Preferential trade agreements (ptas) effect on exchange rate

- Brent J. Sackett

Referee Report 3: Copelovitch, M. S., Pevehouse, J. C. (2013). Ties that Bind? Preferential Trade Agreements and Exchange Rate Policy Choice. International Studies Quarterly, 57(2): 385-399

Summary

This paper assesses the effect of preferential trade agreements (PTAs) on exchange rate policies. When a country joins a PTA, the government's ability to employ trade protection is constrained. This increases incentives to maintain fiscal and monetary autonomy in order to manipulate its domestic political economy. One way to do this is by implementing a flexible exchange rate policy. The authors argue that a PTA with a nation's " base" country (the country to whom they have traditionally fixed their currency, or a country where they have extensive trade ties), makes a country less likely to adopt a fixed exchange rate. In addition, this paper argues that countries who have signed a base PTA will also tend to maintain an undervalued exchange rate level. Using an original data set of 99 countries from 1975 to 2004, the authors find empirical support for their argument.

Evaluation

My overall impression of this article is positive. In fact, I would say this article will be excellent after a few methodological problems are corrected. The paper clearly identifies a research question and provides an important insight that expands our understanding of exchange rate policy. However, I will present some comments and recommendations for improvement.

Comment 1 (Theory and Causal Mechanism)

In general, the theory and hypotheses are clearly presented and easy to understand. However, one part of the theoretical link between PTAs and exchange rate policy is missing and should be discussed more thoroughly. This may simply be a matter of terminology, or it may indicate a missing link in thecausal chain. The authors assert that " PTAs generally commit members to more extensive free trade (2)." This seems to indicate the causal mechanism behind the story: PTAs tie the hands of governments who want to employ trade protection, so they resort to exchange rate policy instead of tariffs or other means.

However, PTAs are not all the same in the way they constrain behavior regarding trade protection (Baccini, Dür, Elsig & Milewicz, 2011, Kucik, 2012). While the authors note substantial cross-national variation in PTA *participation* , the discussion of variation in the *PTAs themselves* is inadequate. PTAs are not homogenous and actually vary substantially. Baccini et al. and Kucik both explain that variation in PTA design and implementation goes far beyond simple " free-trade" protections to include intellectual property, investments, enforcement, and even significantly differing tariff levels and exemptions. Is the paper's theory based on free-trade commitments generally or PTAs specifically? In footnote 9 on page 4, the authors state that GATT/WTO membership had no influence on exchange rate choice even though in theory it should constrain trade policy choice in the same way a PTA does. This leads to some confusion about the causal mechanism that needs to be clarified.

What exactly is the causal mechanism within PTA participation and why does it fail in other commitments to free trade? In addition, I would like to know if the large variation in PTA design effects the causal mechanism. These questions need to be answered to clarify the argument.

I have a second concern regarding theassumptions behind the theory. For the causal mechanism to work, the nation must feel pressure to comply with trade restrictions in the PTA. Otherwise, there is no incentive to use exchange rate policy to circumvent the PTA. However, others research has shown that compliance with international agreements is not straightforward and the intention to comply cannot be assumed (Simmons, 1998). Some nations may join PTAs with no intention to comply at all. Others may sign a PTA because they already intended to behave in accordance with the free trade commitments anyway. In either case, the causal mechanism of the paper is undermined. If Simmons and others are correct, a PTA may not provide the restraint the authors assume it does. Although a thorough discussion of compliance is not necessary, I would like to see it mentioned at least briefly. Both of these comments lead to some concerns about the data.

Comment 2 (Data)

I have two comments regarding the data. The first is a concern about potentialmeasurement errorsthat follows from my questions about the causal mechanism. The primary explanatory variable *BasePTA* uses the PTA dataset based on Mansfield et al. (2007). However, the data include significant heterogeneity in the likely causal mechanism (free trade commitments) that is not measured properly. Kucik notes that: “ At one end of the design spectrum, roughly 25% of all PTAs grant their members full discretion over

the use of escape clauses, imposing very few if any regulations relating to the enforcement of the contract's flexibility system. At the other end, no less than 27% of PTAs place strict limits on (or entirely forbid) the use of flexibility (2012, 97)." If this is true, a highly flexible PTA may actually be similar to an observation without a PTA at all. A more refined measurement of the causal mechanism than simple PTA participation may be needed.

My second concern regarding the data is related toselection effects. Countries do not join PTAs randomly. For example, democracies are more likely to participate in PTAs (Mansfield, Milner, and Rosendorff, 2002). In addition, there may be other unobserved reasons that individual countries decide to enter into PTAs – especially with their base country. I would like to see a more detailed discussion regarding selection effects and perhaps some statistical method to test for it such as a Heckman model.

Comment 3 (Methodology)

Two problems withendogeneityin the models need to be address. One of the primary dependent variables, *Undervaluation* , is calculated using GDP per capita (5) to control for the fact that non-tradable goods tend to be cheaper in poorer countries. This is problematic when GDP per capita is also used as an explanatory variable in models 3 and 4 as shown in Table 4. A model using the same variable on both sides of the equation potentially causes problems.

This is especially problematic considering the limitations of the other variable capturing the concept of undervaluation – *REER* . According to the authors, *REER* fails to capture the concept at all! *REER* "…does not actually

indicate whether a currency is over- or undervalued… (5).” It only measures changes in the exchange rate relative to the baseline year. The variable *Undervaluation* was added to correct this shortcoming, but is hampered by endogeneity. The combination of these two factors may be why the findings about exchange rate levels are not definitive.

Another form of endogeneitysneaks into the authors’ model. Beaulieu, Cox, & Saiegh (2012) illustrate that GDP per capita and regime types are endogenous. High levels of GDP per capita may simply be an indication of long term democratic government. When both variables are included in models predicting exchange rate policy, the resulting coefficients may be incorrect. The models reported in Tables 2 – 4 include both GDP per capita (log) as well as democracy (POLITY2) and result in inconsistent levels of statistical significance for both variables. This endogeneity should be addressed using a proxy or other methods.

I also have a minor concern withomitted variable bias. Bernhard, et al. (2002) emphasize that Exchange rate policy and Central Bank Independence (CBI) cannot be studied in isolation. They have potentially overlapping effects and measurements of both need to be included in a model explaining monetary policy. I recommend incorporating an additional variable that measures CBI.

My final concern with methodology has to do with theoperationalization of the conceptof democratic institutions. The authors briefly note that domestic political institutions influence exchange rate policy. Specifically, the nature of the electoral process and interest group influence can result in variations

in exchange rate policy (for example, Moore & Mukherjee 2006; Mukherjee, Bagozzi, and Joo 2014). In addition, Bearce (2014) shows that democracies manipulate exchange rate policy to appease domestic groups without regard to PTAs. To control for this, the authors use the Polity2 variable and two export composition variables. However the composite measurement of democracy fails to account for the variation in political institutions (such as parliamentary systems) that have been found causal in influencing exchange rate policy. In addition, the variables *Mfg Exports and Ag Exports* fail to account for an interest group's ability to influence policy. To fully control for democratic institutions, the authors need to identify the relevant democratic institutions and use a variable to capture those institutions. The Polity2 composite is inadequate.

Comment 4 (Discussion and Implications) **:**

My first comment about the discussion is positive. I think the model extension to capture the interaction effects between *BasePTA* and *Base Trade* is excellent and insightful. In particular, Figure 1 is very well done and clearly illustrates this effect. However, the rest of the discussion of the findings is overshadowed by the data and methodological problems. In particular, the comment about the " noisy (12)" nature of the findings regarding exchange rate levels seems like a cop-out. I would rather see the methodology strengthened instead of excuses (although to be fair, exchange rate levels are indeed noisy).

Smaller issues

The general structure of the paper is solid and the writing is clear, but I have some comments regarding minor issues that could improve the impact of this paper.

Comment 1 (Primary Dependent Variable discussion) :

I am concerned by the comment that the potential measurements of the dependent variable (*Exchange Rate Regime*) differ in methodology and yield "… quite different classifications across countries and over time (5)." This caused a red flag and left me concerned initially. Valid and reliable measurement of this variable is essential to properly test the hypothesis. I recommend rewording this and explaining more simply from the start why this variation exists and why it does not threaten the model.

Comment 2 (Inflation Variable discussion)

The *inflation* variable (6) uses two sources to account for missing observations (World Bank and IMF). I am concerned that the measurement methodology may not be exactly the same and could introduce bias when the observations are combined. A brief sentence or two covering the compatibility between the two sources would eliminate this concern.

Recommendation to the editor

Revisions required: This paper will make a strong contribution to the literature with some revisions. My biggest concern has to do with the causal mechanism and how the concept is captured in the primary explanatory

variable. Explaining this in more detail and addressing the other issues will make this paper ready for publication.

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