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Fallacy of Floating? Reconsidering the ability of flexible exchange rates to offset terms-of-trade volatility in developing countries

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Abstract

This paper questions the traditionally accepted superiority of flexible exchange rate regimes in offsetting commodity price fluctuations. Employing an updated measure of the commodity terms-of-trade, a comparison of exchange rate regime classifications and more recent data than much of literature supporting this assertion, I find little evidence that flexible regimes deliver better outcomes in this regard. Although previous results are reproducible with the use of a certain terms-of-trade measure, their significance dissipates when employing an arguably more comprehensive measure. The results are similarly sensitive to the use of an alternative exchange rate regime classification. Notably, any significance found with either measure vanishes in the period after 2004, potentially indicative of a structural break in the transmission of terms-of-trade improvements to economic growth. In light of these findings, I suggest that, with updated measures and awareness of a potential structural break in hand, it may be time to re-evaluate the way in which terms-of-trade shocks have been understood to transmit in developing economies and what kind of exchange rate regime is best suited to this transmission mechanism.

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1. Introduction

Natural resource dependence in many developing countries prompts the key policy question of how to best use exchange rate policy to attenuate commodity price volatility. This paper revisits the evidence that underpins the view that flexible exchange rates better insulate developing countries from terms-of-trade volatility and investigates its robustness. Economic theory and a substantial part of the literature on exchange rate regimes emphasise the superiority of flexible regimes in dealing with terms-of-trade shocks in developing countries exposed to commodity price volatility. The literature goes back to Friedman (1953) and Mundell (1961), who favoured flexible regimes in the face of terms-of-trade shocks due to their 'shock absorber' property. Prominent proponents of flexible regimes ever since have been Edwards (2003), Levy-Yeyati and Sturzenegger (2003), Broda (2004), Céspedes and Velasco (2012) and well as IMF papers such as Adler and Sosa (2011). Using theory or empirical evidence, they have all argued that flexible regimes more effectively insulate the economy from terms-of-trade volatility. The central idea of this literature is that countries primarily exposed to real shocks (which terms-of-trade shocks are categorised as) should pursue flexible regimes, as these allow for natural adjustment to shocks through the exchange rate. Fixed regimes, on the other hand, force output volatility if prices are sticky and the nominal exchange rate cannot change. This is accompanied in practice by the risk of a speculative attack on an overvalued currency and the danger of depleting foreign reserves. The bipolar model of exchange rate regime choice is a topic of great controversy, however, with questions on the other side around the suitability of flexible regimes for developing countries. The theoretical predictions depend crucially on the categorisation of a terms-of-trade shock as a *real* (current account) rather than *asset* (capital account) shock, as in the case of the latter the balance can shift in favour of fixed regimes.

I revisit the received wisdom and ask whether the central argument holds robustly. Relative to previous literature that has been used to justify the recommendation of flexible rate regimes, I use updated (and arguably improved) measures of the commodity terms-of-trade, multiple classifications of exchange rate regimes, and more recent data. My analysis suggests that the superiority of flexible regimes holds up in some circumstances but is less robust than posited by theoretical predictions and the existing literature.

All views expressed in this paper are those of the author and do not represent the views of the Research department of J.P. Morgan Securities plc ("JPMS") or the views of JPMorgan Chase or any of its affiliates.

I begin by setting up the theoretical framework (the Mundell-Fleming model) that illustrates the transmission of commodity terms-of-trade movements through the economy; I then summarise its predictions on the relative performance of fixed and flexible regimes in the face of shocks. I run an OLS specification on panel data spanning a period from 1974 to 2019 to show that the relationship upheld by the literature - i.e., that fixed exchange rate regimes amplify terms-of-trade movements - is highly sensitive to the measurement of terms of trade and to the classification of countries' exchange rate regimes. Even then, the significance of the relevant coefficient does not seem to hold after 2004. I conduct robustness checks that consider the question using a dynamic panel model and include a panel VAR to complement my findings with impulse responses, generating them for the different terms-of-trade measures in order to verify whether these are in line with the main results. These methods support my main findings in questioning the superiority of flexible exchange rate regimes. Finally, I discuss possible reasons behind the disparity in results across measures and across time.

Although the theoretical arguments for different exchange rate regimes have been extensively spelled out, recent empirical literature is surprisingly sparse. Broda (2004) considers whether responses of real GDP, real exchange rate and prices to terms-of-trade shocks differ systematically across exchange rate regimes using panel VARs. Edwards and Levy-Yeyati (2003) attempt to answer a similar question using Feasible Generalised Least Squares and Generalised Method of Moments (GMM). Céspedes and Velasco (2012) investigate whether the impact of commodity price shocks on output differs across regimes and levels of financial market development, using interaction terms in Weighted Least Squares estimation. Adler and Sosa (2011) explore to what extent vulnerability to commodity price shocks is determined by flexibility and quality of the policy framework; they embark on cross-sectional and panel approaches to assess the amplifying role of a variety of country fundamentals. Towbin and Weber (2013) compare the transmission of external shocks onto domestic output under alternative regimes using panel VARs and allow the responses to vary with foreign currency debt and import structure. All papers above use real GDP growth as the outcome variable (although some consider it relative to its trend) and conclude that more flexible regimes are associated with a smaller response of output after terms-of-trade shocks - in other words, that flexible regimes reduce volatility caused by terms-of-trade shocks. Importantly, Adler and Sosa (2011) highlight that exchange rate flexibility appears to "lose power as a shock absorber in the presence of high financial dollarisation" and Towbin and Weber (2013) find the same at high external debt levels.

2. Theoretical framework

Before delving into the best defence against terms-of-trade shocks, it is important to understand the mechanisms of the propagation of these shocks in the economy. The most appropriate theoretical framework for studying the pass-through of terms-of-trade fluctuations onto output is the Mundell-Fleming three-equation model that incorporates the effect of real and asset market shocks on the output gap. The Mundell-Fleming shows that under sticky prices, fixed exchange rate regimes exacerbate the effect of real shocks (e.g. terms-of-trade shocks) on the output gap, but insulate the effects of monetary and asset shocks on the output gap. This model features a goods market equilibrium (the IS curve) with imported goods, a money market equilibrium (the LM curve), and the balance of payments (the BB curve) with capital inflows.

IS

$$y_t = \bar{y} + \delta(e_t + p_t^* - p_t - \bar{q}) + \varepsilon_t^y \tag{2.1}$$

where y_t is output and $\bar{y}$ is equilibrium output, $q_t = e_t + p_t^* - p_t$ is the real exchange rate (RER), and $\bar{q}$ the equilibrium RER, e_t is the nominal exchange rate, p_t is the goods price and ε_t^y a real shock.

LM

$$m_t - p_t = \varphi y_t - \beta E_t i_{t+1} + \varepsilon_t^m \tag{2.2}$$

where $m_t - p_t$ is the log of real money balances, which is increasing in output y_t and decreasing in $E_t i_{t+1}$, the opportunity cost of holding money (the expected interest rate). In this version of the model, ε_t^m is an asset price shock.

BB (uncovered interest parity)

$$E_t \Delta e_{t+1} = E_t(i_{t+1} - i_t^*) \tag{2.3}$$

where $E_t \Delta e_{t+1}$ is the expected change in the exchange rate and is related to the expected differential between domestic and world interest rates. Prices are assumed to be sticky and adjust only gradually, while asset prices, interest rate and the nominal exchange rate can adjust instantaneously (Agénor & Montiel, 2015).

When a real shock (ε_t^y) hits, a fixed nominal exchange rate prevents e_t from moving to offset the shock, while prices are sticky so also cannot adjust in the short run. This implies that the shock translates into output volatility as the output gap will have to move with the shock. In contrast, flexible exchange rate regimes provide better insulation as e_t can jump to offset the shock and stabilise the output. Terms-of-trade shocks have been traditionally considered to fall under *real* shocks, which would suggest that flexible exchange rate regimes are a better insulator of such shocks. A negative terms-of-trade shock is then expected to "induce RER depreciation under flexible exchange rates but recession under fixed rates" (Agénor & Montiel, 2015). In the case of a negative asset price shock, adjustment occurs through a cut in the interest rate, and a fixed exchange rate blocks transmission to the real economy. Under a flexible regime, a cut in the interest rate leads to a depreciation of the nominal exchange rate, which pushes down output, so the shock in the asset market is transmitted into the real economy. If the financialisation of the commodity markets has led to commodity price shocks transmitting as asset shocks more often, rather than real shocks, as considered by the structural models, one can argue for the superiority of fixed regimes in insulating such shocks.

In the literature, the "textbook proposition" that supports flexible regimes is that they act as "automatic stabilisers" for terms-of-trade shocks, allowing for smooth adjustment (Broda, 2004; Frankel, 2012). Under flexible regimes, smooth adjustment to a negative shock can be achieved through a nominal exchange rate depreciation (fall). Allowing the depreciation in response to a fall in demand reduces the perceived price of the tradable good to foreign buyers, partially offsetting the effects of a real shock (Broda, 2004; van der Ploeg, 2019).

Under fixed regimes, adjustment can happen through domestic prices - if the price to a foreign buyer is a function of the domestic price and the nominal exchange rate, then the only way to make it cheaper, and thus extract demand from foreign buyers, is to lower domestic prices. In the case of a positive terms-of-trade movement, this pushes up prices and thus inflation. However, as prices are sticky, adjustment has to happen through output. Countries with pegs would need to accept "a higher degree of output and unemployment volatility" than would be required under flexible regimes (Rafiq, 2011). Intricacies matter here, however – a flexible regime may be needed for a country "with a relatively large nontradable sector", while fixed regimes may be better suited to countries with a relatively small one (Sinnott et al., 2010).

Nonetheless, support for flexible exchange rates is not universal. Flexible exchange rate regimes can aggravate balance of payments problems following a commodity price fall and the subsequent depreciation of the exporter's currency if there are large dollar-denominated debt holdings. This 'fear of floating' is prevalent across emerging market economies that hold high levels of foreign currency debt (Calvo & Reinhart, 2000). A number of authors have pointed out that the appeal of flexible exchange rates does not apply in such countries and may even be "problematic" in face of negative commodity price shocks (Eichengreen et al., 2003). If a commodity price shock leads to currency depreciation and debt is denominated in dollars, the value of the debt burden can skyrocket to the point of becoming unsustainable. Another advantage of fixed exchange rates is the credibility associated with it which may induce less volatility. Although this applies to the greatest extent to fully dollarised economies, fixed exchange rates can allow countries to credibly commit to a pursuance of low and stable inflation, using an anchor currency that successfully pursues these goals.

However, a peg short of dollarisation that is easy to undo is vulnerable to speculative attacks on the currency (van der Ploeg, 2019). This is especially relevant in context of a negative terms-of-trade shock, where the central bank would need to step in by selling foreign reserves in order to buy the home currency to sustain the peg, leading to rapid depletion and the possibility of such a speculative attack (van der Ploeg, 2019). The 1994 Mexican and 2001-02 Argentinian crises are considered prime examples of a pegged but overvalued currency that resulted in a "devastating currency crisis" (Drechsel et al., 2019). A fixed rate is also necessarily contractionary under these conditions, by inducing "an additional fall in employment" (Broda, 2004). Dollarisation, or any peg, also does not allow countries to respond optimally to idiosyncratic shocks within their own economy using countercyclical monetary policy; instead, they have to rely on shocks hitting the anchor country in the same way. In the absence of co-movement with the anchor, the country is subordinate to the anchor country's monetary policy. This is especially problematic in the case of terms-of-trade shocks, as natural resource commodity-exporting economies' terms-of-trade differ significantly from those of the US or Eurozone. For this reason, Sinnott et al (2010) call for a flexible regime for countries that "experience real shocks that are asymmetric to their main trading partners" (note here again the focus on *real* shocks). Particularly in the case of an oil price spike, an oil importer and exporter have to respond differently, as evidenced by the oil price shocks of the 1970s.

3. Data

This paper takes advantage of the new IMF country-specific commodity terms-of-trade index database, released in 2019, and runs the analysis on two alternative calculations of the terms-of-trade. There are two main alternative weighting mechanisms: net commodity exports weighted by each commodity's *share of GDP* and as a *share of total commodity trade*. For the remainder of the paper, I will refer to the former measure as *tot_gdp* and to the latter as *tot_trade*. I will also use terms-of-trade to refer to the *commodity* terms-of-trade. Weighting by overall commodity trade rather than output "is similar to what is done in standard price indices" and closer to what is used in older literature (Gruss & Kebhaj, 2019). Weights are computed using three-year rolling averages of "trade values and output to smooth fluctuations" and thus "predetermined to price fluctuations" (ibid, 2019). This ensures that a change in the index reflects international prices rather than "endogenous changes" in trade volumes (ibid, 2019). It thus addresses an important concern that countries may increase output as a result of rising prices, at least in the short run.

As pointed out in the IMF paper that introduces the index, the *tot_trade* approach fails to account for the differing importance of commodities across countries. I judge *tot_gdp* to be the superior measure to both *tot_trade* and an unweighted terms-of-trade (exports over imports) measure, as it better accounts for the commodity dependence of developing countries: two countries may see a similar drop in a commodity price but experience "very different windfall gains or losses" as the importance of commodities to GDP is larger (ibid, 2019). The paper authored by the creators of the index presents an example of the increase in *tot_trade* in 2001-7 being larger in Argentina than Indonesia, but that the measure does not reveal that the "associated windfall gains were somewhat smaller" due to Argentina's larger commodity dependence, which is captured in *tot_gdp* (ibid, 2019).

Not attaching weights to their terms-of-trade measures may be a shortcoming of much of the relevant literature - for example, Broda (2004) uses the net barter terms-of-trade (WDI), while Edwards and Levy-Yeyati (2003) use the relative price of exports to imports. As I will show later in the paper, the results are very sensitive to the measure employed, and using what I argue to be the superior measure wipes out much of the significance from earlier studies. Adler and Sosa (2011) use a measure similar in spirit that weights the terms-of-trade changes by the previous-year ratios of exports and imports to GDP, but does not consider the weights of individual commodities. Furthermore, using the previous-year ratios does not deal as well as using the three-year rolling average with the concern around the endogeneity of the country responding to a positive commodity cycle by increasing output. The *tot_gdp* measure is thus potentially more insightful than the measures used by above-mentioned literature and than *tot_trade* itself. It is also the preferred measure that Gruss and Kebhaj (2019) (the researchers who constructed both this index and the alternative tot_trade measure) use in their own analysis. Exogeneity of terms-of-trade is corroborated by evidence of the index' creators as well as Broda (2004) and others, and will be discussed in the next section.

In order to evaluate the suitability of one regime over the other, it is first necessary to classify the behaviour of central banks into exchange rate regimes. Although the IMF has been publishing the yearly AREAER report since 1950, it is frequently criticised for providing only a *de jure* (self-reporting by governments), rather than *de facto*, classification of monetary policy and exchange rate frameworks. Especially in developing countries, these have shown substantial discrepancy. The IMF acted upon this criticism when it began to report on de facto arrangements, but this change only took place in 2001, which led me to consider other prominent exchange rate classifications. My main classification of reference is Reinhart and Rogoff (RR), which is useful due to data availability, as it has been updated up to 2019 (Reinhart & Rogoff, 2002). It is also the most commonly used classification in the exchange rate literature. My secondary classification is Levy-Yeyati and Sturzenegger (LYS) which is only updated up to 2014 (Levy-Yeyati & Sturzenegger, 2003). I compare my results up to 2014 in order to test the sensitivity of analysis is to the exchange rate regime classification, which is striking.

RR crucially includes data on parallel and multiple exchange rate markets, pointing out that a classification that does not differentiate between clean pegs and pegs accompanied by black markets is "fundamentally flawed" (Reinhart & Rogoff, 2002). It considers market-determined, rather than official, exchange rates. LYS assign regimes using data that includes interest rates and reserve movements, in order to differentiate between lack of movement of the nominal exchange rate due to policy from the lack thereof due to calm economic conditions (Klein & Shambaugh, 2010). Bleaney and Francisco (2007) find considerable differences in classification between the two, in their sample covering developing countries between 1985-2000: LYS defines 45.7% of observations as floats, while RR define 28.2% as such (ibid, 2007). An alternative classification by Shambaugh (2004) puts 71% into the floating basket. LYS features a significant increase in the number of floating regimes with time, while RR does not; this may be due to the fact that multiple exchange rates and pegs were previously considered floating in RR, and once they actually became floating, the classification under RR did not change (Francisco & Bleaney, 2007). The general variation mostly lies in the decisions on the stringency of a "fixed" definition (i.e. "how wide a range of fluctuation to accept" before classifying a regime as floating) (Francisco & Bleaney, 2007). The authors note that regimes change more frequently with LYS, which is likely due to the higher year-to-year variation "measured volatility and reserves and interest rates" compared to measures which other classificatons focus on (Francisco & Bleaney, 2007). In my analysis, I use a dummy variable that equals to 1 if the regime corresponds to one of the categories relating to a fixed exchange rate regime, and 0 otherwise. RR set up six main categories, where I code categories 1 and 6 as fixed. This includes no separate legal tender, pre-announced pegs or currency board arrangements, de facto pegs and *pegs accompanied by parallel markets*. I judge the incorporation of the latter into fixed to be appropriate as it captures the flawed pegs of the last century. It does not include crawling pegs and bands. LYS divides regimes into three categories: fixed, intermediate and floating, and I code the first as fixed.

Real GDP (GDP at constant 2015 prices, expressed in U.S. dollars), population, inflation (consumer prices, annual percentage change), external debt (stocks as percentage of GNI) data are taken from the World Bank's published data sources (World Bank, 2021). Capital account openness is provided by Chinn and Ito's (2008) classification.

4. Empirical strategy

In this section, I show that the results that underpin the conventional policy recommendation are *reproducible* but fragile upon scrutiny. I document that the central findings are not robust to alternative measures of terms-of-trade or exchange rate regimes classifications, or to the inclusion of data from more recent years. I begin by laying out my main empirical strategy, which splits the sample into two periods, before attempting such a strategy using different measures of terms-of-trade and exchange rate regime classifications.

If the underlying statistical relationship were robust, it would hold regardless of the terms-of-trade measure used and across the reputable exchange rate classifications. Sensitivity to these tweaks would suggest that the relationship may be at best only narrowly applicable, and at worst spurious.

I run a simple OLS model on panel data to identify whether the association between commodity terms-of-trade growth and output growth is differentially larger under fixed regimes, as much of the literature posits. The model therefore needs to regress the growth rate of real GDP per capita on the change in the commodity terms-of-trade price index, the exchange rate regime, an interaction between exchange rate regime and commodity terms-of-trade index change and country and year fixed effects.

$$\Delta y_{it} = \alpha + \beta_1 \Delta tot_{it} + \beta_2 fixed_{i,t-1} + \beta_3 \Delta tot_{it} * fixed_{i,t-1} + \gamma_i + \eta_t + u_{it} \tag{4.1}$$

where y_{it} is the log real GDP per capita (which will be referred to as *GDP* for the rest of paper) of country i at time t, tot_{it} is the log commodity terms-of-trade price index of country i at time t, $fixed_{i,t-1}$ is a dummy that equals 1 when the exchange rate regime in the previous period was fixed and 0 otherwise, γ_i are the country dummies and η_t are the year dummies which act as fixed effects. β_1 will be interpreted as the association between a change in terms-of-trade and a change in GDP under a flexible exchange rate regime, while $\beta_1 + \beta_3$ will be the association between a change in terms-of-trade and a change in GDP under a fixed exchange rate regime. A positive β_3 (given a positive β_1) indicates that fixed exchange rate regimes amplify the transmission of terms-of-trade changes to GDP. The GDP growth (Δy_{it}) denotes the log difference between GDP at time t and $t-1$ ($y_{it} - y_{i,t-1}$). rr_fixed will be used to denote the fixed exchange rate regime dummy according to the RR classification, while lys_fixed refers to the LYS classification.

The crucial identifying assumption throughout the paper is that terms-of-trade are exogenous to the country, which follows the small open economy (SOE) model of a developing commodity-exporting country. This requires that the commodity exporter is a price taker and has no influence on world commodity prices and consequently its terms-of-trade. This exogeneity assumption is tested by Broda (2004) and accepted for his sample of developing countries, which excludes oil exporters (which could violate the assumption) and countries with populations below one million (which are not large enough to threaten the assumption). He considers all goods "for which the export share of any developing country exceeds 15 per cent of world export of that good" and finds that "only 22 goods from 9 countries out of a sample of 1000 goods and 75 countries" violate the price-taking assumption (ibid, 2004). He concludes that the price-taking small-open economy assumption is a "good approximation for most of the countries involved", but rejects the hypothesis of exogenous terms-of-trade for developed and oil-exporting countries (ibid, 2004). I therefore also restrict my sample to exclude oil exporters, as he does. I use clustered standard errors (at the country level) in order to correct for heteroskedasticity and autocorrelation within countries across time.

I include both year and country fixed effects (introduced as dummy variables); simply running OLS without them may induce bias as they would be captured in the composite error term.

The former would pick up geographical or structural features of a country (such as its natural resources, which determine its exports), while the latter would pick up the global business cycle, which may be correlated with the terms-of-trade changes. Year fixed effects are crucial for the exogeneity assumption as global conditions are the driver of commodity prices in a SOE. Especially in this century, there has been considerable negative correlation between world economic conditions and commodity prices (Drechsel et al., 2019). The lack of inclusion of fixed effects is a shortcoming of Edwards and Levy-Yeyati (2010) as it may lead to an overestimation of the effect of terms-of-trade growth on GDP growth. Note that country and year fixed effects dummies will be excluded from all my regression tables but are run in the regressions.

If terms-of-trade are exogenous, then, the inclusion of further controls may be problematic as they are likely to be bad controls. For example, government expenditure and external debt are likely bad controls as a positive terms-of-trade shock can also affect GDP growth through higher government expenditure as a result of the windfall – the voracity effect, or through a declining external debt burden, which in turn both affect GDP (Tornell & Lane, 1999). Capital account openness may itself also be a bad control as the transmission of terms-of-trade to GDP can work through a more open capital account. These would constitute bad controls (*"overcontrol bias"*) as we would "block the total effect" of terms-of-trade on GDP, which is the "*very effect* we want to estimate" (Cinelli et al., 2020). On the other hand, they may affect the choice of exchange rate regimes and also output growth and be confounders to the exchange rate dummy variable. For this reason, I include them in my robustness checks while aware that they are likely bad controls to the terms-of-trade variable. Albeit unnecessary due to terms-of-trade exogeneity, the inclusion of the lagged level of real GDP per capita would act as a neutral control that could possibly increase precision, as it is uncorrelated with the terms-of-trade but correlated with the outcome. This prompts the use of IV or GMM due to the arising endogeneity, and will be discussed as part of my robustness checks. The exogeneity of terms-of-trade also serves to identify the panel VAR discussed later in this paper.

A further concern is the potential endogeneity of the exchange rate regime: one could imagine a terms-of-trade shock that puts so much pressure on a pegged currency that the country runs out of reserves to defend it and engenders the abandonment of the peg, or the country switches the regime for another related reason. To account for this, I use the fixed regime dummy as a lagged variable, i.e. the regime that was in place the year before. In my robustness checks, I also restrict the sample to observations where the same regime has been in place for several years. A worry remains if countries with higher or lower growth rates are more or less likely to peg their exchange rates, and if this is not accounted for by the country fixed effects. The assumption of no multicollinearity is satisfied, as each explanatory variable changes over time for at least some i – both exchange rate classifications are observed in the data, even if it does not vary for some countries.

It is important to note that GDP is non-stationary, as GDP series are highly persistent. I test for unit root using the Im-Pesaran-Shin test where H_0: all panels contain unit roots and H_1: some panels are stationary. I cannot reject the null for log real GDP but I can reject it at the 1% significance level for its first difference. This indicates that a model regressing the GDP growth rate, i.e. the log difference of GDP, is more appropriate. Otherwise, since T is large, we may be running spurious regressions if we face unit roots, so running it in differences has the advantage of turning the I(1) into weakly dependent series.

5. Results

Throughout my analysis, I split the sample into the whole 1974-2019 period (cols. 1, 4, 7), as well as pre-2004 (cols. 2, 5, 8) and post-2004 (cols. 3, 6, 9). There appears to exist a breakdown in any magnifying effect of fixed regimes that was seen before 2004 using the combination of *tot_trade* measure and RR classification. I test for a structural break in the relationship between log real GDP and log terms of trade using the Chow test and find a statistically significant structural break in 2004. The initial split was inspired by the time at which some of the influential papers (such as Broda, 2004 and Edwards & Levy Yeyati, 2003) were written in order to investigate if their results still hold in the period after their publishing. It also mirrors, however, the commodity co-movement theory which has been hypothesised to have emerged around that time period (Drechsel et al., 2019).

Table 7.1 presents the results of my main specification on the sample of all developing countries that are not major oil exporters. The first three columns use the RR classification and the *tot_trade* measure, in order to reproduce the relationship upheld by the literature. The middle three columns use the same classification (RR) and the potentially superior terms-of-trade measure *tot_gdp*. The last three columns use the *tot_trade* measure in order to see if the pre-2004 significance is robust to the use of an alternative classification (LYS). I repeatedly refer back to the *tot_trade* in order to highlight that the results are fragile to both terms-of-trade measure and regime classification.

Table 5.1: Main specification (OLS) split into time periods, alternative measures of commodity terms-of-trade and exchange rate regime classifications

Δy_{it}	(1) all	(2) pre-2004	(3) post-2004	(4) all	(5) pre-2004	(6) post-2004	(7) all	(8) pre-2004	(9) post-2004
Δtot_trade_{it}	0.0129	-0.00849	0.0364*				0.0242*	0.0230	0.0196
	(0.94)	(-0.38)	(2.10)				(2.14)	(1.59)	(0.84)
$rr\_fixed_{i,t-1}$	0.00463	0.000174	0.0123	0.00441	-0.000161	0.0121			
	(1.20)	(0.03)	(1.89)	(1.13)	(-0.03)	(1.87)			
$\Delta tot\_trade_{it} * rr\_fixed_{i,t-1}$	0.0235	0.0540*	-0.0105						
	(1.45)	(2.27)	(-0.51)						
Δtot_gdp_{it}				0.139**	0.0868	0.166*			
				(2.94)	(1.46)	(2.14)			
$\Delta tot\_gdp_{it} * rr\_fixed_{i,t-1}$				-0.0389	0.0774	-0.126			
				(-0.57)	(0.96)	(-1.22)			
$lys\_fixed_{i,t-1}$							-0.00674	-0.00939	0.00800
							(-1.73)	(-1.94)	(1.08)
$\Delta tot\_trade_{it} * lys\_fixed_{i,t-1}$							-0.0132	-0.0160	-0.0120
							(-0.62)	(-0.60)	(-0.34)
N	4258	2533	1617	4258	2533	1617	3016	2010	914

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

I find the interaction term to be statistically significant in the period up to 2004 (col. 2), suggesting that fixed exchange rate regimes amplify transmission from terms-of-trade changes to GDP growth, only if I use the *tot_trade* measure. Using the RR classification and *tot_trade* measure, the results suggest that there is no significant association between Δtot_{it} and Δy_{it} under floating and intermediate regimes, but that the differential effect is significant and positive under fixed exchange rate regimes pre-2004. After 2004, however, there is no significant effect of Δtot_{it} under either regime.

Using the same regime classification but the arguably more sophisticated *tot_gdp* measure, the association between Δtot_{it} is insignificant under either regime pre-2004 (col.5). The post-2004 sample in fact shows an "insulating", albeit insignificant, effect of fixed regimes (col. 6). Even when we use the *tot_trade* measure that shows an amplifying effect of fixed regimes in column (2), but opt for the LYS regime classification, the results do not show such an effect (col. 8). Interestingly, this is the only combination that suggests that fixed exchange rate regimes are associated with a lower GDP growth rate (significant at the 10% level) pre-2004. The combinations that use RR (cols 3 & 6) show that fixed regimes are associated with higher GDP growth in the period after 2004 but not before. Consequently, while I find some evidence of an amplifying effect of fixed exchange rate regimes for commodity terms-of-trade movements at least in the period before 2004, the results do not hold across different measures of the terms-of-trade and classifications of the exchange rate regime. This indicates that the relationship is not as strong as frequently asserted by the literature.

The difference in magnitude between the results when each terms-of-trade measure is used are due to the measurement and calculation of the indices. The standard deviation of *tot_trade* is much higher than that of *tot_gdp*, as *tot_trade* better adjusts for the importance to GDP of terms-of-trade changes. The magnitudes of each effect are therefore different by construction, and attention should be paid to the significance rather than magnitude for fair comparison. An alternative here would be to reparametrise my variables and express the effects in terms of standard deviation changes.

To consider asymmetry, I split the sample into negative (col. 1-3) and positive (col. 4-6) terms-of-trade movements and find, even with the *tot_trade* measure, that the interaction on fixed regimes is only significant in the case of positive terms-of-trade growth, but not in a fall in the terms-of-trade index. This suggests that even if fixed regimes had an amplifying effect on output in the period before 2004 (using this measure), this was only the case for terms-of-trade growth, not in the case of (arguably more worrying) terms-of-trade falls. The lower sample size for each category becomes an issue, however, and it is strange that negative terms-of-trade changes are associated (even if insignificantly) with positive GDP growth.

Table 5.2: Main specification using *tot_trade* and RR, split into time periods and positive and negative terms-of-trade changes

Δy_{it}	if $\Delta tot_{it} < 0$			if $\Delta tot_{it} > 0$		
	all	pre-2004	post-2004	all	pre-2004	post-2004
Δtot_trade_{it}	0.0469	0.0199	0.0792	-0.0215	-0.0359	-0.0251
	(1.97)	(0.58)	(1.59)	(-0.40)	(-0.46)	(-0.77)
$rr\_fixed_{i,t-1}$	0.00258	-0.00254	0.00983	-0.00127	-0.00956	0.00272
	(0.45)	(-0.32)	(0.86)	(-0.21)	(-1.18)	(0.23)
$\Delta tot\_trade_{it} * rr\_fixed_{i,t-1}$	-0.0202	0.00402	-0.0902	0.0804	0.161*	0.0200
	(-0.59)	(0.08)	(-1.60)	(1.72)	(2.07)	(0.62)
N	2342	1398	870	1916	1135	747

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6. Robustness checks and extensions

6.1 Limitations

It is important to consider of the limitations of my analysis. Concerns remain around the potential *endogeneity of exchange rate regime* that is insufficiently accounted for in the analysis. Another concern would be that the years of slow growth under a flexible regime that come after renouncing a peg, in the face of an adverse shock, could be seen as the negative effect of a flexible regime, when in fact this is the result of the crisis that made the peg unsustainable. Additionally, if the terms-of-trade exogeneity assumption were disproven, this would render my identification strategy (as well as much of the literature's) invalid.

The *lack of higher-frequency data* (i.e. monthly or at least quarterly) in many developing countries constrains our ability to analyse shorter-term fluctuations, and track the channels of transmission and path of impulse responses more accurately. For example, in the VARs below, we only see the responses a year into the shock, but the most important time is arguably the few months after. Higher-frequency data is especially important in a world where the salience of commodities and FX as financial instruments potentially prevails over structural factors.

Changing commodity production patterns in the long term are also a potential threat to my analysis. While the *tot_gdp* measure provides a rolling average that accounts for the possibility that a country would produce more of a commodity at the expense of others in response to higher prices in that commodity, it does not account for more long-term adjustments as the rolling average only covers the previous three years. If the terms of trade weights change in a way that is correlated with the error term, such as a shift in export policy (think export-led growth versus import substitution), we would worry about bias in the coefficient on terms-of-trade.

The focus in this paper have been *changes* in the terms-of-trade index and GDP growth, in order to see whether the association between the GDP growth rate and terms-of-trade growth is stronger under either regime. Other ways this could be looked at is terms-of-trade *shocks* or other measures of volatility (such as the variance over several months or years) which may be more insightful. The supplementary VAR section below exhibits impulse responses of GDP growth to a 10% shock in the terms-of-trade.

6.2 Endogeneity or misclassification of exchange rate regime

To address fears that countries may switch exchange rate regimes as a result of a terms-of-trade-related shock, or that the years in the aftermath of a change are inaccurately classified, I only include observations where the exchange rate regime has been the same in the previous year or previous three.

To account for any changes in previous years, I run alternative regressions using the contemporaneous regime dummy but only include observations where the fixed regime has been in place for at least the last four years (see Appendix Table 11.1) and, less restrictively, at least the previous two years (see Addendum Table 12.1). The results for both terms-of-trade measures and exchange rate regimes still hold up well when the fixed regime has been in place for at least two years, but the significance on the interaction term using tot_trade and RR, which was the combination that previously showed a significant coefficient pre-2004, is in fact slightly weaker using the restriction of at least four years (see Appendix Table 11.1).

A limitation relating to the endogeneity of the regime still remains, however: while I take care of the possibility of a country endogenously switching regime in response to a shock, I may not be sufficiently dealing with the risk that there are confounders affecting both the GDP growth rate and the choice to engage with a particular regime. Country fixed effects may satisfactorily solve this, but there is the possibility that countries that have fixed regimes have slower growth for another reason we do not account for, leading to biased estimates. We can include further controls that might affect exchange rate regime choice, but it is a difficult balancing act as these may prove bad controls for the terms-of-trade growth. I include them for completeness, however, and choose controls in line with other papers (see Appendix Table 11.2). Capital account openness, the external debt to GNI ratio and the inflation rate are time-variant factors that may affect both the outcome and the regime choice. For example, a country with runaway inflation may choose to dollarise in order to create a foreign anchor for its prices, while a commodity-dependent country with high levels of external debt might choose a fixed regime to protect the value of its debt burden from exchange rate depreciation following a negative terms-of-trade shock. The first two are significant coefficients in a random effects logit analysis of the determinants of exchange rate regime choice in an IMF paper (Al-Sadiq et al., 2021). Strikingly, upon their inclusion, even the previously significant interaction term in column (2) becomes insignificant. The coefficients on Δtot_trade (col. 3) and Δtot_gdp (col.6) also lose significance, although here we worry especially about the bad control for the terms-of-trade introducing bias as explained in the empirical strategy section.

6.3 Anderson-Hsiao IV for dynamic panel

For robustness, I control for the lagged real GDP level. This should be a neutral control that increases precision as it affects the GDP growth rate but not the exogenous terms-of-trade. It is commonly done in the growth literature (Caselli et al., 1996):

$$\Delta y_{it} = y_{it} - y_{i,t-1} = \alpha + \beta_1 y_{i,t-1} + \beta_2 \Delta tot_{it} + \beta_3 fixed_{i,t-1} + \beta_4 \Delta tot_{it} * fixed_{i,t-1} + \gamma_i + \eta_t + \varepsilon_{it} \quad (6.1)$$

which is equivalent to

$$y_{it} = \alpha + \varphi_1 y_{i,t-1} + \beta_2 \Delta tot_{it} + \beta_3 fixed_{i,t-1} + \beta_4 \Delta tot_{it} * fixed_{i,t-1} + \gamma_i + \eta_t + \varepsilon_{it} \quad (6.2)$$

where $\varphi = 1 + \beta_1$.

Note that the error term is correlated with $y_{i,t-1}$, which violates strict exogeneity. FE estimation requires that $y_{i,t-1}$ is strictly exogenous, but it is positively correlated with $\varepsilon_{i,t-1}$ which can be seen if we lag the regression equation by one period. The OLS estimator suffers from a downward bias and inconsistency for fixed T, although there is a case for minimal bias for large T. If we are willing to accept this amount of bias, it is reasonable to run the model without an instrument.

The potential Nickell bias prompts me to apply an Anderson-Hsiao method that instruments $y_{i,t-1}$ with $y_{i,t-2}$. We could choose either the second-lagged level or difference of the lagged dependent variable as an instrument (Arellano, 1989). If lagged GDP is in levels, the difference can be a weak instrument in the face of the high persistence of GDP (Blundell & Bond, 1999) We should thus instrument using a further lag of our lagged GDP variable, so $y_{i,t-2}$ for $y_{i,t-1}$. The relevance condition holds as the level of GDP is highly correlated across time. For validity to hold, we need $y_{i,t-2}$ to have no partial effect on Δy_{it} once we have controlled for $y_{i,t-1}$, which is reasonable given that GDP from two years ago should only affect current GDP growth through GDP last year. We also need u_{it} to be serialy uncorrelated. This is tested using the Arellano and Bond *m1* test, where the null hypothesis is rejected, and the *m2* test, where I fail to reject the null hypothesis.

The IV estimator is consistent but not efficient as it does not make use of all moment conditions available, but given the high T nature of the data, GMM may not be appropriate (Arellano, 1989). I consider using system GMM, which Blundell and Bond (1999) propose to deal with a non-stationary series. They deem it superior to difference GMM as the latter "breaks down" from weak instrument problems if the data is highly persistent (Kruiniger, 2009). However, the number of instruments in GMM "explodes as T grows large" (Roodman, 2007). We would then run into the "quadratic-in-T instrument proliferation in dynamic panels", which leads to considerable bias in high T samples. While we could collapse the instrument in order to keep the instrument count lower than the rule-of-thumb of N, Anderson-Hsiao's single instrument method is more appropriate here while enjoying less restrictive assumptions than system GMM - it would be difficult to justify the assumption of $E(\Delta x_{it} a_i) = 0$, i.e. that the terms-of-trade change is uncorrelated with the fixed effects, as terms-of-trade changes are very likely to be correlated with year fixed effects. Appendix Table 11.4 shows the results, which exhibit a similar pattern to the main specification - the interaction term is significant at the 5% level when using the combination of *tot_trade* and RR (col. 2), but not using the other combinations.

6.4 VAR impulse responses

I run panel VARs to complement my main empirical strategy, in order to generate impulse response functions that confirm the direction of the main findings and show the channels thereof. VARs are said to be most effectively used "in *tandem* with other methods that help triangulate insights" (Adam & Wilson, 2021). I follow Towbin and Weber (2013) in their interacted panel VAR approach, making use of their *Interacted Panel VAR* Matlab toolbox (Weber & Towbin, 2011), while I adopt Broda's choice of endogenous variables - the real GDP, CPI and real exchange rate (RER). This provides useful insights into the channel of transmission of terms-of-trade shocks under different regimes, as per the theoretical framework. Lack of previous availability for the CPI and RER indices constrain the period to 1984-2018. I estimate a recursive VAR:

$$\begin{pmatrix} 1 & 0 & 0 & 0 \\ \alpha^{21}_{0,it} & 1 & 0 & 0 \\ \alpha^{31}_{0,it} & \alpha^{32}_{0,it} & 1 & 0 \\ \alpha^{41}_{0,it} & \alpha^{42}_{0,it} & \alpha^{43}_{0,it} & 1 \end{pmatrix} \begin{pmatrix} \Delta \ln tot_{it} \\ \Delta \ln y_{it} \\ \Delta \ln rer_{it} \\ \Delta \ln p_{it} \end{pmatrix} = \sum_{l=1}^{L} \begin{pmatrix} \alpha^{11}_{l,it} & 0 & 0 & 0 \\ \alpha^{21}_{l,it} & \alpha^{22}_{l,it} & \alpha^{23}_{l,it} & \alpha^{24}_{l,it} \\ \alpha^{31}_{l,it} & \alpha^{32}_{l,it} & \alpha^{33}_{l,it} & \alpha^{34}_{l,it} \\ \alpha^{41}_{l,it} & \alpha^{42}_{l,it} & \alpha^{43}_{l,it} & \alpha^{44}_{l,it} \end{pmatrix} \begin{pmatrix} \Delta \ln tot_{i,t-l} \\ \Delta \ln y_{i,t-l} \\ \Delta \ln rer_{i,t-l} \\ \Delta \ln p_{i,t-l} \end{pmatrix} + \gamma_i + \eta_t + \mathbf{u}_{it} \tag{6.3}$$

where $\alpha^{jk}_{l,it}$ are "deterministically varying coefficients" as a function of the exchange rate regime and time period, l is the number of lags, and γ_i and η_t are country and year fixed effects as per my main specification. The panel VAR is estimated using OLS equation-by-equation, allowing for country and year fixed effects and a Cholesky structure is imposed (Towbin & Weber, 2013). My core identifying assumption as in the rest of the paper is that terms of trade are not affected by the other three variables in the VAR. I therefore set α_{it}^{12}, α_{it}^{13} and α_{it}^{14} equal to zero. I then allow for interaction terms, following Towbin and Weber (2013), such that the coefficients in (8.4) are made up of:

$$\alpha_{l,it}^{jk} = \beta_{l,1}^{jk} + \beta_{l,2}^{jk} fixed_{it} + \beta_{l,3}^{jk} pre2004 + \beta_{l,4}^{jk} fixed_{it} * pre2004_{it} \tag{6.4}$$

where $fixed_{it}$ is the exchange rate dummy taking the value 1 if the regime is fixed and zero otherwise, and $pre2004_{it}$ is a dummy taking the value 1 if the observation lies before 2004. This allows me to generate impulse response functions under fixed and flexible exchange rate regimes before 2004 and under fixed and flexible regimes after 2004 to illustrate the heterogeneity of response. I also recreate this using the two different terms-of-trade measures and the two exchange rate regime classifications in order to follow my main discussion and show that the discrepancy in results between the different measures also persists using this methodology. I use bootstrapped standard errors (see Towbin and Weber, 2013) to address the concern that impulse responses are a non-linear function of the OLS estimate (Towbin & Weber, 2013). This allows for the computation of 90% confidence intervals.

Having performed a unit root test in my main specification, I find that GDP level suffers from a unit root. As before, I therefore take log differences. I perform a unit root test on the differences and reject the hypothesis of the unit root and hence choose to use that outcome, as in the main specification. I check the stability condition of the panel VAR and confirm that all eigenvalues lie inside the unit circle. I calculate selection-order statistics to identify the optimal moments and model lag order and find that the optimal lag order is two (Abrigo & Love, 2016). I test for Granger causality; I can reject the null hypothesis that a change in terms-of-trade and the RER does not Granger-cause real GDP growth, but cannot reject that for the effect of a change in CPI. I reject the null that terms-of-trade growth does not Granger-cause changes in the RER, but I cannot reject that terms-of-trade growth does not cause a change in CPI.

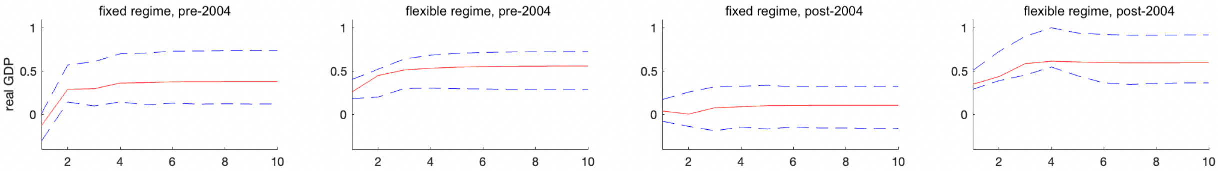

Figure 6.1: The real GDP impulse response to a 10% terms-of-trade shock under a fixed and flexible exchange rate regimes (RR) before and after 2004, using the *tot_gdp* measure. See addendum for response of real exchange rate and CPI.

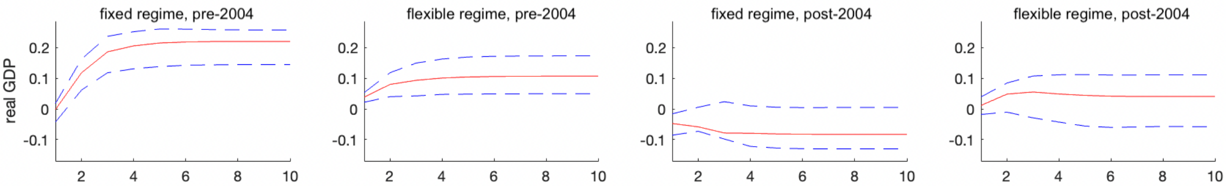

Figure 6.2: The real GDP impulse response to a 10% terms-of-trade shock under a fixed and flexible exchange rate regimes (RR) before and after 2004, using the *tot_trade* measure. See addendum for response of real exchange rate and CPI.

The impulse response functions suggest the following: using the *tot_gdp* measure and the RR classification, it appears that real GDP response is slightly larger under flexible regimes pre-2004, but that this difference becomes much more pronounced post-2004 - real GDP barely responds under fixed regimes in the latter period. We only get a higher real GDP response under fixed regimes when using the *tot_trade* measure in combination with the RR classification, as in the main specification, but responses appear small under both regimes post-2004.

7. Discussion

Whilst I do not deny that the assertion that fixed exchange rate regimes amplify terms-of-trade shocks holds under specific circumstances, the sensitivity of the results to the use of different terms-of-trade measures and exchange rate regime classifications indicates that the theory is more fragile than widely accepted. This should encourage us to re-evaluate this universal acceptance that fixed exchange rate regimes are inferior to flexible regimes in coping with terms-of-trade shocks.

One of the main findings of my analysis is the discrepancy of my results across different terms-of-trade measures and exchange rate regime classifications - whereby the amplification effect of fixed regimes is significant only for one very specific choice of terms of trade measure in conjunction with one specific classification of countries into exchange rate regimes. This result is absent when using the *tot_gdp* measure and either classification, but also when using the *tot_trade* measure using the LYS classification. It is therefore worth exploring what might be the driver to this particular result arising only from combining *tot_trade* with RR. In countries that are not highly commodity-dependent, the *tot_trade* measure exaggerates the importance of terms-of-trade fluctuations in determining GDP growth. To illustrate, take a country that specialises in only a few commodities rather than many, and each of those commodities makes up a large portion of overall commodity trade, but overall commodity trade is not a large portion of GDP. By accounting for the importance to GDP, the *tot_gdp* measure renders some fluctuations insignificant. A similar intuition may hold for the interaction term if this overestimation of the importance of the commodity to the economy is larger for countries that have fixed regimes.

It is important to note that the two regime classifications disagree, amongst other things, on the categorisation of fixed regimes with parallel markets. Given that Levy-Yeyati and Sturzenegger analyse reserves and central bank actions in their classification of exchange rate regimes, it is likely that a substantial portion of regimes that were officially pegged but had a parallel market are coded as 1 (fixed) when using LYS. RR, on the other hand, considers the existence of multiple exchange rates as a separate category to pegs but often code these as flexible (which appears as 0 in my data). An example is Bolivia in 1980-82, whereby Reinhart and Rogoff (2002) point to the high parallel market premia, but these are coded as "freely floating" as a primary classification and "parallel market" only as a secondary classification, so it is coded as flexible despite the peg to the US dollar. In the LYS classification, on the other hand, Bolivia is coded as having a fixed regime in these years. This would imply that RR picks out only well-managed fixed regimes and therefore ignore a feature of fixed regimes that has has led to great distortions across time, which may be misleading. This provides intuition for the fact that fixed regimes are associated with lower GDP growth before 2004 when using LYS. It is curious, however, that this the amplifying effect is significant under RR but insignificant under LYS. On the other hand, it is possible that the pass-through into output is absorbed by the parallel exchange rate market in these regimes, softening the blow as if it were a floating regime. The literature may be right in using RR for this issue, although the 'missing' bad pegs may undermine the results for the pre-2004 period, when multiple exchange rate regimes were relatively commonplace; in fact, it is in the post-2004 period that the results fall apart, when multiple exchange rates are no longer widespread. However, the very fact that there is a polemic around exchange rate regime classifications indicates that the use of RR is not undoubtedly superior and thus the sensitivity of the results to the regime classification raises questions. It is also important to note that the LYS classification covers data up to 2014, so the post-2004 sample mostly covers an upward trend in the commodity supercycle. Both the upward trend and the reversal is covered by RR's data up to 2019, however.

Another interesting result that emerged is that even when there was significance in the sample before 2004, when some of the more influential papers were written, this vanished in the period after 2004. Whilst it is difficult to compare two periods where the world economy was so different, evaluating the relationship in this century in isolation shows that there appears to be no significant magnifying effect of commodity terms-of-trade transmission under fixed exchange rate regimes. There are a number of possibilities for why this may be the case.

One plausible and interesting explanation for this structural break is directly tied to the Mundell-Fleming model. In the model, fixed regimes amplify real shocks but dampen asset shocks. If terms-of-trade shocks this century have increasingly come to behave like asset price shocks, this would explain why the results post-2004 are contrary to the theory and earlier literature. In light of the increased importance of financial instrument trading, resulting in the recent counter-cyclicality and co-movement of commodity prices (e.g. Drechsel et al, 2019), there has perhaps been at least some shift in the nature of terms-of-trade shocks and their transmission channels in the economy. Some recent stylised facts on the financialisation of commodity prices are (1) the increased correlation between commodity prices (2) the increased correlation between commodity prices and other asset prices and (3) the sharp increase in the "number of transactions in commodity futures relative to commodity production" (Drechsel et al., 2019). Given these changes around commodities' increased role as financial instruments, the traditional theoretical motivations behind the push for flexible regimes may no longer be fitting. In that case, fixed exchange rate regime could potentially buffer against the volatility in the asset markets spilling into the economy, as illustrated by the model of asset price shocks. Point (3) becomes crucial here. Whilst acknowledging that the end of the 20th century and beginning of the 21st century are very different periods for developing countries, this paper seeks to highlight that the widely accepted inferiority of fixed exchange rate regimes may no longer be well-founded.

A different explanation might emerge from the Salter-Swan three-good model, once adjusted to allow composite commodities and varying income and substitution effects. In this model, the effects of a terms-of-trade shock depend crucially on the elasticity of substitution in consumption (Devarajan et al., 1993). If the elasticity of substitution between foreign and domestic goods of consumers in developing countries has increased over time, (adverse) terms-of-trade movements may be more easily offset by shifting consumption onto domestic goods when import prices rise. Previously, if the elasticity was low (e.g., if technology constraints precluded developing countries from producing goods of the same quality as abroad and so foreign goods were not easily substitutable), an adverse terms-of-trade shock would display only an income effect, making consumers poorer.

The nature of the terms-of-trade movement also matters. Demand shocks tend to have a significant effect on exporters' "real activity and external and fiscal balances", but supply shocks less so (Bluedorn et al., 2012). Perhaps a higher incidence of supply rather than demand shocks could explain the statistical insignificance of terms-of-trade movements to growth this century. One could also imagine, however, that if commodity prices move together, the export and import price indices might offset each other to some extent, making the overall effect insignificant.

Lastly, the pegged exchange rate regimes of today may not resemble their counterparts last century. They may now be accompanied by better exchange rate management and more advanced policy tools that do not end up accompanied by parallel exchange rates, as previously widespread and epitomised by Chile's five different exchange rates in 1952. It could also reflect a movement away from fixed regimes, which with a smaller sample would induce a loss of significance for the results, although the negative coefficient is striking here (Adam & Wilson, 2021).

8. Conclusion

This exploration has shown that the claim that flexible exchange rate regimes attenuate the impact of commodity price fluctuations on GDP growth is not irrefutable. To demonstrate this, I perform empirical analysis on different terms-of-trade measures and exchange rate regime classifications, to show the sensitivity of the results to the choice of measure and classification. Crucially, although the use of one terms-of-trade measure does suggest a significant amplifying effect of fixed exchange rate regimes, this dissipates after 2004. Using an arguably superior measure of the terms-of-trade, or alternative exchange rate regime classifications, I find no amplifying effect of fixed regimes. My findings thus dispute the general narrative in the literature and policy-making world which posits that flexible regimes are better insulators to terms-of-trade volatility. To complement this, I generate impulse responses using panel VARs in order to show that the response of real GDP to a terms-of-trade shock mirrors the patterns of my main specification, and ran a dynamic panel regression with an Anderson-Hsiao IV-style instrument for robustness. The structural break in the relationship between terms-of-trade movements and real GDP growth at the start of this century calls for further future research into a potential shift in the nature of the transmission of commodity price shocks into the economy, which could be related to the financialisation of commodity and foreign exchange rate markets. In light of this, I suggest that it may be time to re-evaluate the common wisdom around the superiority of one exchange rate regime over the other. Of course, different exchange rate regimes suit each country differently and there are many factors - e.g. broader institutional reform, as per Calvo and Mishkin (2003) - to account for rather than fixating on exchange rate regime. This paper, however, has sought to apply a toolkit of more comprehensive measures and recent data to assess the generalisation that flexible regimes tend to be better-suited to tackle commodity price volatility.

Appendix

Table 8.1: Main specification, restricting the sample to observations where the fixed regime has been in place for at least four years

Δy_{it}	(1) all	(2) pre-2004	(3) post-2004	(4) all	(5) pre-2004	(6) post-2004	(7) all	(8) pre-2004	(9) post-2004
Δtot_trade_{it}	0.0155	-0.00512	0.0365*				0.0332*	0.0368*	0.0195
	(1.11)	(-0.23)	(2.06)				(2.48)	(2.34)	(0.66)
$rr\_fixed_{i,t-1}$	0.00783	0.00275	0.0163*	0.00750	0.00210	0.0163*			
	(1.65)	(0.43)	(2.15)	(1.57)	(0.34)	(2.16)			
$\Delta tot\_trade_{it} * rr\_fixed_{i,t-1}$	0.0206	0.0531*	-0.0104						
	(1.35)	(2.34)	(-0.54)						
Δtot_gdp_{it}				0.151**	0.122*	0.179*			
				(3.06)	(2.00)	(2.28)			
$\Delta tot\_gdp_{it} * rr\_fixed_{i,t-1}$				-0.0457	0.0752	-0.158			
				(-0.66)	(0.82)	(-1.71)			
$lys\_fixed_{i,t-1}$							-0.00103	-0.00217	0.0164
							(-0.22)	(-0.46)	(0.98)
$\Delta tot\_trade_{it} * lys\_fixed_{i,t-1}$							-0.00935	-0.0177	-0.0109
							(-0.55)	(-0.86)	(-0.28)
N	3951	2280	1568	3951	2280	1568	2172	1430	672

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8.2: Main specification including controls that may determine exchange rate regime choice and affect growth (capital account openness, external debt to GNI ratio and inflation)

Δy_{it}	(1) all	(2) pre-2004	(3) post-2004	(4) all	(5) pre-2004	(6) post-2004	(7) all	(8) pre-2004	(9) post-2004
Δtot_trade_{it}	0.00969	-0.0120	0.0276				0.00438	0.00851	-0.00652
	(0.56)	(-0.40)	(1.44)				(0.41)	(0.71)	(-0.29)
$rr\_fixed_{i,t-1}$	0.00358	0.00117	0.00661	0.00349	0.000871	0.00652			
	(1.09)	(0.24)	(0.94)	(1.06)	(0.18)	(0.93)			
$\Delta tot\_trade_{it} * rr\_fixed_{i,t-1}$	0.00787	0.0283	-0.0118						
	(0.44)	(0.95)	(-0.59)						
Δtot_gdp_{it}				0.0929	0.0188	0.130			
				(1.32)	(0.21)	(1.40)			
$\Delta tot\_gdp_{it} * rr\_fixed_{i,t-1}$				-0.0574	0.0276	-0.106			
				(-0.71)	(0.27)	(-0.95)			
$lys\_fixed_{i,t-1}$							-0.00746	-0.0117	0.00324
							(-1.57)	(-1.94)	(0.57)
$\Delta tot\_trade_{it} * lys\_fixed_{i,t-1}$							-0.0120	-0.0218	-0.0151
							(-0.41)	(-0.54)	(-0.49)
$external\ debt\ ratio_{t-1}$	-0.0000779**	0.0000503	-0.0000426	-0.0000789**	0.0000459	-0.0000448	-0.0000711*	0.0000543	-0.0000314
	(-2.83)	(0.88)	(-0.84)	(-2.83)	(0.81)	(-0.90)	(-2.23)	(1.07)	(-0.91)
$capital\ account\ openness_{t-1}$	0.00319	0.0171	-0.0192	0.00314	0.0168	-0.0197	0.00477	0.0165	-0.0282
	(0.51)	(1.95)	(-1.47)	(0.51)	(1.94)	(-1.48)	(0.71)	(1.97)	(-1.55)
$inflation\ rate_{t-1}$	-0.00000183	-0.000000707	-0.000502	-0.00000182	-0.000000635	-0.000508*	-0.00000246*	-0.00000118	-0.000768
	(-1.95)	(-0.63)	(-1.97)	(-1.94)	(-0.62)	(-2.01)	(-2.54)	(-1.10)	(-1.91)
N	3117	1645	1384	3117	1645	1384	2238	1374	789

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8.3: Anderson-Hsiao-style IV for the lagged dependent variable

Δy_{it}	(1) all	(2) pre-2004	(3) post-2004	(4) all	(5) pre-2004	(6) post-2004	(7) all	(8) pre-2004	(9) post-2004
y_{it}	-0.0359***	-0.0717***	-0.106***	-0.0361***	-0.0716***	-0.107***	-0.0605***	-0.0869***	-0.182***
	(-5.16)	(-4.47)	(-4.55)	(-5.15)	(-4.44)	(-4.57)	(-6.46)	(-5.78)	(-5.02)
Δtot_trade_{it}	0.00548	-0.0213	0.0319*				0.0166	0.0106	0.0152
	(0.40)	(-0.98)	(1.97)				(1.61)	(0.89)	(0.79)
$rr\_fixed_{i,t-1}$	0.00501	0.00363	0.0175**	0.00486	0.00334	0.0175**			
	(1.17)	(0.64)	(2.82)	(1.13)	(0.59)	(2.81)			
$\Delta tot\_trade_{it} * rr\_fixed_{i,t-1}$	0.0297	0.0652**	-0.0150						
	(1.95)	(2.91)	(-0.84)						
Δtot_gdp_{it}				0.0990	0.0219	0.141			
				(1.95)	(0.33)	(1.91)			
$\Delta tot\_gdp_{it} * rr\_fixed_{i,t-1}$				0.0135	0.170	-0.120			
				(0.18)	(1.61)	(-1.38)			
$lys\_fixed_{i,t-1}$							-0.00586	-0.00618	0.00411
							(-1.51)	(-1.37)	(0.57)
$\Delta tot\_trade_{it} * lys\_fixed_{i,t-1}$							-0.00348	-0.00472	-0.0184
							(-0.16)	(-0.18)	(-0.62)
N	4149	2425	1616	4149	2425	1616	2944	1939	913

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

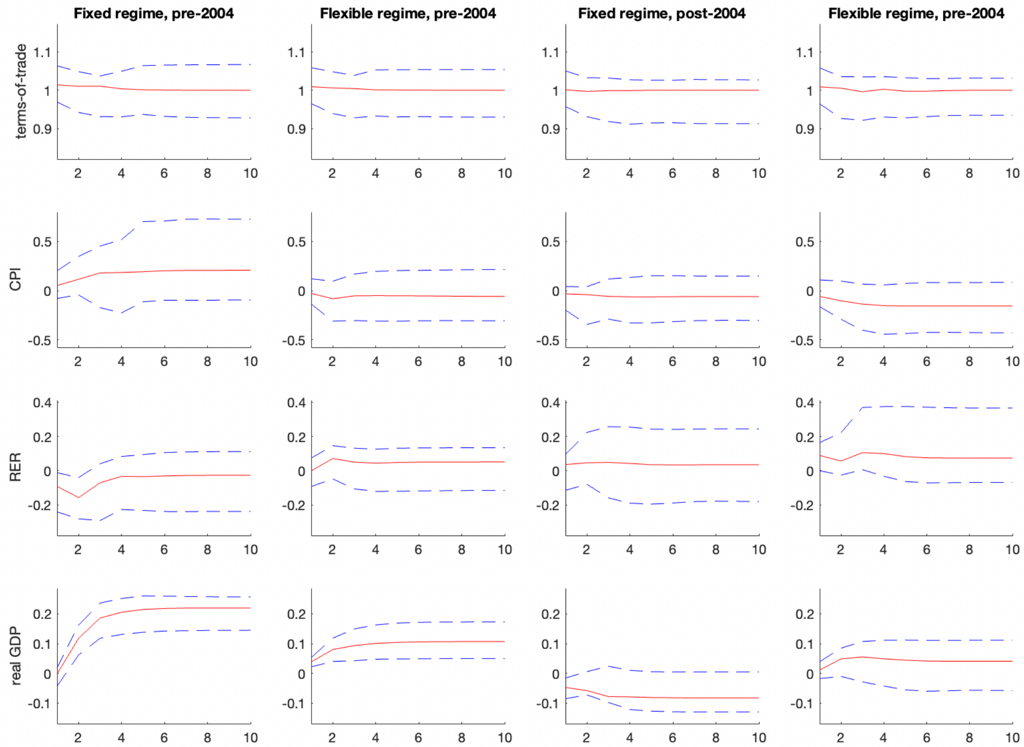

Figure 8.1: The responses to a 10% terms-of-trade shock under a fixed and flexible exchange rate regime (RR), using the *tot_trade* measure

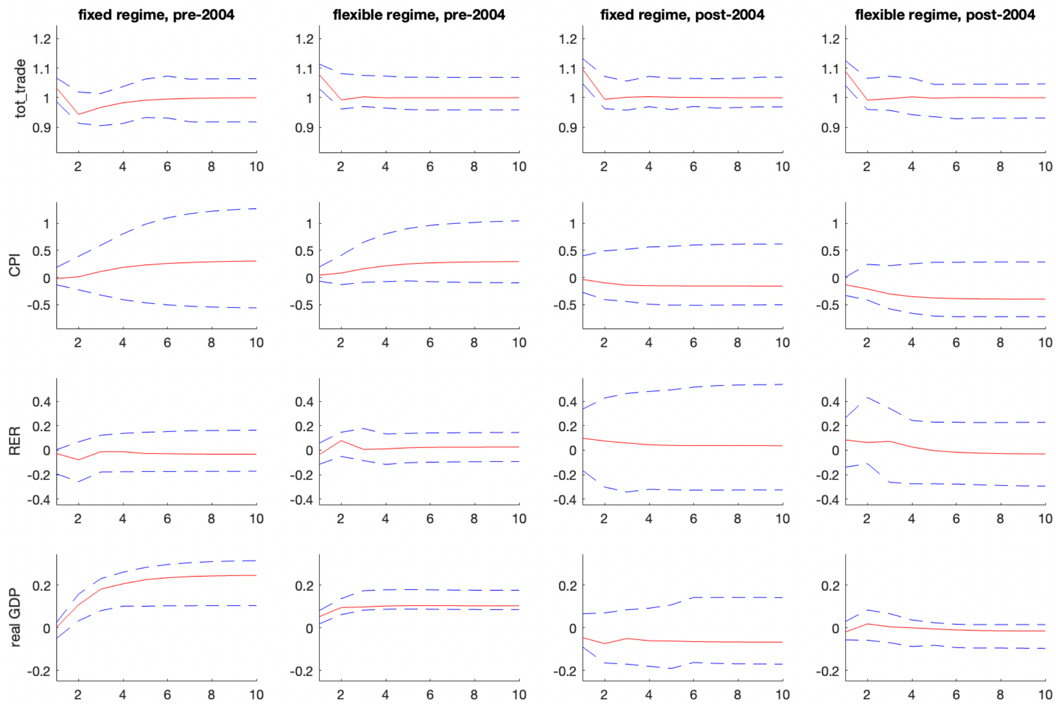

Figure 8.2: The responses to a 10% terms-of-trade shock under a fixed and flexible exchange rate regime (LYS), using the *tot_trade* measure

Table 8.4: Main specification, restricting the sample to observations where the fixed regime has been in place for at least two years

Δy_{it}	(1) all	(2) pre-2004	(3) post-2004	(4) all	(5) pre-2004	(6) post-2004	(7) all	(8) pre-2004	(9) post-2004
Δtot_trade_{it}	0.0145	-0.00586	0.0361*				0.0350*	0.0421*	0.0124
	(1.03)	(-0.26)	(2.06)				(2.53)	(2.54)	(0.44)
$rr\_fixed_{i,t-1}$	0.00862	0.00467	0.0168*	0.00833	0.00431	0.0167*			
	(1.97)	(0.77)	(2.15)	(1.89)	(0.71)	(2.16)			
$\Delta tot\_trade_{it} * rr\_fixed_{i,t-1}$	0.0234	0.0547*	-0.00934						
	(1.40)	(2.19)	(-0.47)						
Δtot_gdp_{it}				0.143**	0.0926	0.177*			
				(3.00)	(1.56)	(2.28)			
$\Delta tot\_gdp_{it} * rr\_fixed_{i,t-1}$				-0.0472	0.0739	-0.150			
				(-0.72)	(0.90)	(-1.55)			
$lys\_fixed_{i,t-1}$							-0.000204	-0.00299	0.00937
							(-0.04)	(-0.60)	(0.58)
$\Delta tot\_trade_{it} * lys\_fixed_{i,t-1}$							-0.00308	-0.00479	-0.0147
							(-0.16)	(-0.21)	(-0.37)
N	4128	2429	1593	4128	2429	1593	2233	1477	682

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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