EUROPEAN CENTRAL BANK

EUROSYSTEM

WORKING PAPER SERIES

NO 1618 / NOVEMBER 2013

GROWTH, REAL EXCHANGE RATES AND TRADE PROTECTIONISM SINCE THE FINANCIAL CRISIS

Georgios Georgiadis and Johannes Gräb

In 2013 all ECB publications feature a motif taken from the €5 banknote.

Acknowledgements
This paper should not be reported as representing the views of the European Central Bank (ECB). The views expressed are those of the authors and do not necessarily reflect those of the ECB. We would like to thank Johannes Fritz and Sebastian Kripfganz, seminar and conference participants at the ECB, the 14th Conference on International Economics in Palma de Mallorca and the Mainz Workshop in Trade and Macroeconomics as well as an anonymous referee for helpful comments and suggestions.

Georgios Georgiadis
European Central Bank; e-mail: georgios.georgiadis@ecb.europa.eu

Johannes Gräb (corresponding author)
European Central Bank; e-mail: johannes.graeb@ecb.europa.eu

Address	Kaiserstrasse 29, 60311 Frankfurt am Main, Germany
Postal address	Postfach 16 03 19, 60066 Frankfurt am Main, Germany
Telephone	+49 69 1344 0
Internet	http://www.ecb.europa.eu
Fax	+49 69 1344 6000

ISSN	1725-2806 (online)
EU Catalogue No	QB-AR-13-115-EN-N (online)

Abstract

Existing evidence suggests that protectionist activity since the financial crisis has been muted, raising the question whether the historically well-documented relationship between growth, real exchange rates and trade protectionism has broken down. This paper re-visits this relationship for the time period since 2009. To this end, we use a novel and comprehensive dataset which considers a wide range of trade policies stretching beyond the traditionally considered tariff and trade defence measures. We find that the specter of protectionism has not been banished: Countries continue to pursue more trade-restrictive policies when they experience recessions and/or when their competitiveness deteriorates through an appreciation of the real exchange rate; and this finding holds for a wide array of contemporary trade policies, including "murky" measures. We also find differences in the recourse to trade protectionism across countries: trade policies of G20 advanced economies respond more strongly to changes in domestic growth and real exchange rates than those of G20 emerging market economies. Moreover, G20 economies' trade policies vis-à-vis other G20 economies are less responsive to changes in real exchange rates than those pursued vis-à-vis non-G20 economies. Our results suggest that—especially in light of the sluggish recovery—the global economy continues to be exposed to the risk of a creeping return of trade protectionism.

Keywords: Trade protectionism, growth, exchanges rates.
JEL-Classification: F13, F14.

Non-technical summary

The sharp global economic downturn in 2008/09 has nurtured fears that governments might resort to trade protectionism in order to support their economies by sheltering them from foreign competition. As the experience of the Great Depression in the 1930s has vividly illustrated, such a policy response is likely to trigger retaliatory action and thereby to deepen the recession significantly. The fear of a protectionist response to the financial crisis is well-founded, in particular given the robust empirical evidence for the time period prior to the financial crisis showing that governments were more likely to erect trade barriers whenever their economies experienced recessions and/or losses in competitiveness through an appreciation of the real exchange rate. Interestingly, despite this historically well-documented relationship between growth, competitiveness and trade protectionism on the one hand and the deep recession of the global economy on the other hand, existing data suggests that recent trade policies of G20 economies have not been particularly protectionist. This phenomenon raises the question whether the relationship between protectionism growth and real exchange rates documented for the time period prior to the financial crisis continues to hold. On the one hand, it could be that this relationship has broken down, for example on the back of the vertical fragmentation of supply chains across countries that annihilate the incentives to shut domestic markets to foreign producers. If so, fears of a creeping rise in protectionism would be unfounded and efforts to contain protectionism should be redirected to other policy issues. On the other hand, if the relationship continues to hold and the limited protectionist response is due to one-off factors for example the existence of social safety nets and concerted stimulus packages protectionism may indeed resurface. This is all the more relevant as fiscal positions in many countries around the globe have been stretched to the extent that many governments have endorsed tight austerity measures. Against this background, in this paper we analyse the relationship between growth, competitiveness and trade protectionism for G20 economies for the time period since the financial crisis. In particular, we estimate bilateral regressions of the number of trade-restrictive measures implemented by each G20 economy against its trading partners on domestic and trading partner's GDP growth as well as the real bilateral exchange rate. We use novel and comprehensive data on trade policies since the financial crisis compiled by Global Trade Alert. In particular, the data we use cover a wide range of different types of trade policies, for example traditional forms of protectionist policies such as trade defence and tariff measures. Importantly, the data we use also cover "murky" trade policy measures which have been shown to be a particularly important dimension of trade policies since the financial crisis. Murky trade policy resorts to state measures that may at least potentially abuse policy space granted in international trade agreements or that are beyond the latters reach in order to discriminate against foreign producers, for example health and safety regulations or buy-local clauses in stimulus and bail out packages. Our main empirical results are that the relationship between growth, competitiveness and trade protectionism documented for the time period prior to the financial crisis continues to hold, and that this finding holds for

a wide array of contemporary trade policies, including murky measures. Moreover, we also find that trade policies of G20 advanced economies responded more strongly to changes in domestic growth and real exchange rates than those of G20 emerging market economies. Furthermore, we find that G20 economies trade policies vis--vis other G20 economies were less responsive to changes in competitiveness than those pursued vis--vis non-G20 economies. Our results suggest that especially in light of the sluggish recovery the global economy continues to be exposed to the risk of a creeping return of trade protectionism.

1 Introduction

The eruption of the financial crisis was followed by a sharp global economic downturn and a collapse in world trade. Interestingly, existing evidence suggests that the protectionist response to this slowdown in global economic activity has been remarkably modest (Rodrik, 2009; Bown, 2011a). The restraint in global protectionist activity appears particularly benign in light of firmly established evidence for the decades prior to the financial crisis documenting that countries typically adopted trade-restrictive policies when they experienced recessions and/or losses in competitiveness through an appreciation of the real exchange rate; some evidence also suggests that countries erected trade barriers when their trading partners experienced weaker growth.[1] The combination of a sharp global economic downturn and the sluggish recovery from the financial crisis on the one hand and the muted protectionist activity on the other hand thus raise the question whether the relationship between growth, real exchange rates and trade protectionism has broken down more recently, or whether the global economy might still be exposed to the risk of a creeping return of trade protectionism.

In order to shed light on this question, we use novel data released by Global Trade Alert (GTA) which cover a wide array of trade policies, ranging from traditional trade policies such as tariff or trade defence to non-traditional policies with discriminatory potential—so-called "murky" measures—implemented by more than 140 countries since GTA's inception in 2008. We estimate bilateral regressions of the number of newly implemented trade-restrictive measures on the real bilateral exchange rate, implementing country's and affected trading partner's real GDP growth. We find that in the period since the financial crisis—despite the overall limited recourse to trade protectionism—countries did adopt trade-restrictive policies when they experienced weaker domestic growth and losses in competitiveness through an appreciation of the real exchange rate. Moreover, in contrast to the evidence from the decades prior to the financial crisis we find that G20 economies refrained from erecting trade barriers against trading partners which experienced their own economic downturn. In addition, we find that trade policies of G20 advanced economies (AEs) responded more strongly to changes in domestic growth and real exchange rates than those of G20 emerging market economies (EMEs). Finally, we find that trade policies of G20 economies vis-à-vis other G20 economies were less responsive to losses in competitiveness through an appreciation of the real exchange rate than trade policies of G20 economies vis-à-vis non-G20 economies.

The innovations to the literature our paper provides are twofold. First, our paper is the first to provide robust empirical evidence showing that despite the overall limited recourse to trade protectionism in the period since the financial crisis, countries did systematically respond to recessions and losses in competitiveness through an appreciation of the real exchange rate by adopting trade-restrictive policies. Second, in establishing this evidence we consider dimensions

[1] See Section 2 for a discussion of the relevant literature on the relationship between growth, real exchange rates and trade protectionism.

of trade policy that stretch beyond tariffs and trade defence measures and that have so far not been considered in this literature; importantly, the trade policy data we consider include "murky" measures, that is, state measures which at least potentially abuse policy space granted in international trade agreements or that are beyond the latter's reach in order to discriminate against foreign producers; examples for "murky" measures include health and safety regulations or buy-local clauses in stimulus and bail-out packages. Our results suggest that the relationship between growth, real exchange rates and trade protectionism for the time period since the financial crisis holds both for traditional and non-traditional "murky" trade policy measures.

The rest of this paper is organised as follows. We first review the existing empirical literature on the relationship between growth, real exchange rates and trade protectionism as well as on the protectionist response to the financial crisis in Section 2. In Section 3, we describe and analyse the dataset on trade policies that underlies our empirical analysis. Section 4 presents our empirical model and core results. Extensive robustness checks are discussed in Section 5. Finally, Section 6 concludes.

2 Related Literature

Our paper is related to two strands of the literature. First, our paper contributes to the empirical literature on the relationship between growth, real exchange rates and trade protectionism. In general, the evidence put forth in this literature suggests that there existed a close relationship between growth, real exchange rates and trade protectionism before the financial crisis. For the time period from 1980 to 1998, Knetter and Prusa (2003) detect a negative relationship between declines in domestic growth and losses in competitiveness through an appreciation of the real exchange rate on the the one hand and anti-dumping (AD) filings for the US, the EU, Australia, and Canada against their main trading partners on the other hand. Using aggregate instead of bilateral data in order to extend the time-series coverage back to 1947, Irwin (2005) confirms the results of Knetter and Prusa (2003) for the US. Bown (2008) analyses data on 28 industries in Argentina, Brazil, Colombia, India, Indonesia, Mexico, Peru, Turkey and Venezuela over the time period from 1995 to 2004, and finds that declines in domestic growth and losses in competitiveness increased the probability of an industry filing for an AD investigation. Focusing on trading partners' economic conditions, Crowley (2011) sets up a two-stage binary choice model with selection for AD filings by 28 US industries and AD decisions by the US government against 49 trading partners for the time period from 1980 to 2001; she finds that weak growth in trading partners' economies increased the likelihood of a US industry submitting an AD filing and the authorities imposing (preliminary) duties. Bown and Crowley (2013) analyse data on AD and global as well as China-specific safeguard initiations for the time period from 1988 to 2008 for the US, the EU, Australia, South Korea and Canada. They find that losses in competitiveness through an appreciation of the real exchange rate and rising domestic unemployment were

typically associated with an increase in the number of trade defence investigations initiated; they also find that trade defence investigations were typically initiated against trading partners that were going through their own periods of weak economic growth. Bown and Crowley (2012) adopt the framework of Bown and Crowley (2013) and apply it to data for 13 EMEs over the time period from 1995 to 2008. As for AEs in Bown and Crowley (2013), Bown and Crowley (2012) find that losses in competitiveness and weak domestic growth were typically followed by a higher demand for trade defence measures. Finally, Pelc and Davis (2012) study data on trade defence investigations for 23 advanced, emerging market and developing economies over the time period from 1997 to 2009, and find that a domestic crisis increased the demand for trade protectionism, but that this effect was attenuated if the crisis had a globally systemic reach. Pelc and Davis (2012) also find that losses in competitiveness through an appreciation of the real exchange rate increased the likelihood of trade defence investigations being initiated.[2] An exception in the sense that it does not find trade protectionism to be closely related to growth is the study of Rose (2012), which investigates a large number of different trade policy measures and the business cycle for up to 180 countries over the last 40 years. While the results of Rose (2012) seem to suggest that trade protectionism has not been countercyclical, his analysis possibly suffers from problems regarding model specification and data. Most importantly, the approach of Rose (2012) does not account for the shift in the composition of trade policies that has arisen for many EMEs, which progressively used trade defence measures rather than tariffs after they joined the WTO (Bown and Crowley, 2012).

To the best of our knowledge, to this date there exist only two studies which attempt to extend their analysis of the relationship between growth, real exchange rates and trade protectionism to the time period since the financial crisis. Bown and Crowley (2012, 2013) add interaction terms to their model in order to test whether the relationship between growth, real exchange rates and trade protectionism during the financial crisis has changed relative to the pre-crisis period. For AEs, they conclude that "the model estimates are not sufficiently robust across specifications to allow us to conclude [whether or not] there was a change in the relationship between trade policy and the exchange rate or between trade policy and domestic unemployment during the Great Recession" (Bown and Crowley, 2013, p. 12). Similarly, for EMEs they "caution against drawing too much inference from the post-Great Recession [results], given that identification is coming off only two [time-series observations]" (Bown and Crowley, 2012, p. 22). Only for the relationship between trading partners' growth and trade policies do Bown and Crowley (2013) provide robust evidence for a change in the time period since the financial crisis: in contrast to the pre-crisis period, AEs refrained from imposing trade-restrictive policies against trading partners undergoing their own periods of weak economic growth.

[2] For the sake of brevity, we only discuss the more recent empirical literature. Earlier contributions include Takacs (1981), Salvatore (1987), Grilli (1988), Coughlin et al. (1989), Bohara and Kaempfer (1991) as well as Leidy (1997) and generally arrive at the same conclusions as the papers discussed above, at least regarding the effects of domestic growth and real exchange rates.

Second, our paper is motivated by the literature which finds that protectionist activity since the financial crisis was modest. For example, Bown (2011b) finds that trade policies—in particular in terms of temporary trade barriers—of major G20 economies essentially followed their pre-crisis trends during the years after the financial crisis. Kee et al. (2010) set up the overall trade restrictiveness indicator (OTRI) as that uniform tariff that if imposed on all home imports instead of the existing structure of tariffs and AD duties would leave aggregate imports at their current level. Kee et al. (2010) calculate the OTRI for 135 countries for 2008 and 2009, and find that there was no widespread increase in protectionism via tariffs in response to the financial crisis. Gawande et al. (2011) examine data on the difference between applied bilateral and upper-bound tariffs that are consistent with WTO commitments for Argentina, Brazil, China, India, Mexico, Turkey and South Africa. They find that most of these countries did not utilise their trade policy space in response to the financial crisis. Vandenbussche and Viegelahn (2011) fail to find a regime shift in the EU's trade policy in terms of AD investigation initiations after the financial crisis. Vandenbussche and Viegelahn (2011) also fail to detect an increase in the EU's most-favored nation tariff rates after the financial crisis. Finally, it its latest Report on G20 Trade and Investment Measures also the World Trade Organization reaches the conclusion that "on the whole, most countries have, so far, resisted resorting to widespread protectionism" (WTO, 2013).[3]

Our paper contributes to the literature along two dimensions. First, given the firmly-established evidence for a relationship between growth, real exchange rates and trade protectionism for the decades prior to the financial crisis and the limited protectionist activity since then, it is natural to ask whether this relationship has broken down more recently, or whether the sluggish recovery of the global economy may give rise to a creeping return of trade protectionism at some point further down the road; to date, there does not exist conclusive empirical evidence that could shed light on this question. Second, in contrast to the existing empirical literature on the relationship between growth, real exchange rates and trade protectionism we consider trade policies that stretch beyond tariffs and trade defence measures. This broadening of perspective seems to be warranted by several trade policy observers arguing that governments progressively resort to "murky" measures—state measures that at least potentially abuse policy space granted in international trade agreements or that are beyond the latter's reach in order to discriminate against foreign producers(Baldwin and Evenett, 2009; Evenett and Wermelinger, 2010; Cernat and Madsen, 2011). The need to broaden the perspective to include these new types of trade policy measures is further corroborated by empirical evidence which suggests that these have a stronger effect on trade flows than traditional trade policies (Henn and McDonald, 2011).

Our paper aims to fill these gaps: We provide robust empirical evidence which shows that—despite the limited overall protectionist activity since the financial crisis—the relationship be-

[3]While the GTA Reports generally paint a more pessimistic picture of global trends in trade protectionism (see, for example, GTA, 2012, 2013), it has been shown that a detailed analysis of the GTA dataset suggests a more nuanced picture (ECB, 2013).

tween growth, real exchange rates and trade protectionism continues to hold; and that it also applies to the recently important "murky" trade policies.

3 Global Trade Alert Data

GTA is an independent initiative created in 2008 in order to monitor trade policies after the financial crisis.[4] As of today, GTA provides the most comprehensive dataset on trade policy measures implemented since 2009.[5] For each newly implemented trade policy measure the GTA dataset features information on: trade policy measure category; whether the measure is trade-restrictive or trade-liberalising; date of inception; date of reporting in the GTA dataset; duration over which the measure will be in place; countries, sectors and product lines affected. As of April 2013, the GTA dataset documents around 2,600 trade policy measures that have been implemented by 140 countries since 2009. The collection of this data is carried out by regional GTA nodes which monitor trade policy in their region. In addition to the regional nodes, third parties are encouraged to report the implementation of trade policy measures for scrutiny as well.

There exist only few empirical studies which have used the GTA dataset so far. One example is Henn and McDonald (2011) who estimate the effects of newly implemented trade-restrictive measures on bilateral trade flows, finding that only a marginal fraction of the decline in world trade until 2010 can be attributed to protectionist trade policies. Evenett et al. (2011) test empirically the cross-sectional predictions of the Grossman and Helpman (1994) model for trade policy determination using the GTA dataset for the time period from 2008 to 2010. In particular, Evenett et al. (2011) argue that traditional trade policy models fail to account for the role played by international trade agreements and the progressive fragmentation of supply chains across countries. Finally, Boffa and Olarreaga (2012) investigate whether trade policies since the financial crisis have been driven by retaliation using the GTA dataset.

Table 1 presents the number of newly implemented trade policy measures per trade category in the GTA dataset. We divide the trade policy measures reported in the GTA dataset into three categories: "murky" measures, traditional measures and a residual category, labeled non-tariff barriers by GTA. Table 1 illustrates that "murky" measures have been a quantitatively important dimension of trade policy since the financial crisis, accounting for close to 40% of all newly implemented measures. The importance of "murky" measures reported in Table 1 is in

[4] GTA is located at the University of St. Gallen, and is directed and advised by Professors Simon Evenett and Richard Baldwin, respectively.

[5] The WTO and the EC also provide datasets on trade policy measures implemented since the financial crisis. However, we do not use the data underlying WTO (2012) owing to the fact that it does not include "murky" measures of trade protectionism. The data underlying European Commission (2012) suffer from similar shortcomings, and in addition cover only a much smaller set of countries. Finally, the Temporary Trade Barriers Database (Bown, 2010) covers data on trade defence measures only.

line with the oft-cited change in trade policies after the financial crisis (Baldwin and Evenett, 2009; Evenett and Wermelinger, 2010; Cernat and Madsen, 2011). This stresses the importance of considering "murky" measures along with traditional trade policy measures in the analysis of the relationship between growth, real exchange rates and trade protectionism.[6]

Unfortunately, analysis of trends in trade protectionism using the GTA dataset is not straightforward. Since national governments are not required to report to GTA when they implement a trade policy measure, GTA needs to carry out its own data gathering; as a result, the implementation of a trade-related measure may be detected and reported by GTA only with a lag; in this case, the comparison of the numbers of trade policy measures that were implemented at two different points in time is blurred. For example, it is a priori unclear to what extent the declining trend in the number of newly implemented trade-restrictive measures across all countries displayed in Figure 1 is due to weakening protectionist momentum, or the fact that as of April 2013 GTA staff has had much less time to detect and report trade-restrictive measures that were implemented in 2013Q1 than it has had for those that were implemented in 2009Q1.[7,8]

In the descriptive analysis carried out in this section, we address the distortion caused by reporting lags by considering the ratio between newly implemented trade-restrictive measures and newly implemented trade-liberalising measures in each quarter; we refer to this metric as the "red-to-green" ratio in the following. Figure 2 depicts the evolution of the red-to-green ratio for AEs as well as for EMEs and developing countries in Latin America, Asia, the former Soviet Union and Africa for all trade policy measure categories listed in Table 1. The upper panel in Figure 2 suggests that the immediate response to the financial crisis was notably protectionist in AEs: the number of newly implemented trade-restrictive measures was substantially larger than the corresponding number of trade-liberalising measures. Splitting the total number of trade-restrictive and trade-liberalising measures into traditional and "murky" trade policy measures and calculating the corresponding red-to-green ratios, suggests that the initial spike in AEs was primarily due to the extensive use of "murky" measures (see the lower panel in Figure 2): at the height of the financial crisis bail-out and state aid measures accounted for 40% of all newly implemented trade-restrictive measures, and about 75% of these bail-out measures were

[6] A recent example for "murky" protectionism can be gleaned from Ukrainian complaints in August 2013 about changes in procedures by Russian customs authorities. While Ukrainian companies acknowledged that no new trade restrictions had been introduced *officially* by the Russian side nor had there been any legislative changes, they reported that a number of Ukrainian products started facing additional custom control measures, time-consuming document and product inspections. The Financial Times commented that "this marks an escalation in a wave of trade rows that has seen Moscow (...) stepping up restrictions on Ukrainian imports, citing health and quality control concerns, in effect bypassing international free trade commitments" (Financial Times, 2013).

[7] Notice that due to missing data in the GTA dataset for the time period before the financial crisis, in this paper we do not compare trends in trade protectionism since 2009 to long-term trends. Rather, we aim to assess whether the relationship between growth, real exchange rates and trade protectionism documented extensively for the decades prior to the financial crisis continues to hold.

[8] A separate issue is that GTA may fail—even beyond reporting lags—to detect the full set of newly implemented trade-related measures because no country is required to report to GTA. However, notice that this would lead to a systematic underreporting of protectionist pressures, which would make it *more* difficult for us to reject the null of *no* systematic relationship between growth, real exchange rates and trade protectionism.

implemented in AEs. After the height of the financial crisis, however, protectionist pressures eased in AEs. In contrast, in EMEs and developing countries around the world protectionist momentum as measured by the red-to-green ratio has been broadly stable since the financial crisis.

Figure 3 suggests that trade policy agendas have been different across economies even after the height of the financial crisis. For instance, "murky" protectionism (in particular in the form of bail-out and state aid) has been important in Russia, China and in AEs in general. Trade defence has been an important element of trade policy in Brazil, China, the US and Argentina, but less so in the EU and in Russia (which joined the WTO only in August 2012). Tariff measures did play some role for Brazil and Russia, but did not account for a large share of all implemented trade-restrictive measures in the other four economies. Finally, the trade policy agenda of Argentina has been dominated by recourse to non-tariff barriers. The heterogeneity in the trade policy agendas of these selected countries and the widespread use of non-traditional, "murky" measures underlines that focusing only on tariffs or trade defence is likely to miss important aspects of contemporary trade protectionism.

Finally, Table 2 provides information on the distribution of newly implemented trade-restrictive measures across implementing G20 economies and affected economies that is considered in our regressions in Section 4. The table reports for each G20 economy the number of newly implemented trade-*restrictive* measures (first column), the total number of trading partners affected (second column), and the average number of trade-restrictive measures by which each of the trading partners was affected (third column). The table also presents these figures splitting the affected countries into G20 (columns four and five) and non-G20 economies (columns six and seven). For instance, between 2009Q1 and 2012Q2 Argentina implemented 212 trade-restrictive measures which affected 51 economies (17 G20 economies and 34 non-G20 economies). On average, each of these newly implemented measures targeted 33 economies. While Table 2 suggests that there is a large variation in the use of trade-restrictive measures across G20 economies, G20 EMEs stand out as having resorted much more frequently to trade-restrictive policies than G20 AEs. Moreover, as displayed in columns (5) and (7), G20 economies have erected more trade barriers against other G20 economies than against non-G20 economies, most likely owing to trade volumes among G20 economies being substantially larger than between G20 and non-G20 economies.

4 Empirical Model and Results

4.1 Empirical Model

The sample we consider in our empirical analysis spans the time period from 2009Q1 to 2012Q2 and includes observations on trade-restrictive measures implemented by all G20 economies vis-

à-vis their trading partners.[9] Table 3 presents the number of newly implemented trade policy measures per trade category and weighted by the number of affected trading partners as they enter our regressions.[10] For each G20 economy, the set of trading partners comprises all countries for which we were able to find (i) data on growth and real exchange rates, and for which (ii) GTA reports at least one trade-related measure that has been implemented over the period under review (see Table 2 for an overview of the number of trading partners of each G20 economy included in the regression sample).

We follow the existing literature and consider the number of trade-restrictive measures y_{ijt} implemented by country i against country j in quarter t as dependent variable (Knetter and Prusa, 2003; Bown and Crowley, 2012, 2013).[11,12] Because many of the trade policy measures we consider in this paper are implemented against individual trading partners, we expect sharper results from using bilateral rather than aggregate data and regressions (Knetter and Prusa, 2003).[13] In particular, we relate the number of trade-restrictive measures implemented by country i against trading partner j in quarter t, y_{ijt}, to (100 times) the first differences of the logarithm of the real bilateral exchange rate between countries i and j, year-on-year real GDP growth of the implementing country i, and year-on-year real GDP growth of the affected country j.

We use year-on-year growth rates for real GDP in order to be able to include China, which accounts for a large number of trade-restrictive measures implemented since the financial crisis. We obtain year-on-year real GDP growth data from the IMF's International Financial Statistics database. We consider GDP instead of the unemployment rate as a measure of real activity

[9] The share of global trade accounted for by the country pairs we consider is about 75%.

[10] We do not consider all measures that are reported in the GTA dataset in our regressions (see Table 1), largely owing to the lack of information on the implementation date or the affected trading partners. Specifically, we drop 18% of the observations from the GTA dataset. The relative importance of the different measure categories is, however, very similar in the full GTA dataset and the sample that we use for our empirical analysis.

[11] Ideally, one would consider the volume or the share of a country's trade that is affected by newly implemented trade-restrictive measures, possibly also accounting for heterogeneity in the impact of different types of trade-restrictive measures. However, the literature has to date focused on using the count of newly implemented measures, mainly due to missing information on the heterogeneity of the impact of different trade policy measures on trade flows, the lack of sufficiently disaggregated trade flow data and the difficulties to identify affected products.

[12] We do not include the observations on trade defence investigation *initiations* reported in the GTA dataset in our baseline sample. The reason is that we cannot assign these measures to a particular quarter as GTA does not report inception dates for trade defence investigation initiations. Ideally, we would of course consider trade defence investigation initiations because as well it has been shown that even investigations that precede the imposition of duties may have a trade-chilling effect (Staiger and Wolak, 1994). In Section 5, we report robustness checks in which we include trade defence investigation initiations in our regressions assuming that their reporting date is equal to their inception date.

[13] As many trade-related measures affect particular sectors only, one might obtain even sharper results from adding the sectoral dimension to the analysis. However, setting up a dataset with consistent definitions of sectoral production and prices and trade-related measures across G20 countries is a challenging undertaking. We leave this for future research. In the meantime, it should be noted that inspecting aggregate data when the true model, in fact, relates *sectoral* growth and real exchange rates to trade protectionism introduces noise in the analysis which makes it *more* difficult for us to reject the null of *no* systematic relationship between growth, real exchange rates and trade protectionism.

because the latter is available for a smaller set of countries only, and because it is likely to be subject to substantial measurement error for EMEs and developing countries. Data on the real bilateral exchange rate is obtained from the Economic Research Service of the U.S. Department of Agriculture. The bilateral exchange rate is denoted in terms of units of the currency of country j per unit of the currency of the domestic country i; thus, an increase of the real bilateral exchange rate represents an appreciation of the domestic currency and a loss in competitiveness through an appreciation of the real exchange rate of the domestic economy. Table 4 presents descriptive statistics for our dependent and explanatory variables.

We account for the integer nature of the dependent variable (the number of newly implemented trade-restrictive measures) by considering a non-linear regression model. As our dependent variable displays over-dispersion, assuming a standard Poisson distribution is inappropriate.[14] Therefore, we consider a negative binomial regression model, which can be derived by introducing random variation into the conditional mean of the standard Poisson model (Hausman et al., 1984). Denoting by $\lambda_{ijt} = e^{(\boldsymbol{x}_{ijt}\boldsymbol{\beta})}$ the conditional mean of the Poisson distribution given the vector of explanatory variables $\boldsymbol{x}_{ijt}$, following Greene (2008) one can write

$$E(y_{ijt}|\boldsymbol{x}_{ijt}, u_{ijt}) = \boldsymbol{x}_{ijt}\boldsymbol{\beta} + \epsilon_{ijt} = log\ \lambda_{ijt} + log\ u_{ijt}, \tag{1}$$

where $u_{ijt} = e^{\epsilon_{ijt}}$ introduces unobserved random heterogeneity in the conditional mean of the Poisson distribution and is assumed to follow a Gamma distribution. We include country-pair and time fixed effects in $\boldsymbol{x}_{ijt}$ in order to control for unobserved, country-pair specific, time-invariant heterogeneity and unobserved common factors, respectively. For example, the country-pair fixed effects pick up systematic variation in the number of newly implemented trade-restrictive measures stemming from differences in country(-pair) size;[15] the time fixed effects, in turn, capture the bias in the time-series variation in the GTA dataset introduced by reporting lags, see Section 3.[16] In order to further mitigate the possible impact of reporting lags on our results, our sample only runs until 2012Q2, although the GTA dataset also covers 2013Q1 as of April 2013. This we do because the variation in the reporting lags should be smaller for time periods farther in the past.[17] In Section 5, we carry out a number of robustness checks in which we adopt alternative approaches to account for the reporting lags in the GTA dataset.

Following standard practice for negative binomial regressions, we report incidence rate ratios

[14] The null hypotheses that the variance is equal to one—implying a Poisson distribution for the dependent variable—can be rejected at the 1% significance level based on a likelihood ratio test.

[15] Country-pair fixed effects would fail to control for this size effect if relative bilateral trade volumes changed over time. However, IMF Direction of Trade Statistics suggest that for the G20 countries there has not been a noteworthy change in bilateral trade shares over the time period from 2008 to 2012: the average change has been virtually nil, with a standard deviation of only 1.6 percentage points.

[16] We do not account for country-pair heterogeneity in the slope coefficients β that could arise, for example, through the existence of multilateral free trade agreements, such as the EU's single market. Notice, however, that if such heterogeneity amounts to slope coefficients being zero for some country pairs, a positive (negative) pooled slope estimate will reflect that the average of the true country-pair specific slope coefficients is positive (negative).

[17] Our results are robust to extending the sample to 2013Q1. The results are available upon request.

(IRR) instead of the coefficient estimates $\widehat{\boldsymbol{\beta}}$. For example, an IRR of 1.01 means that if the value of the corresponding regressor was increased by one unit, then the count of newly implemented trade-restrictive measures y_{ijt} would *increase* by one percent. Analogously, an IRR of 0.99 means that if the value of the corresponding regressor was increased by one unit, then the count of newly implemented trade-restrictive measures y_{ijt} would *decline* by one percent.

4.2 Empirical Results

4.2.1 Baseline Results

The results for the incidence rate ratio estimates for our baseline specification are presented in column (1) of Table 5. Our main findings regarding the relationship between growth, real exchange rates and trade protectionism since the financial crisis are the following: First, countries implemented more trade-restrictive measures when they experienced lower growth; a drop in growth in country i by one percentage point typically triggered an increase in the number of trade-restrictive measures implemented against trading partner j by 4.4%. Second, countries implemented more trade-restrictive measures when their competitiveness deteriorated; a one percent real appreciation of country i's currency was typically associated with an increase in the number of newly implemented trade-restrictive measures against trading partner j by 1%. Finally, we find that trading partner's growth was unrelated to the trade policies of G20 economies in the period since the financial crisis. Our results, thus, offer an explanation that may reconcile the limited overall recourse to trade-restrictive policies in the presence of a systematic relationship between growth and trade protectionism: the reluctance of countries to erect trade barriers against trading partners that experienced weaker economic growth.[18]

These results demonstrate that the relationship between domestic growth and real exchange rates on the one hand and protectionist activity on the other hand documented extensively for the pre-crisis period continues to hold in the period since the financial crisis. In addition, our results illustrate that this relationship is not confined to traditional trade policies such as tariff and trade defence measures, but also holds when the recently important dimension of "murky" protectionism is considered as well. Moreover, we find that after the financial crisis countries have refrained from pursuing protectionist policies against trading partners which experience their own periods of weak economic growth. Notice, that this result contrasts with the evidence for the decades prior to the financial crisis, which suggests that countries imposed trade-restrictive policies against trading partners experiencing lower growth (Bown and Crowley, 2013); this

[18]The literature has suggested a number of additional potential reasons for the modest protectionist activity since the financial crisis: favorable exchange rate developments in crisis-struck economies, the existence of social safety nets in advanced economies, concerted stimulus packages, by now difficult-to-change national legislations on trade liberalization, the increased lobbying power of export sectors, the lack of policy space due to World Trade Organization (WTO) rules and regional trade agreements as well as the progressive vertical fragmentation of supply chains across countries (Dadush et al., 2011; Gawande et al., 2011; Bown and Crowley, 2013).

result may be one explanation for the limited overall protectionist activity since the financial crisis.

4.2.2 Results by *Implementing* Country Groups

The GTA dataset features information on trade-restrictive measures for all G20 economies and thus allows us to test whether the relationship between growth, real exchange rates and trade protectionism displays heterogeneity across country groups. Columns (2) and (3) of Table 5 present the regression results for samples split by whether the trade policy measure *implementing* country is a G20 AE or a G20 EME. The findings for the subsamples are qualitatively unchanged relative to the baseline specification: lower domestic GDP growth and an appreciation of the real exchange rate were typically followed by an increase in the number of newly implemented trade-restrictive measures, both in G20 AEs as well as in G20 EMEs; moreover, neither G20 AEs nor G20 EMEs considered their trading partners' GDP growth when erecting trade barriers. However, there exist quantitative differences between the regression results for G20 AEs and G20 EMEs. The statistics in column (4) refer to tests for equality of the coefficient estimates of G20 AEs and G20 EMEs.[19] The test statistic for the coefficients of the real bilateral exchange rate is statistically significant, suggesting that the relationship between changes in real exchange rates and trade protectionism has been weaker for G20 EMEs than for G20 AEs. Similarly, the test statistic for the coefficients of domestic GDP growth are statistically significant as well, implying that the negative impact of changes in domestic growth on protectionist activity has been weaker for G20 EMEs than for G20 AEs.

Taken together, our results suggest that the protectionist response of G20 governments' trade policies to changes in domestic growth and real exchange rates since the financial crisis has been weaker for G20 EMEs than for G20 AEs. Notice, however, that this does not necessarily imply that G20 EMEs resorted less to trade protectionism than G20 AEs. Figure 4 displays the coefficient estimates of a regression which includes interactions of the G20 EME dummy variable and the time fixed effects.[20] These interactions reflect the percentage difference between the number of trade-restrictive measures implemented by G20 EMEs and the number of measures implemented by G20 AEs in a given quarter, after controlling for domestic and trading partner GDP growth, changes in real exchange rates and differences in the coefficients of these three regressors as well country-pair and time fixed effects. Figure 4 illustrates that this difference has not been statistically significantly different from zero in the period between 2009Q1 and 2010Q3. However, in four out of the seven most recent quarters in our sample, G20 EMEs implemented statistically significantly more trade-restrictive measures than G20 AEs. In 2012Q2, this difference peaked at 127%, suggesting that G20 EMEs implemented more than twice as

[19] The test statistics stem from a regression which includes interaction terms between all regressors, including the time fixed effects, and the G20 EME dummy variable. The regression results are available upon request.

[20] This is the regression which is used in the tests for equality of coefficients in the previous paragraph.

many trade-restrictive measures as G20 AEs, after controlling for domestic and trading partner growth as well as changes in real exchange rates.

4.2.3 Results by *Affected* Country Groups

Next, we examine whether the trade policy responses of G20 countries to changes in GDP growth and real exchange rates differed across *affected* trading partners. Columns (5) and (6) of Table 5 present estimates for the full sample split by affected trading partners. Column (5) reports results based only on observations on trade-restrictive measures implemented by G20 against other G20 countries. Analogously, the results displayed in column (6) are based on observations on trade-restrictive measures implemented by G20 against non-G20 countries only. In both cases, the qualitative pattern is essentially the same as in the baseline results. The test statistics in column (7) refer to tests for equality of the effect of changes in growth and real exchange rates on G20 trade policies vis-à-vis other G20 countries versus trade policies vis-à-vis non-G20 countries.[21] The statistically significant test statistic for the coefficient on the real bilateral exchange rate suggests that when G20 countries erected trade barriers in response to a loss in competitiveness, more trade-restrictive measures were implemented against non-G20 than against G20 countries. By contrast, there is no statistically significant evidence that the relationship between domestic growth and trade protectionism differed depending on whether or not the affected trading partner was a G20 country.

In order to test if G20 countries erected more trade barriers vis-à-vis non-G20 than vis-à-vis other G20 countries—after controlling for growth dynamics, changes in real exchange rates and differences in the corresponding coefficient estimates as well as country pair and time fixed effects—we consider a regression which includes interaction terms between the non-G20 affected trading partner dummy and the time fixed effects. Figure 5 illustrates that at the height of the financial crisis in 2009 and early 2010 G20 countries had, in fact, implemented more trade-restrictive measures against non-G20 than against other G20 countries, at least after holding constant the effects of other determinants. The percentage difference peaked in early 2009, when the number of trade barriers erected by G20 countries against non-G20 countries exceeded the number of trade barriers implemented against other G20 countries by more than 50%. Notice that this result is not inconsistent with our finding that G20 countries on average imposed more trade-restrictive policies against other G20 countries (see Table 2). As discussed in Section 3, country-pair fixed effects control for the fact that trade volumes—and, correspondingly, the number of newly implemented trade-restrictive measures—among G20 countries are substantially larger than between G20 and non-G20 countries. Our results illustrate that after controlling for this size effect, at the height of the financial crisis G20 trade policies vis-à-vis

[21] The test statistics stem from a regression which includes interaction terms between all regressors, including the time fixed effects, and the non-G20 affected trading partner dummy variable. The regression results are available upon request.

non-G20 countries were considerably more protectionist than those vis-à-vis countries under the G20 umbrella.

Taken together, these results suggest that since 2009 G20 trade policies vis-à-vis other G20 countries were less protectionist overall and responded less strongly to changes in real exchange rates than those pursued vis-à-vis non-G20 economies. One possible explanation for this finding is international cooperation among G20 economies.[22]

4.2.4 Results by Trade Policy Measure

Finally, we examine whether or not our baseline findings on the relationship between growth, real exchange rates and trade protectionism are confined to specific dimensions of trade policy. Based on the classification laid out in Table 3, we first distinguish between "murky" and traditional, non-"murky" measures. Second, we restrict the dependent variable to trade defence measures (excluding initiations). This is of interest because the majority of studies documenting the relationship between growth, real exchange rates and trade protectionism has focused on trade defence measures (see Section 2). Finally, we restrict the dependent variable to tariffs, which account for the largest share in total measures in the GTA dataset (see Table 3).

Columns (1) to (4) of Table 6 demonstrate that the relationship between growth, real exchange rates and trade protectionism is neither confined to traditional trade policy measures in general, nor to trade defence measures in particular, but also applies to "murky" measures.[23] This result has two implications. First, it highlights the importance of considering the multiple dimensions of trade policy when analysing the relationship between macroeconomic factors and protectionist policies. Second, the result may partly explain the muted protectionist activity since the financial crisis documented in many studies that consider only traditional trade policy measures: As suggested by the significant coefficients in column (1), governments have responded to weak growth and losses in competitiveness by erecting new "murky" measures, implying that governments may have partly substituted "murky" measures for traditional policy measures (Gawande et al., 2011; Kee et al., 2010; Bown, 2011b); focusing only on traditional measures, thus, falls short of grasping the entire universe of trade protectionist activities over recent years.

[22] G20 state leaders have repeatedly called on themselves to exercise restraint in trade protectionism (G20, 2009, 2013). International cooperation among G20 economies may thus have moderated G20 governments' trade policy response vis-à-vis peer G20 economies relative to those pursued vis-à-vis non-G20 economies. However, there are also alternative explanations for this finding. First, G20 governments might have targeted specific sectors rather than countries, with these sectors being clustered in non-G20 economies. Second, it might be that due to the small size of non-G20 economies the likelihood that they would respond to G20 protectionist trade policies by retaliation was perceived to be smaller.

[23] Notice that in the regression on trade defence measures only (column (3)), the coefficient of the affected trading partner's GDP growth becomes positive and statistically significant at the 10% level. This result is consistent with the findings in Bown and Crowley (2013).

5 Robustness

We test the sensitivity of our baseline results to changes in (i) the choice and specification of the dependent variable and the explanatory variables, (ii) the way reporting lags are addressed and (iii) the model specification.

5.1 Robustness to Choice and Specification of Dependent Variable and Explanatory Variables

The sensitivity tests of our baseline results to changes in the choice of the dependent variable are twofold. First, we replace the number of newly implemented trade-restrictive measures by what we call the "red-minus-green" count: the difference between the number of newly implemented trade-restrictive and trade-liberalising measures.[24] This is an interesting alternative choice for our dependent variable as countries which implement more trade-restrictive measures could at the same time also implement more trade-liberalising measures; in that case, these countries should not necessarily be flagged as having pursued more protectionist trade policies. Second, we include the observations on trade defence investigation *initiations*—that have so far been excluded—in our sample (see the discussion in Section 3). Columns (1) and (2) of Table 7 demonstrate that our results do not change with any of the two alternative choices for the dependent variable.

Next, we check the robustness of our results to changes in the choice and the lag structure of the explanatory variables. Column (3) in Table 7 reports results for a specification in which we replace domestic and trading partners' GDP growth by the change in the corresponding unemployment rate.[25] In line with our baseline results, the IRR of the domestic unemployment rate is larger than unity: A higher unemployment rate in the domestic economy has been associated with an increase in the number of trade-restrictive measures implemented. Also in line with our baseline results, there is no evidence that deteriorating labor market conditions abroad have induced governments to adopt trade-restrictive policies. Our results are therefore also robust to the choice of the measure for real activity. Another robustness check we carry out concerns the timing of the explanatory variables. As the implementation of trade-restrictive measures may take more than a quarter because of time-demanding legal processes, column (4) reports results from a regression in which the second lags of all explanatory variables are substituted for the first lags from the baseline specification. None of our results is affected by this change in the lag structure.

[24] We do not consider robustness checks based on the red-to-green ratio considered in Section 3 as this metric cannot be calculated for the large number of country-pair-quarter observations which have a zero count of trade-liberalising measures.

[25] As for GDP growth, the unemployment rate is measured as the year-on-year change (in percentage points).

5.2 Robustness to Alternative Approaches to Addressing Reporting Lags

Next, we check the sensitivity of our results to alternative approaches to addressing the reporting lags in the GTA dataset. As discussed in Section 3, reporting lags are likely to entail underestimation of recent protectionist pressures. As a consequence, they may lead to failure of finding evidence for a relationship between growth, real exchange rates and trade protectionism, in particular when country-pair fixed effects are entered in the panel regression in order to account for unobserved, time-invariant factors: The inclusion of fixed effects implies that only the within country-pair variation over time is exploited in order to identify the relationship between growth, real exchange rates and trade protectionism; unfortunately, owing to reporting lags the time-series variation in the GTA dataset is biased towards displaying less protectionist activity in more recent periods, and this may bias our coefficient estimates towards zero.

As a first alternative to the time fixed effects in our baseline specification we consider a linear time trend. This alternative specification has the advantage to render our results comparable with the existing literature (Bown and Crowley, 2013; Crowley, 2011). The results in column (5) of Table 7 confirm our baseline results. However, the specification with linear time trend—which we deem to be inferior to our baseline specification with time fixed effects—suggests that trading partner's growth had a tempering effect on trade policies of G20 economies. This result contrasts with findings for the pre-crisis period in much of the literature (Bown and Crowley, 2013; Crowley, 2011), but is consistent with the rationale put forth by Pelc and Davis (2012) who argue that in times of a global crisis the likelihood of retaliation is higher, so that governments shy away from protectionist trade policies.

Including a linear time trend to account for reporting lags requires that the downward bias in the number of newly implemented trade-restrictive measures reported by GTA increases linearly over time. However, in case the data gathering process by GTA has improved over time this bias would grow in a non-linear fashion. Specifically, newly implemented trade-related measures may have been detected with a relatively larger lag at the early stages of GTA, when it was still in progress of setting up regional nodes to improve data collection. This suggests that the bias would grow when considering more recent quarters rather than periods farther in the past, but would do so at a decreasing rate. To address a non-linear reporting lag, we consider an alternative specification with a quadratic time trend. Column (6) in Table 7 suggests that our baseline results remain unchanged.

So far we have assumed that reporting lags are homogenous across countries, both in the time fixed effects as well as in the linear and quadratic time trend specifications. However, it may be the case that the extent of under-reporting in more recent quarters has been less pronounced in some more visible economies in which the data collection process is more comprehensive than in some other economies in which the regional nodes may be less well organised. To control for this heterogeneity in the reporting lags, we add implementing country-specific time trends to

our baseline specification. The results reported in column (7) in Table 7 are similar to those from the baseline.

Finally, another way to address the reporting lags is to consider the number of newly implemented trade-restrictive measures reported by GTA up to a fixed reporting lag as dependent variable: Figure 6 plots the evolution of the number of trade-restrictive measures that were implemented in period t and that were reported in the GTA data in period $t + h$, with each line representing a different reporting lag $h = 0, 1, 2, \ldots, 15$. For example, there were no trade-restrictive measures that were (i) implemented in 2009Q1 and (ii) reported by GTA as of 2009Q1, that is, up to a reporting lag of zero; in contrast, as of 2009Q4 (up to a reporting lag of three) GTA reports about 120 trade-restrictive measures that were implemented in 2009Q1. Investigating the evolution of the total number of newly implemented trade-restrictive measures up to a fixed reporting lag in Figure 6 suggests that protectionist momentum has, at the least, not strengthened since the financial crisis. Column (8) in Table 7 reports results from a regression in which the dependent variable is the number of newly implemented trade-restrictive measures reported by GTA up to a fixed reporting lag of zero. The results suggest that also with this second alternative approach to addressing the reporting lag problem our baseline results are unchanged.[26]

5.3 Robustness to Alternative Model Specifications

The sensitivity tests of our results to alternative model specifications are fourfold. We start by testing wether or not our results are robust to the choice of empirical model framework. Our baseline model accounts for the non-negative count nature of our dependent variable and assumes that the latter follows a negative binomial distribution. However, ignoring the count nature of the number of newly implemented trade-restrictive measures and relaxing the assumption on its distribution by running a linear regression with country-pair and time fixed effects yields very similar results (see column (1) in Table 8). To further test how the estimation technique may influence the estimated parameters, we employ the Poisson pseudo-maximum likelihood estimator—an estimator which allows for consistent estimation for the case in which the dependent variable follows a Poisson distribution but displays over-dispersion—as proposed by Santos Silva and Tenreyro (2011). Column (2) in Table 8 shows that our main findings are qualitatively not affected by this alternative choice of modeling framework.

Next, we allow for persistence in trade policies in three ways. First, we add the first lag of the dependent variable to the baseline negative binomial regression model. Next, we consider a linear panel model by including a lagged dependent variable as regressor. To this end, we use a GMM estimator in order to address the endogeneity bias that arises due to the short length of

[26] The results are unchanged for all fixed reporting lags up to six quarters. Results are available upon request.

our panel ($T = 14$).[27] Employing the GMM estimator has an additional advantage compared to the standard approach, namely that it controls for simultaneous endogeneity of the explanatory variables. Finally, we add the lagged stock of trade-restrictive measures as a regressor to the baseline specification.[28] While the result in column (3) in Table 8 suggests that trade policies have displayed persistence since the financial crisis, the GMM estimates do not imply significant persistence (see column (4)). More importantly, however, the baseline findings regarding the existence of a relationship between growth, real exchange rates and trade protectionism are unchanged in both specifications. Moreover, the GMM results in column (4) suggest that our baseline estimates are unlikely to be driven by an endogeneity bias. Interestingly—but not inconsistent with the notion of trade policies potentially displaying persistence—the IRR estimate of the lagged stock of trade-restrictive measures in column (5) being below unity suggests that countries that have accumulated a greater stock of trade-restrictive measures in the past have tended to implement fewer trade barriers in period t.

As an additional specification test we correct standard errors for heteroscedasticity and cluster them at the level of the implementing country, as our baseline results could be driven by implementing country-specific shocks that are correlated across affected countries.[29] However, as reported in column (6) in Table 8 the estimates of the coefficients of the real bilateral exchange rate and domestic GDP growth remain statistically significant when using robust standard errors.

Finally, we consider an alternative specification of the fixed effects. Instead of country-pair fixed effects, we include separate implementing and affected country fixed effects. Column (7) of Table 8 shows that our baseline results are robust to this alternative specification.

6 Conclusion

On the one hand, empirical evidence from the decades prior to the financial crisis has nourished fears that the persistent fragility of the global economy might give rise to a creeping return

[27] The model is estimated with a reduced set of instruments in order to avoid issues resulting from instrument proliferation as discussed by Roodman (2006). In particular, we restrict the number of lags out of which instruments are constructed to four, collapse the set of instruments and finally extract the principal components. This procedure reduces the instrument count in our model from 425 to 21. In order to further reduce the instrument count we replace the time fixed effects by a linear time trend. We also use the two-step variance matrix estimators and robust standard errors as suggested by Bun and Windmeijer (2010).

[28] The stock is obtained by cumulating for each country and each quarter the number of trade-restrictive measures that have been implemented since 2009Q1 less the number of trade-liberalising measures that have been put in place over the corresponding period.

[29] The baseline regression is based on the standard computational procedure for negative binomial panel regression model implemented in Stata 12. This estimation procedure—labeled "xtnbreg"—does not allow for correcting standard errors, neither for heteroscedasticity nor for clustering. Robust standard errors in column (6) are reported based on a negative binomial least square dummy variable (at the country-pair level) estimator. However, this procedure is computationally very extensive. For this reason, and in the light of the results reported in column (6), we use the standard "xtnbreg" command throughout the paper.

of trade protectionism. In all likelihood, this would depress the global economy, possibly to a similar extent as during the Great Depression in the 1930s. This fear has been exemplified by the titles of the four most recent GTA Reports: "Protectionist's Quiet Return", "Débâcle", "Trade Tensions Mount" and "Resolve Falters As Global Prospects Worsen". On the other hand, because existing evidence suggests that so far protectionist activity since the financial crisis has been muted, one could be tempted to think that international peer pressure, trade policy rules, and the progressive vertical fragmentation of supply chains across countries could have made trade wars a remote threat. This paper contributes to this discussion by providing robust empirical evidence showing that the specter of protectionism has not banished: We find that weak domestic growth and losses in competitiveness through an appreciation of the real exchange rate continue to induce governments to resort to protectionist trade policies. As a consequence, it should be clear that the longer it takes for the global economy to recover, the more hazardous it is to believe that trade protectionism will remain contained. G20 governments have to withstand demands for trade protectionism, and five years after the financial crisis this becomes more difficult the longer their economies do not gain momentum. Thus, efforts in order to strengthen peer pressure, monitoring and international cooperation need to be undertaken ever more forcefully.

References

Baldwin, R., Evenett, S., 2009. The Collapse of Global Trade, Murky Protectionism, and the Crisis: Recommendations for the G20. CEPR.

Boffa, M., Olarreaga, M., 2012. Protectionism During the Crisis: Tit-for-Tat or Chicken-Games? Economics Letters 117 (3), 746–749.

Bohara, A., Kaempfer, W., 1991. A Test of Tariff Endogeneity in the United States. American Economic Review 81 (4), 952–60.

Bown, C., 2008. The WTO And Antidumping In Developing Countries. Economics and Politics 20 (2), 255–288.

Bown, C., 2010. Temporary Trade Barriers Database. The World Bank.

Bown, C., 2011a. Introduction. In: Bown, C. (Ed.), The Great Recession and Import Protection. CEPR and World Bank, Ch. 1, pp. 1–51.

Bown, C., 2011b. Taking Stock of Antidumping, Safeguards and Countervailing Duties, 1990-2009. The World Economy 34 (12), 1955–1998.

Bown, C., Crowley, M., 2012. Emerging Economies, Trade Policy, and Macroeconomic Shocks. Working paper, Federal Reserve Bank of Chicago.

Bown, C., Crowley, M., 2013. Import protection, business cycles, and exchange rates: Evidence from the great recession. Journal of International Economics 90 (1), 50–64.

Bun, M. J. G., Windmeijer, F., 2010. The weak instrument problem of the system gmm estimator in dynamic panel data models. Econometrics Journal 13 (1), 95–126.

Cernat, L., Madsen, M., March 2011. "Murky Protectionism" and Behind-the-Border Barriers: How Big an Issue? The 100 Euro Question. voxeu.org.

Coughlin, C., Terza, J., Khalifah, N., 1989. The Determinants of Escape Clause Petitions. The Review of Economics and Statistics 71 (2), 341–47.

Crowley, M., 2011. Cyclical Dumping and US Antidumping Protections: 1980-2001. Working paper, Federal Reserve Bank of Chicago.

Dadush, U., Ali, S., Odell, R., 2011. Is Protectionism Dying? Working paper, Carnegie Endowment for International Peace.

ECB, July 2013. Is there a risk of a creeping rise in trade protectionism? In: Monthly Bulletin. European Central Bank.

European Commission, 2012. Report on Potentially Trade Restrictive Measures.

Evenett, S., Fritz, J., Gerasimenko, D., Nowakowska, M., Wermelinger, M., 2011. The Resort to Trade Protectionism During the Great Recession: Which Factors Mattered? mimeo, University of St. Gallen.

Evenett, S., Wermelinger, M., 2010. A Snapshot of Contemporary Protectionism: How Important Are the Murkier Forms of Trade Discrimination? In: Rising Non-Tariff Protectionism and Crisis Recovery. Studies in Trade and Investment. United Nations Economic and Social Commission for Asia and the Pacific (ESCAP), Ch. 1, pp. 8–26.

Financial Times, 15 Aug. 2013. Russia accused of triggering trade war with Ukraine. URL http://www.ft.com/cms/s/0/99068c0e-0595-11e3-8ed5-00144feab7de.html#axzz2c877YUdH

G20, 2009. Leaders' Statement. The Pittsburgh Summit, Pittsburgh, September 2009.

G20, 2013. Final Communique. G20 Meeting of Finance Ministers and Central Bank Governors, Washington D.C., April 2013.

Gawande, K., Hoekman, B., Cui, Y., 2011. Determinants of Trade Policy Responses to the 2008 Financial Crisis. World Bank Policy Research Working Paper 5862, The World Bank.

Greene, W., 2008. Econometric Analysis. Vol. 7. Prentice Hall.

Grilli, E., 1988. Macroeconomic Determinants of Trade Protection. The World Economy 11 (3), 313–326.

Grossman, G., Helpman, E., 1994. Protection for Sale. American Economic Review 84 (4), 833–50.

GTA, 2012. Débâcle: The 11th GTA report on protectionism. CEPR.

GTA, 2013. Protectionism's quiet return: GTA's pre-G8 summit report. CEPR.

Hausman, J., Hall, B., Griliches, Z., 1984. Econometric Models for Count Data with an Application to the Patents-Research and Development Relationship. Econometrica 52 (4), 909–938.

Henn, C., McDonald, B., 2011. Protectionist Responses to the Crisis: Damage Observed in Product-Level Trade. Working Paper 139, IMF.

Irwin, D., 2005. The Rise of US Anti-dumping Activity in Historical Perspective. The World Economy 28 (5), 651–668.

Kee, H., Neagu, C., Nicita, A., Apr. 2010. Is Protectionism on the Rise? Assessing National Trade Policies During the Crisis of 2008. Policy Research Working Paper 5274, The World Bank.

Knetter, M., Prusa, T., 2003. Macroeconomic Factors and Antidumping Filings: Evidence from Four Countries. Journal of International Economics 61 (1), 1–17.

Leidy, M., 1997. Macroeconomic Conditions and Pressures for Protection Under Antidumping and Countervailing Duty Laws: Empirical Evidence for the United States. IMF Staff Papers 44 (1), 132–144.

Pelc, K., Davis, C., 2012. Cooperation in Hard Times: Self-Restraint of Trade Protection. mimeo, McGill University.

Rodrik, D., Oct. 2009. The Myth of Rising Protectionism. Project Syndicate Blog.
URL `http://www.project-syndicate.org/commentary/the-myth-of-rising-protectionism`

Roodman, D., Nov. 2006. How to Do xtabond2: An Introduction to Difference and System GMM in Stata. Working Papers 103, Center for Global Development.

Rose, A., 2012. Protectionism Isn't Counter-Cyclic (anymore). Discussion Paper 8937, CEPR.

Salvatore, D., 1987. Import Penetration, Exchange Rates, and Protectionism in the United States. Journal of Policy Modelling 9 (2), 125–141.

Santos Silva, J., Tenreyro, S., 2011. Further simulation evidence on the performance of the poisson pseudo-maximum likelihood estimator. Economics Letters 112 (2), 220–222.

Staiger, R., Wolak, F., 1994. Measuring Industry Specific Protection: Antidumping in the United States. Working Paper 4696, NBER.

Takacs, W., 1981. Pressures for Protection: An Empirical Analysis. Economic Inquiry 19, 687–693.

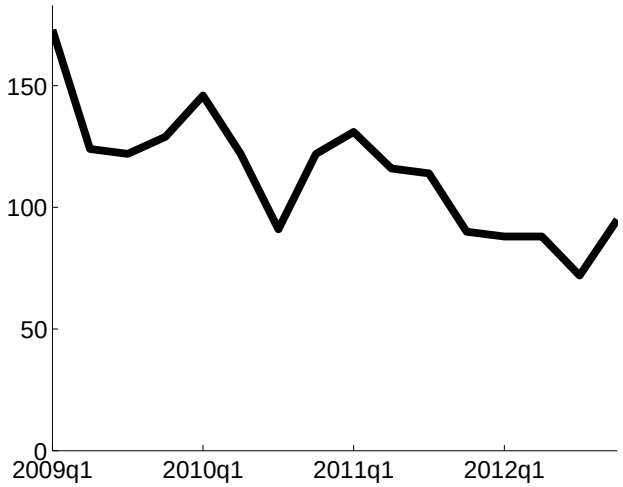
Vandenbussche, H., Viegelahn, C., 2011. No Protectionist Surprises: EU Antidumping Policy Before and During the Great Recession. Open access publications, Katholieke Universiteit Leuven.

WTO, 2012. Report on G20 Trade and Investment Measures. World Trade Organization.

WTO, 2013. Report on G20 Trade and Investment Measures. World Trade Organization.

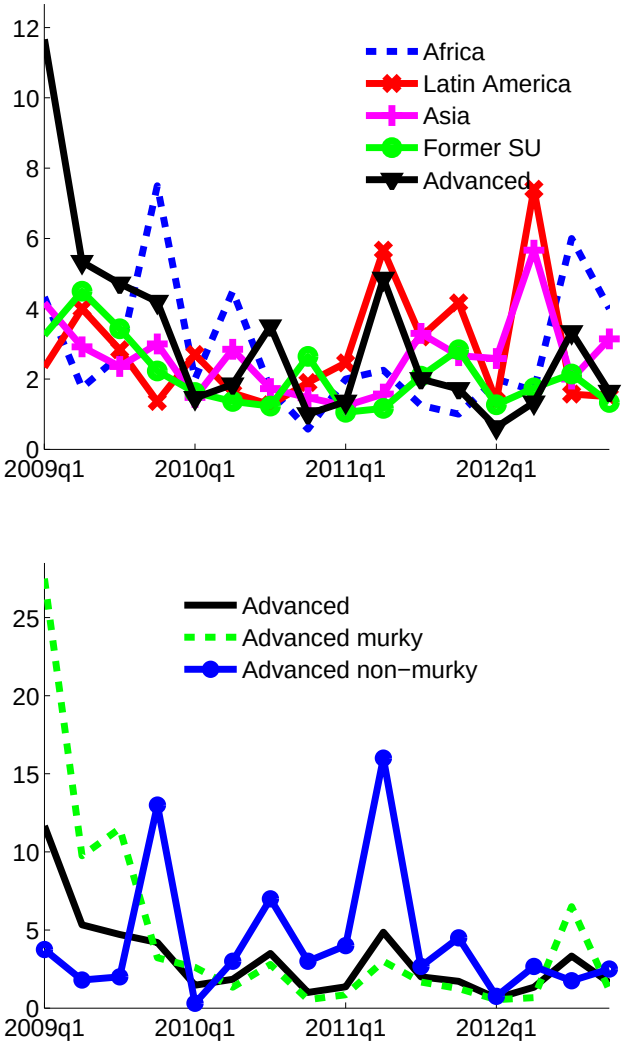
A Figures

Figure 1: Evolution of the Number of Newly Implemented Trade-Restrictive Measures Since the Financial Crisis



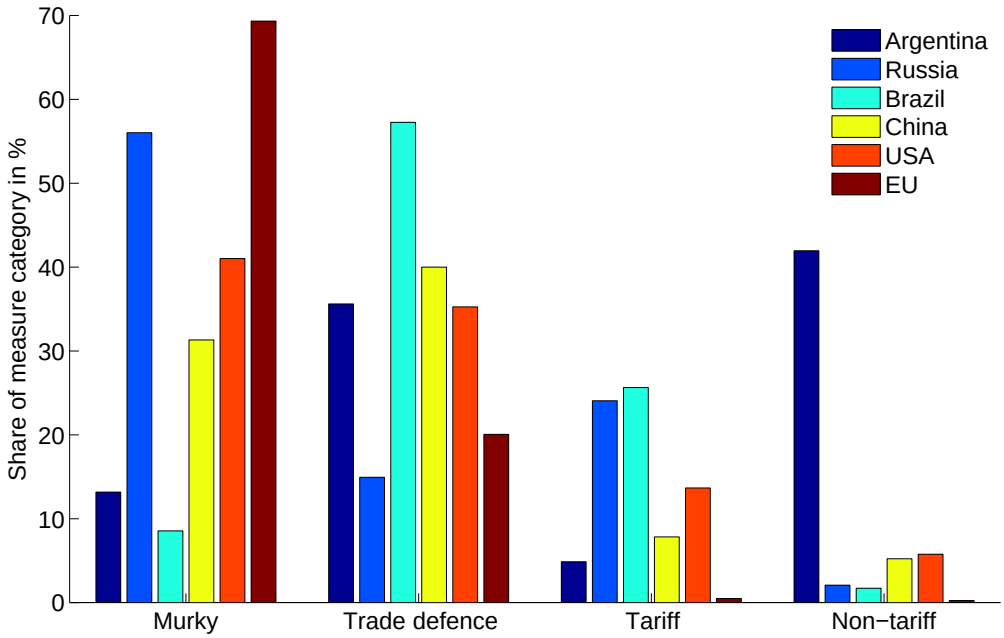
Note: The figure displays the evolution of the number of newly implemented trade-restrictive measures in the GTA data aggregated across countries. Notice, that the number of expired trade-liberalizing (green) measures in the GTA data are added to the number of newly implemented trade-restrictive measures (red and amber) for each quarter.

Figure 2: Evolution of the Ratio Between Newly Implemented Trade-Restrictive and Newly Implemented Trade-Liberalising Measures



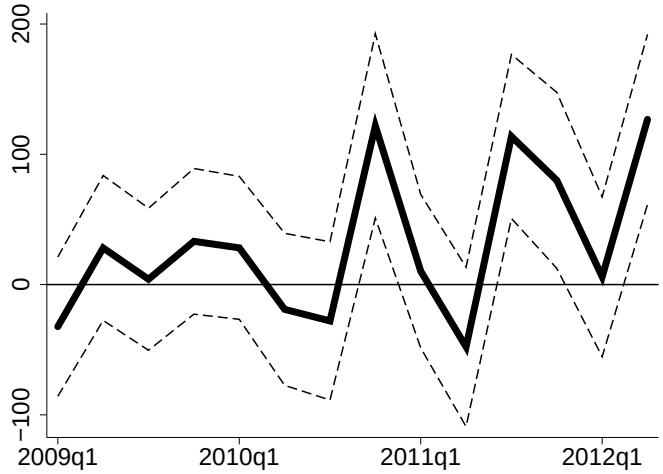
Note: The upper panel displays the evolution of the ratio between newly implemented trade-restrictive and newly implemented trade-liberalising measures in the GTA dataset for AEs and regional aggregates (non-AEs in Latin America, Asia, the former Soviet Union, and Africa). Notice that the number of expired trade-liberalising (green) measures in the GTA dataset are added to the number of newly implemented trade-restrictive measures (red and amber) for each quarter and vice versa. The lower panel displays the evolution of the red-to-green ratio for AEs only and separately for "murky" and traditional non-"murky" measures.

Figure 3: Countries' Trade Policy Agendas



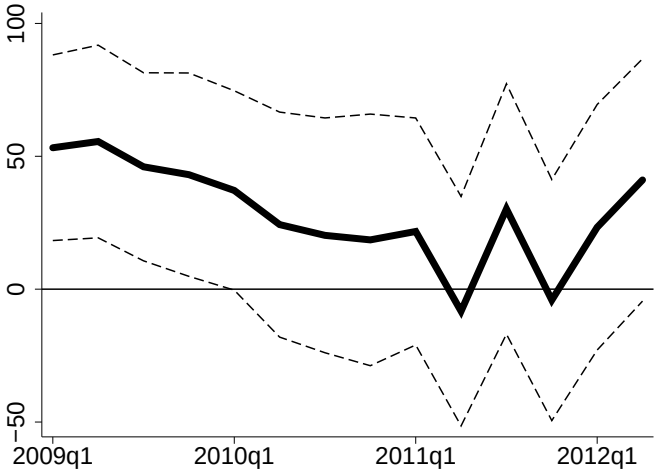
Note: The figure displays shares in the total number of newly implemented trade-restrictive measures accounted for by "murky" measures, trade defence, tariffs and non-tariff measures for the EU, Argentina, Russia, Brazil, China and the US. The shares do not sum to 100% as not all trade-restrictive measures implemented belong to one of these four categories, see Table 1.

Figure 4: Evolution of the Coefficient Estimates of the Interaction Terms of the G20 EME Dummy Variable and the Time Fixed Effects



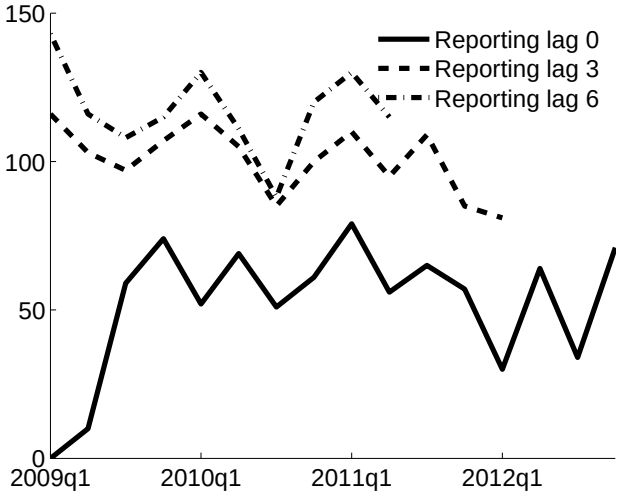
Note: The solid black line displays the evolution of the coefficient estimates of the interaction terms of the G20 EME dummy variable and the time fixed effects (see Section 4.2.2). The dashed lines represent the 95% confidence bands. Notice, that, here, coefficient estimates $\hat{\beta}$ are reported instead of IRRs, with $\hat{\beta} = log(IRR)$.

Figure 5: Evolution of the Coefficient Estimates of the Interaction Terms of the non-G20 Affected Trading-Partner Dummy Variable and the Time Fixed Effects



Note: The solid black line displays the evolution of the coefficient estimates of the interaction terms of the non-G20 trading-partner dummy variable and the time fixed effects (see Section 4.2.3). The dashed lines represent the 95% confidence bands. Notice, that, here, coefficient estimates $\hat{\beta}$ are reported instead of IRRs, with $\hat{\beta} = log(IRR)$.

Figure 6: Evolution of the Aggregate Number of Newly Implemented Trade-Restrictive Measures for Varying Reporting Lags



Note: The figure displays the evolution of the number of newly implemented trade-restrictive measures (red, amber and expired green) in the GTA database aggregated across countries for reporting lags $h = 0, 3, 6$.

B Tables

Table 1: Number of Trade-Related Measures Per Measure Category Considered in GTA Dataset

Measure Category	**Trade Restrictive**	**% of Total**	**Trade Liberalising**	**% of Total**
"Murky" measures				
Bail out / state aid measure	478	18.3	4	0.6
Consumption subsidy	13	0.5	4	0.6
Intellectual property protection	9	0.3	3	0.5
Investment measure	113	4.3	84	13.1
Local content requirement	64	2.4	4	0.6
Migration measure	102	3.9	58	9.1
Other service sector measure	39	1.5	8	1.3
Public procurement	75	2.9	3	0.5
Sanitary and phytosanitary measure	26	1	7	1.1
State-controlled company	29	1.1	2	0.3
State trading enterprise	7	0.3	1	0.2
Sub-national government measure	7	0.3	0	0
Technical barrier to trade	28	1.1	17	2.7
Trade finance	35	1.3	0	0
	1025	39.2	195	30.6
Traditional measures				
Competitive devaluation	6	0.2	0	0
Export subsidy	70	2.7	3	0.5
Export taxes or restriction	141	5.4	43	6.7
Import ban	60	2.3	7	1.1
Import subsidy	10	0.4	4	0.6
Quota (including tariff rate quotas)	39	1.5	11	1.7
Tariff measure	309	11.8	330	51.6
Trade defence measure	769	29.4	20	3.1
	1404	53.7	418	65.3
Non-tariff barrier	184	7	26	4.1
Total	2613	99.9	639	100

Note: In the first (third) column, the table reports the number of trade-restrictive (trade-liberalising) measures in the GTA dataset for each trade measure category. The second (fourth) column reports the share of trade-related measures from a specific trade measure category in the total number of newly implemented trade-restrictive or liberalising measures.

Table 2: Trade-Restrictive Measures by Implementing G20 Economy and Affected Country Group

Implementing country	**Affected country group**						
		Full sample		**G20**		**Non-G20**	
	Total measures	Countries affected	Avg. per country	Countries affected	Avg. per country	Countries affected	Avg. per country
Argentina	212	51	33.5	17	53.6	34	23.5
Australia	33	45	3.4	18	4.3	27	2.7
Brazil	192	52	30.6	18	40.9	34	25.1
Canada	53	50	3.9	17	7.4	33	2.2
China	121	51	20.6	17	28.3	34	16.7
EU	87	36	13.3	14	19.7	22	9.3
France	12	50	10.9	18	15.1	32	8.5
Germany	20	44	10.3	17	14.6	27	7.6
India	160	51	25.5	17	34.4	34	21.1
Indonesia	77	52	16.1	18	21.7	34	13.2
Italy	12	50	6.9	17	11.4	33	4.7
Japan	24	50	4.5	17	6.6	33	3.4
Mexico	39	50	1.5	17	3.0	33	0.8
Republic of Korea	34	50	6.7	17	9.3	33	5.3
Russian Feder.	265	51	46.6	17	69.2	34	35.3
South Africa	54	49	4.2	17	6.9	32	2.7
Turkey	42	46	4.0	17	6.8	29	2.3
United Kingdom	18	49	7.6	17	12.7	32	4.8
United States	62	50	4.7	17	7.4	33	3.3

Source: Authors' calculations based on GTA data.
Note: Column (1) reports for each G20 economy the number of trade-restrictive measures implemented vis-à-vis all trading partners included in the regression sample, that is, vis-à-vis all trading partners where information on business cycle variables is available. The number of affected trading partners is reported in column (2). Column (3) shows the average number of trade-restrictive measures by which each of the affected countries was affected. Columns (4) to (7) report the corresponding numbers for the sample splits by affected country groups.

Table 3: Number of Trade-Related Measures Per Measure Category Considered in the Data Sample used in the Empirical Analysis

Measure Category	**Trade Restrictive**	**% of Total**	**Trade Liberalising**	**% of Total**
"Murky" measures				
Bail out / state aid measure	1646	12.7	417	5.7
Consumption subsidy	169	1.3	54	0.7
Intellectual property protection	39	0.3	0	0.0
Investment measure	119	0.9	31	0.4
Local content requirement	402	3.1	34	0.5
Migration measure	291	2.3	122	1.7
Other service sector measure	37	0.3	27	0.4
Public procurement	328	2.5	44	0.6
Sanitary and Phytosantiary Measure	61	0.5	4	0.1
State-controlled company	97	0.8	0	0.0
Technical Barrier to Trade	181	1.4	123	1.7
Trade finance	380	2.9	99	1.4
	3750	29.0	955	13.0
Traditional measures				
Export subsidy	922	7.1	98	1.3
Export taxes or restriction	967	7.5	449	6.1
Import ban	94	0.7	24	0.3
Import subsidy	105	0.8	36	0.5
Quota (including tariff rate quotas)	416	3.2	243	3.3
Tariff measure	3580	27.7	3793	51.8
Trade defence measure	1502	11.6	141	1.9
	7586	58.7	4784	65.3
Non-tariff barrier	1582	12.2	1582	21.6
Total	12,918		7,321	

Note: In the first (third) column, the table reports the number of trade-restrictive (trade-liberalising) measures considered in the sample of the empirical analysis for each trade measure category. The second (fourth) column reports the share of trade-related measures from a specific trade measure category in the total number of newly implemented trade-restrictive or liberalising measures. Notice that the sum of trade-restrictive and liberalising measures exceeds the number of entries considered in the data sample of the empirical analysis as the GTA assigns some of the measures implemented to multiple trade measure categories. Notice also that we add the number of trade-liberalising measures that expire in quarter t to the number of trade-restrictive measures that are implemented in quarter t. While we deem it to be necessary to count expiring trade-liberalising as newly implemented trade restrictive measures in order to adequately reflect protectionist activity, it should be noticed that our empirical results presented in Section 4 do not hinge on this adjustment. The results are available on request.

Table 4: Descriptive Statistics: 2009Q1 to 2012Q2

	Full sample	G20 AE	G20 EME
Dependent Variable			
Newly Implemented Trade-Restrictive Measures	0.98	0.50	1.52
	(1.74)	(0.88)	(2.22)
Explanatory Variables			
Log-Difference of Real Bilateral exchange rate	0.45	0.19	0.74
	(6.04)	(5.78)	(6.30)
Domestic GDP growth	2.56	0.67	4.62
	(4.59)	(3.28)	(4.92)
GDP Growth of Affected Trading Partner	2.21	2.24	2.19
	(4.58)	(4.60)	(4.57)
Observations	12334	6412	5922

Source: Authors' calculations based on GTA data.
Note: Sample means. Standard deviations reported below in parentheses.

Table 5: Incidence Rate Ratio Estimates from Regressions by *Implementing* Country Groups (column (1)-(4)) and by *Affected* Country Groups (column (5)-(7))

Model	(1) Full sample (G20)	(2) G20 AEs	(3) G20 EMEs	(4) *Test statistic*	(5) G20 vs. G20	(6) G20 vs. non-G20	(7) *Test statistic*
L.Real Bilateral FX	1.010***	1.013***	1.008***	[2.78*]	1.007***	1.012***	[2.76*]
	(0.002)	(0.002)	(0.002)		(0.002)	(0.002)	
L.GDP	0.956***	0.887***	0.969***	[31.29***]	0.953***	0.956***	[0.09]
	(0.005)	(0.013)	(0.006)		(0.007)	(0.007)	
L.GDP Affected	1.000	0.996	1.001	[0.32]	0.995	1.002	[0.62]
	(0.004)	(0.007)	(0.005)		(0.007)	(0.005)	
Time fixed effects	Yes	Yes	Yes		Yes	Yes	
Country-pair FE	Yes	Yes	Yes		Yes	Yes	
Observations	12334	6412	5922		4466	7868	

Source: Authors' calculations based on GTA data.
Note: Incidence Rate Ratios (IRR). Standard errors reported below in parentheses. Chi-square test statistics on significant differences in coefficients in square brackets.
*Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Table 6: Incidence Rate Ratio Estimates from Regressions by Trade Policy Dimension

Model	(1) Murky	(2) Non-Murky	(3) Trade defence	(4) Tariff
L.Real Bilateral FX	1.016***	1.006***	1.032***	1.002
	(0.003)	(0.002)	(0.006)	(0.004)
L.GDP	0.972***	0.967***	0.931***	0.881***
	(0.009)	(0.006)	(0.013)	(0.009)
L.GDP Affected	1.003	0.995	1.025*	0.990
	(0.008)	(0.005)	(0.015)	(0.009)
Time fixed effects	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes
Observations	10024	10500	4102	7700

Source: Authors' calculations based on GTA data.
Note: Incidence Rate Ratios (IRR). Standard errors reported below in parentheses. Notice that in a negative binomial regression framework country-pair groups are dropped from the regression sample if the dependent variable takes the value of zero for all observations within this group. For this reason, the number of observations differs across the regression samples reported above.
*Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Table 7: Robustness Checks (I): Choice and Specification of Dependent and Explanatory Variables (column (1)-(4)) and Alternative Approaches to Address Reporting Lags (column (5)-(8))

Model	(1) Red - Green	(2) Including Initiations	(3) Unemp. rate for GDP	(4) Second lag	(5) Linear time trend	(6) Quadratic time trend	(7) Implement. country specific time trend	(8) Reporting lag 0
L.Real Bil. FX	1.010***	1.008***	1.009***	1.006***	1.011***	1.011***	1.004***	1.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
L.GDP	0.898***	0.972***		0.967***	0.980***	0.982***	0.964***	0.939***
	(0.005)	(0.005)		(0.005)	(0.004)	(0.004)	(0.005)	(0.007)
L.GDP Affected	0.994	0.998		1.002	1.014***	1.016***	0.999	0.998
	(0.005)	(0.004)		(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
L.Unemp. Rate			1.163***					
			(0.024)					
L.Unemp. Affe.			0.993					
			(0.014)					
Time trend					0.950***	0.920***		
					(0.003)	(0.017)		
Quadratic trend						1.002*		
						(0.001)		
Time FE	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12978	12404	9660	11180	12334	12334	12334	10612

Source: Authors' calculations based on GTA data.
Note: Incidence Rate Ratios (IRR). Standard errors reported below in parentheses.
*Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Table 8: Robustness Checks (II): Alternative Model Specification

Model	(1) Linear FE Model	(2) Pseudo Poisson ML Model	(3) Lagged Dependent	(4) Dynamic Panel (GMM)	(5) Stock of measures	(6) Heterosced. & Cluster robust	(7) Implementing and Affected separate FE
L.Real Bilat. FX	1.008***	1.065***	1.009***	1.009**	1.010***	1.009**	1.004***
	(0.001)	(0.011)	(0.001)	(0.004)	(0.002)	(0.004)	(0.002)
L.GDP	0.947***	0.762***	0.988**	0.951**	0.958***	0.948*	0.958***
	(0.009)	(0.017)	(0.005)	(0.024)	(0.005)	(0.026)	(0.007)
L.GDP Affected	0.999	0.996	1.001	1.020	0.999	1.001	1.000
	(0.004)	(0.018)	(0.004)	(0.022)	(0.004)	(0.003)	(0.004)
L.Dependent Var.			1.037***	0.972			
			(0.006)	(0.111)			
L.Stock Measures					0.994***		
					(0.002)		
Time trend				0.974			
				(0.018)			
Time FE	Yes	Yes	Yes	No	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Observations	12978	12334	12978	12978	12334	12978	12978

Source: Authors' calculations based on GTA data.
Note: Incidence Rate Rations (IRR). For comparability, IRRs are also reported for the linear regression models presented in columns (1) and (4). Standard errors reported below in parentheses. Based on the Hansen test of over-identifying restrictions, the null hypothesis that the instruments are satisfying the orthogonality conditions required for their employment cannot be rejected for the regression reported in column (4).
*Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*