Import restrictions	–0.051***			–$2,105	–$1,908	–$1,672	239		$42,722
	(–4.77)			–0.10%	–0.09%	–0.08%		1.11%	1.97%
Tariff and import bans		–0.030*			–$788	–$408	99		$26,859
		(–1.75)			–0.04%	–0.02%		0.48%	1.24%
Tariff			–0.012			–$322	67		$26,204
			(–0.64)			–0.01%		0.44%	1.21%
Quota			–0.270*			–$18	5		$75
			(–1.84)			0.00%		0.01%	0.00%
Import ban			–0.145*			–$14	23		$100
			(–1.67)			0.00%		0.01%	0.00%
Competitive devaluation			–0.120			–$54	4		$480
			(–1.48)			0.00%		0.02%	0.02%
Trade defense measures		–0.170***	–0.169***		–$291	–$290	102		$1,861
		(–2.73)	(–2.82)		–0.01%	–0.01%		0.02%	0.09%
Nontariff barriers		–0.098***			–$77	–$71	16		$828
		(–3.76)			0.00%	0.00%		0.18%	0.04%
Licensing requirements			–0.092***			–$67	11		$764
			(–3.44)			0.00%		0.17%	0.04%
Sanitary and phytosanitary			–0.605			$0	1		$0
			(–1.14)			0.00%		0.00%	0.00%
Other NTBs			–0.044			–$4	4		$88
			(–0.61)			0.00%		0.02%	0.00%

Table 5: (Continued)

	Estimation of Product-Level Trade Impact[1]			Calculation of Aggregate Trade Impact[3,6]					
Time-Varying Fixed Effects	Product & Country-Pair	Product & Country-Pair	Product & Country-Pair	Agg. Quarterly Trade Impact Implied by Regression No.:			No. of Meas.[4]	Affected Obs.[5]	Affected Quarterly Trade[6]
Regression No.	3	7	8	3	7	8			
Discriminatory purchasing		−0.046***			−$751	−$903	22		$16,661
		(−3.30)			−0.03%	−0.04%		0.49%	0.77%
Local content			−0.068**			−$360	5		$5,506
			(−2.01)			−0.02%		0.12%	0.25%
Public procurement			0.027			$110	9		$4,070
			(0.88)			0.01%		0.12%	0.19%
Consumption subsidies			−0.092***			−$653	8		$7,428
			(−4.46)			−0.03%		0.26%	0.34%
Behind-the-border measures[2]	−0.073***			−$2,462	−$2,444	−$2,463	40		$34,946
	(−4.53)			−0.11%	−0.11%	−0.11%		0.54%	1.61%
Bailouts		−0.072***	−0.072***		−$1,885	−$1,893	27		$27,204
		(−2.63)	(−2.52)		−0.09%	−0.09%		0.19%	1.25%
Domestic subsidies		−0.076***	−0.078		−$559	−$569	7		$7,633
		(−3.91)	(−4.13)***		−0.03%	−0.03%		0.34%	0.35%
Investment subsidies		−0.030	−0.031		$0	$0	6		$1
		(−0.42)	(−0.45)		0.00%	0.00%		0.00%	0.00%
No. of time-varying fixed effects	128,833	128,833	128,833						
No. of observations (thousands)	9,878	9,878	9,878						
Adj. R^2 (percent)	3.12	3.12	3.12						

[1,2,3,4,5,6]Please see notes in Table 2.

data, decreasing trade in affected products by 24 and 13 percent, respectively. Their coefficients are borderline statistically significant, but their contribution to the overall protectionist impact is low given the narrow targeting of these measures.

Trade defense measures, by their nature, were very narrowly focused on specific trade partners in specific products and therefore could not have a large aggregate impact, despite the high number of different duties imposed (102).[67] However, product-level impacts were large indeed, with trade decreasing on average by 16 percent at the 4-digit level and suggesting more dramatic declines in the sub-4-digit level trade flows actually targeted by these measures. Within NTBs, licensing requirements drove the impact, with a 9 percent trade decrease at the product-level, but again narrow application avoided a large aggregate impact. Of all border measures, purchasing provisions generally reduced aggregate imports the most.[68] Among purchasing provisions, consumption subsidies caused the largest decreases both at the product level (–9 percent) and in aggregate (–0.03 percent). Local content requirements covering the entire domestic market were similarly harmful. In contrast, and despite the large attention received by public procurement measures during the crisis, our analysis does not point to a trade impact. When these impacts are evaluated based on TVIMP specification 4 instead, discriminatory purchasing provisions become insignificant, while tariff measures become borderline significant and their product-specific trade impact increases to about 7 percent.[69]

We now move on to behind-the-border measures, keeping in mind from the previous discussion that their estimates are surrounded by higher uncertainty and need to be interpreted with caution. Bailouts and domestic subsidies are each found to have decreased trade in affected products by around 7 percent. With bailouts affecting more trade in the sample, their aggregate impact (–0.09 percent) exceeds that of subsidies (–0.03 percent). Discriminatory subsidies encouraging investment by domestic firms only are not found to have caused a contemporaneous statistically significant trade reduction. This, however, does not preclude that they may reduce imports after investment projects are finalized and new domestic production capacity comes on stream.

Calculations of the aggregate protectionist impact are insensitive to whether they are calculated based on this disaggregation or more aggregate coefficients, as previously. Regressions 7 and 8 yield alternative estimates of reductions in aggregate world trade of 0.20 and 0.19 percent, respectively—very similar to the baseline estimate of 0.21 percent. Calculations based on additional disaggregations also fall around the 0.21 percent baseline estimate. These additional disaggregations

---

[67]Bown (2010), who reviews developments in the use of antidumping, safeguards, and countervailing duties, notes that their use rose by ¼ in 2008–09 as measured by the coverage of product lines by major users.

[68]In regression 7, the aggregate impact of tariffs and import bans is slightly higher, but it results from a statistically insignificant product-level coefficient.

[69]Behind-the-border measures remain insignificant throughout in the TVIMP fixed effects specifications.

in particular investigate which country groups were most affected by protectionism and whose measures were the most harmful.

These results are reported in Henn and McDonald (2011) and we limit ourselves to a brief summary of results here, again based on the TVIM-TVCP specification 3. For border measures, those of advanced economies decreased imports by 8 percent on average, double the amount of developing economy measures. Not only were advanced economies' measures more trade restrictive, but they also affected larger trade flows. Consequently, they accounted for about ¾ of the aggregate trade distortion implied by border measures. Developing economies, and particularly the poorer ones among them, were affected by the most trade-deterring border measures.

For behind-the-border measures, in contrast, those taken by developing economies are found to be more trade restrictive at the product level, although those of the advanced economies were more widespread and thus more harmful in aggregate. Advanced economies were the most harmed by behind-the-border measures, with much of the impact falling on advanced as well as emerging Europe. This mainly reflects highly trade-restrictive behind-the-border measures implemented in the Central Asia region (which includes Russia), a result also found by Cernat and Souza (2010).

V. Conclusion

This paper fills a gap in the literature by providing an ex-post analysis of the trade impact of a wide array of trade policy measures. It exploits relatively new data on protectionist measures collected through monitoring exercises that started with the global crisis 2008–09. These data constitute an important addition to otherwise scarce data on trade policy measures in many less transparent areas. They also open the door to estimating ex-post trade impacts for many measures in a differenced gravity equation framework for which indirect, simulation-based approaches had been the only option until now.

Another important feature of these data is that they allow for an analysis of many types of measures simultaneously. In evaluating crisis protectionism, such a comprehensive approach is needed to fully appreciate the aggregate trade impact, given the large variety of discriminatory measures implemented during this time. Indeed, our results indicate that studies that focus exclusively on conventional border measures likely neglect a key aspect of crisis protectionism.

Given that the enhanced data collection efforts on protectionism only started recently, our protectionist impact estimates are derived based on high-frequency product-level trade flows covering a very turbulent period in the world economy. Our estimation strategy attempts to ensure that protectionist impacts are as much as possible isolated from those of other factors and minimizes potential for both omitted variable and simultaneity bias. It does so through extensive time-varying fixed effect controls and duad and tetrad ratio estimations of a differenced gravity equation. Considering in addition the robustness of our results, particularly those on border import restrictions, we are therefore confident that our estimates contain substantial information on measures' average impacts that are also relevant outside of crisis periods. This is important as more years of data collection lie ahead until analysis can be repeated on data covering truly "normal" times in the world economy. In the meantime, we hope that monitoring activities are sustained.

Our results provide strong evidence that new trade restrictions are significantly decreasing trade in affected products. Estimates of impacts on affected trade flows associated with border measures are about 5–8 percent. There is also considerable evidence that behind-the-border measures impact trade negatively, and impacts may be of roughly similar magnitude. However, issues related to identification keep us from drawing strong conclusions on behind-the-border measures. Tariffs and other traditional trade measures had modest impacts, while antidumping duties and other unconventional types of protectionism such as NTBs, discriminatory purchasing policies, bailouts, and domestic subsidies seem to be having more potent effects on trade flows. Others have documented that unconventional measures are a key feature of the trade policy response to the crisis, but the data and method here have allowed the first comprehensive assessment of the trade impact of crisis protectionism. In aggregate, the crisis protectionism measures implemented during our study period of July 2008–April 2010 are estimated to be reducing global trade by around 0.2 percent.

REFERENCES

Abowd, John M., Francis Kramarz, and David N. Margolis, 1999, "High Wage Workers and High Wage Firms," *Econometrica*, Vol. 67, No. 2, pp. 251–333.

Ahn, JaeBin, Mary Amiti, and David E. Weinstein, 2011, "Trade Finance and the Great Trade Collapse," *American Economic Review*, Vol. 101, No. 3, pp. 298–302.

Alessandria, George, Joe Kaboski, and Virgiliu Midrigan, 2010, "The Great Trade Collapse of 2008–09: An Inventory Adjustment?," *IMF Economic Review*, Vol. 58, No. 2, pp. 254–94.

Amiti, Mary and David Weinstein, 2009, "Exports and Financial Shocks," Working Paper 15556 (National Bureau of Economic Research).

Anderson, James E. and Eric van Wincoop, 2003, "Gravity with Gravitas: A Solution to the Border Puzzle," *American Economic Review*, Vol. 93, No. 1, pp. 170–92.

———, 2004, "Trade Costs," *Journal of Economic Literature*, Vol. 42, No. 3, pp. 691–751.

Anderton, Robert and Tadios Tewolde, 2011, "The Global Financial Crisis: Understanding the Global Trade Downturn and Recovery," *The World Economy*, Vol. 34, No. 5, pp. 741–63.

Asmundson, Irena, 2012, More World Trade Flows: An Updated Dataset, Mimeo (Washington, DC: International Monetary Fund).

Baier, Scott L. and Jeffrey H. Bergstrand, 2007, "Do Free Trade Agreements Actually Increase Members' International Trade?," *Journal of International Economics*, Vol. 71, No. 1, pp. 72–95.

Baldwin, Richard E. ed., 2009, "The Great Trade Collapse: Causes, Consequences, and Prospects," Available via the Internet: www.voxeu.org.

Baldwin, Richard E. and Simon Evenett eds., 2009, "The Collapse of Global Trade, Murky Protectionism, and the Crisis: Recommendations for the G-20," Available via the Internet: www.voxeu.org.

Baldwin, Richard E. and Daria Taglioni, 2006, "Gravity for Dummies and Dummies for Gravity Equations," Working Paper 12516 (National Bureau of Economic Research).

——— , 2009, "The Great Trade Collapse and Trade Imbalances," Available via the Internet: www.voxeu.org.

Bems, Rudolfs, Robert C. Johnson, and Kei-Mu Yi, 2010, "Demand Spillovers and the Collapse of Trade in the Global Recession," *IMF Economic Review*, Vol. 58, No. 2, pp. 295–326.

Bown, Chad P., 2010, "Taking Stock of Antidumping, Safeguards, and Countervailing Duties, 1990–2009," World Bank Policy Research Working Paper 5436.

Bussiere, Matthieu, Emilia Perez-Barreiro, Roland Straub, and Daria Taglioni, 2011, "Protectionist Responses to the Crisis: Global Trends and Implications," *The World Economy*, Vol. 34, No. 5, pp. 826–52.

Cameron, A., Colin Jonah B. Gelbach, and Douglas L. Miller, 2011, "Robust Inference with Multiway Clustering," *Journal of Business and Economic Statistics*, Vol. 29, No. 2, pp. 238–49.

Cernat, Lucian and Nuno Souza, 2010, "The Impact of Crisis-driven Protectionism on EU Exports: The 'Russian Doll' Effect," Available via the Internet: www.voxeu.org.

Chen, Natalie and Dennis Novy, 2011, "Gravity, Trade Integration, and Heterogeneity across Industries," *Journal of International Economics*, Vol. 85, No. 2, pp. 206–21.

Cipollina, Maria and Luca Salvatici, 2010, "Reciprocal Trade Agreements in Gravity Models: A Meta-Analysis," *Review of International Economics*, Vol. 18, No. 1, pp. 63–80.

Decreux, Yvan and Lionel Fontagne, 2011, "Economic Impact of Potential Outcome of DDA," CEPII Working Paper No. 2011-23.

Driscoll, John C. and Aart C. Kraay, 1998, "Consistent Covariance Estimation with Spatially Dependent Panel Data," *Review of Economics and Statistics*, Vol. 80, No. 4, pp. 549–60.

Egger, Peter, Mario Larch, Kevin E. Staub, and Rainer Winkelmann, 2011, "The Trade Effects of Endogenous Preferential Trade Agreements," *American Economic Journal: Economic Policy*, Vol. 3, No. 3, pp. 113–43.

Eicher, Theo S., Christian Henn, and Chris Papageorgiou, 2012, "Trade Creation and Diversion Revisited: Accounting for Model Uncertainty and Natural Trading Partner Effects," *Journal of Applied Econometrics*, Vol. 27, No. 2, pp. 296–321.

Eicher, Theo S. and Christian Henn, 2011a, "In Search of WTO Trade Effects: Preferential Trade Agreements Promote Trade Strongly, but Unevenly," *Journal of International Economics*, Vol. 83, No. 2, pp. 137–53.

——— , 2011b, "One Money One Market: A Revised Benchmark," *Review of International Economics*, Vol. 19, No. 3, pp. 419–35.

Essaji, Azim, 2008, "Technical Regulations and Specialization in International Trade," *Journal of International Economics*, Vol. 76, No. 2, pp. 166–176.

Evenett, Simon ed., 2010, "Managed Exports and the Recovery of World Trade: The 7th GTA Report," Available via the Internet: www.globaltradealert.org.

Francois, Joe, Brad McDonald, and Hakan Nordstrom, 1996, "A User's Guide to Uruguay Round Assessments," CEPR Discussion Paper No. 1410.

Francois, Joe and Ken Reinert eds., 1997, *Applied Methods for Trade Policy Analysis: A Handbook* (Cambridge: Cambridge University Press).

Greene, William H., 2003, *Econometric Analysis* (Princeton, NJ: Prentice-Hall, 5th ed).

Grossman, Gene M. and Elhanan Helpman, 1994, "Protection for Sale," *American Economic Review*, Vol. 84, No. 4, pp. 833–850.

Guimaraes, Paulo and Pedro Portugal, 2010, "A Simple Feasible Procedure to Fit Models with High-Dimensional Fixed Effects," *The Stata Journal*, Vol. 10, No. 4, pp. 628–49.

Haddad, Mona, Ann Harrison, and Catherine Hausman, 2010, "Decomposing the Great Trade Collapse: Products, Prices, and Quantities in the 2008–2009 Crisis," National Bureau of Economic Research Working Paper 16253.

Hansen, Lars Peter and Robert J. Hodrick, 1980, "Forward Exchange Rates as Optimal Predictors of Future Spot Rates: An Econometric Analysis," *Journal of Political Economy*, Vol. 88, No. 5, pp. 829–53.

Head, Keith and Thierry Mayer, 2000, "Non-Europe: the Magnitude and Causes of Market Fragmentation in the EU," *Weltwirtschaftliches Archiv*, Vol. 136, No. 2, pp. 285–314.

———, 2014, "Gravity Equations: Workhorse, Toolkit, Cookbook," in *Handbook of International Economics*, Vol. 4, ed. by Gita Gopinath, Elhanan Helpman, and Kenneth Rogoff (Amsterdam: Elsevier Science Publishers B.V.).

Head, Keith, Thierry Mayer, and John Ries, 2010, "The Erosion of Colonial Trade Linkages after Independence," *Journal of International Economics*, Vol. 81, No. 1, pp. 1–14.

Head, Keith and John Ries, 2001, "Increasing Returns Versus National Product Differentiation as an Explanation for the Pattern of U.S.-Canada Trade," *American Economic Review*, Vol. 91, No. 4, pp. 858–76.

Helpman, Elhanan, Marc Melitz, and Yona Rubinstein, 2008, "Estimating Trade Flows: Trading Partners and Trading Volumes," *The Quarterly Journal of Economics*, Vol. 123, No. 2, pp. 441–87.

Henn, Christian and Brad McDonald, 2010, "Avoiding Protectionism," *Finance and Development*, March, pp. 20–23.

———, 2011, "Protectionist Responses to the Crisis: Damage Observed in Product-Level Trade," IMF Working Paper 11/139 (Washington, DC: International Monetary Fund).

Hoekman, Bernard and Alessandro Nicita, 2011, "Trade Policy, Trade Costs, and Developing Country Trade," *World Development*, Vol. 39, No. 12, pp. 2069–79.

Hummels, David and James Levinsohn, 1995, "Monopolistic Competition and International Trade: Reconsidering the Evidence," *The Quarterly Journal of Economics*, Vol. 110, No. 3, pp. 799–836.

Kee, Hiau Looi, Cristina Neagu, and Alessandro Nicita, 2010, "Is Protectionism on the Rise? Assessing National Trade Policies during the Crisis of 2008," World Bank Policy Research Working Paper 5274.

Levchenko, Andrei A., Logan Lewis, and Linda L. Tesar, 2009, "The Collapse of International Trade during the 2008–9 Crisis: In Search of the Smoking Gun," *IMF Economic Review*, Vol. 58, No. 2, pp. 214–253.

Martin, Philippe, Thierry Mayer, and Mathias Thoenig, 2008, "Make Trade Not War?," *Review of Economic Studies*, Vol. 75, No. 3, pp. 865–900.

Martin, Will and Cong S. Pham, 2009, *Estimating the Gravity Model when Zero Trade Flows are Frequent, mimeo* (Washington, DC: World Bank).

Martins, Joaquim Oliveira and Sonia Araujo, 2009, "The Great Synchronization: Tracking the Trade Collapse with High-frequency Data," in *The Collapse of Global Trade, Murky Protectionism, and the Crisis: Recommendations for the G-20*, ed. by Baldwin, Richard E. and Simon Evenett. Available via the Internet: www.voxeu.org.

Minor, Peter and Marinos Tsigas, 2008, "Impacts of Better Trade Facilitation in Developing Countries: Analysis with a New GTAP Database for the Value of Time in Trade," 11th Annual Conference on Global Economic Analysis, Helsinki, Finland.

Newey, Whitney K. and Kenneth D. West, 1987, "A Simple, Positive Semi-Definite, Heteroskedasticity and Autocorrelation Consistent Covariance Matrix," *Econometrica*, Vol. 55, No. 3, pp. 703–8.

Organization for Economic Cooperation and Development (OECD). 2010, "Trade and Economic Effects of Responses to the Economic Crisis," OECD Trade Policy Studies.

Rose, Andrew K., 2000, "One Money, One Market: The Effect of Common Currencies on Trade," *Economic Policy*, Vol. 15, No. 30, pp. 7–46.

——— , 2004, "Do We Really Know that the WTO Increases Trade?," *American Economic Review*, Vol. 94, No. 1, pp. 98–114.

Rogers, William H., 1993, "sg 17: Regression Standard Errors in Clustered Samples," *Stata Technical Bulletin*, Vol. 3, No. 13, pp. 19–23, In Stata Technical Bulletin Reprints, Vol. 3, pp. 88–94 (College Station, TX: Stata Press).

Shingal, Anirudh, 2009, "The Impact of Cross-Border Discrimination on Japanese Exports: A Sectoral Analysis," in *The Unrelenting Pressure of Protectionism: The 3rd GTA Report*, ed. by Simon Evenett (London: Centre for Economic Policy Research).

Santos Silva, J. and S. Tenreyro, 2006, "The Log of Gravity," *Review of Economics and Statistics*, Vol. 88, No. 4, pp. 641–58.

Stock, James H. and Mark W. Watson, 2008, "Heteroskedasticity-Robust Standard Errors for Fixed Effects Panel Data Regression," *Econometrica*, Vol. 76, No. 1, pp. 155–74.

Trefler, Daniel, 1995, "The Case of Missing Trade and Other Mysteries," *American Economic Review*, Vol. 85, No. 5, pp. 1029–46.

UNCTAD/WTO. 2012, *A Practical Guide to Trade Policy Analysis* (Geneva, Switzerland: UNCTAD/WTO).

Wooldridge, Jeffrey M., 2002, *Econometric Analysis* (Cambridge, MA: MIT Press).

World Trade Organization (WTO). 2013, *Report on G-20 Trade Measures (Mid-October 2012 to Mid-May 2013).* 17 June 2013, Geneva, Switzerland.