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COMMENTS ON OBSTFELD AND ROGOFF'S "THE SIX MAJOR PUZZLES IN INTERNATIONAL MACROECONOMICS: IS THERE A COMMON CAUSE?"

Charles Engel

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ABSTRACT

The paper offers comments on Obstfeld and Rogoff (2000). The comments primarily focus on three issues: (a) How do we reconcile the numerical examples of OR, which show quantitatively plausible resolutions to the major puzzles arising from costs of trade, with previous studies that have found trade costs do not get us very far? (b) Does the solution proposed by OR solve the puzzles at the expense of introducing new puzzles? That is, does their solution have counterfactual implications for other economic relationships? (The prime example of what I have in mind here is what OR call the "Backus-Smith puzzle".) (c) Some of the problems connected with points (a) and (b) can be rectified by moving away from the assumption of complete asset markets. But, then, how do we assess how much of the solution to the puzzle is coming from trade costs versus capital-market imperfections?

Charles Engel
Department of Economics
University of Wisconsin
1180 Observatory Drive
Madison, WI 23706-1393
and NBER, and University of Washington
cmengel@u.washington.edu

Obstfeld and Rogoff (2000) have once again written an important paper that undoubtedly will be highly influential in developing our understanding of many of the major puzzles in international macroeconomics. They highlight the fact that goods markets for consumers appear to be very far from being perfectly integrated, and show how this imperfection can help provide a unified understanding of the puzzles that have eluded satisfactory explanation. These goods-market imperfections are a plausible direction to look toward because the empirical evidence suggests they are significant in magnitude. And, Obstfeld and Rogoff (referred to as OR hereinafter) provide us with models that make sense at an intuitive level.

My comments primarily focus on three issues: (a) How do we reconcile the numerical examples of OR, which show quantitatively plausible resolutions to the major puzzles arising from costs of trade, with previous studies that have found trade costs do not get us very far? (b) Does the solution proposed by OR solve the puzzles at the expense of introducing new puzzles? That is, does their solution have counterfactual implications for other economic relationships? (The prime example of what I have in mind here is what OR call the "Backus-Smith puzzle".) (c) Some of the problems connected with points (a) and (b) can be rectified by moving away from the assumption of complete asset markets. But, then, how do we assess how much of the solution to the puzzle is coming from trade costs versus capital-market imperfections?

In reviewing some of the existing literature, it appears to me that trade frictions alone do not explain the puzzles. While they move things in the right direction, quantitatively goods frictions are insufficient. OR provide us with extraordinary intuition for why goods markets move things in the right direction, but we need more study to be

able to reconcile their compelling but simplified examples with the results that emerge from simulation of more fully-specified dynamic models. This very much reminds me of the literature on one puzzle that OR do not try to resolve – the forward premium puzzle. There, the easy explanation that was proposed is that a foreign exchange risk premium can lead to biased forecasts of the forward premium. But when researchers tried to embed risk premiums into calibrated equilibrium models and assess the size of that effect, they found that the risk premium was far too small to explain the magnitude of the deviations from uncovered interest parity. The parallel is that the literature so far has not found that goods-market imperfections alone can quantitatively explain the OR puzzles.

There is another parallel with the literature on the forward premium puzzle. When researchers finally were able to construct models that got close to matching the magnitude and sign of the deviation from uncovered interest parity, they found that their models had a very unpleasant implication about the moments of another variable. In that case, the problem was that the models implied nominal interest rate volatility that was much greater than what is found in the data. The parallel here is that the models that OR propose imply a high correlation of real exchange rates with relative consumption levels across countries. OR call this the "Backus-Smith" problem. OR appear to dismiss this issue, but in doing so leave me puzzled as to how we can reconcile the implications of their approach with the data.

My comments will focus on puzzles 2-4 of OR (which I call the "core puzzles"): the Feldstein-Horioka puzzle; the home bias in equity portfolios puzzle; and, the international consumption correlations puzzle. These three puzzles are linked in that they

can best be understood as pointing toward a surprising lack of risk sharing internationally. I only briefly comment on the other three puzzles.

To reiterate, I do think that costs of trade are fundamental in understanding these puzzles. Capital market imperfections alone are not the answer. OR provide new insight into how trade costs can help resolve the puzzles, and should help to focus future research endeavors in this promising direction.

1. The core puzzles

To my tastes, the clearest way to demonstrate the claim that trade costs alone can explain the core puzzles would be to use the model of complete asset markets and no trade frictions as the benchmark, and show how far trade costs get us. For example, the home bias in portfolios puzzle is no puzzle at all if the null model is one in which there are restrictions on asset trade or missing asset markets.

Let me briefly review the three core puzzles to help clarify. We find very low correlations of consumption internationally. That is puzzling because it seems to imply that there is very little sharing of idiosyncratic shocks to income. To me (and to OR) the puzzle is not that there is an absence of complete risk sharing. The puzzle is that there appears to be so little risk sharing – much less than we would expect given the wide array of assets that allow us to hedge risk. But how can we measure the ability of trade costs to explain the low correlation of consumption levels? The natural way to me (and apparently to OR), is to assess the effects of introducing trade costs into a model with complete asset markets. We know that the free-trade, complete markets model implies

perfect correlation – so how far does that correlation fall when there are plausible trade costs?

Home bias in portfolios is puzzling at an intuitive level. Investors could more effectively hedge risk by balancing their portfolios among assets from countries across the globe. Diversification is the fundamental principle of risk management. Again, however, it is helpful to have a benchmark to assess the effects of trade costs. In general, full diversification of equity holdings does not achieve complete risk sharing, but OR quite naturally focus on special models where that does occur. This special case is appealing because it gives us a simple benchmark to compare the effects of market imperfections against. Furthermore, as OR show in this paper (and in their 1996 textbook), "for realistic parameters, trade in equities alone can come quite close to attaining the complete-markets consumption allocation, so that the home bias evident under complete markets is a good guide to the home bias in an equities-only model."

The Feldstein-Horioka paradox has been a hard one to pin down. Why is the finding of low correlation of saving and investment a puzzle? OR's (1996) textbook has, for my tastes, the clearest explanation of the puzzle. In a Walrasian model with no trade barriers and complete asset markets, the amount of investment in a country's capital stock should be independent of the parameters that determine the country's consumption level. The simplest way to see this is to think of the special cases in which a diversified portfolio of equities mimics complete markets. In that case, the firm's decision to add to capital must be independent of the consumption choices of the individuals who live in the country where the firm produces. The firm is owned globally, so why would the consumption or saving decisions of the residents of the country where the firm is located

have any special impact on its investment decision? So, again, a natural benchmark to compare the effects of trade costs alone is the free-trade, complete-markets Walrasian model.

2. The literature

There are two reasons why I emphasize that the complete asset markets model is a natural benchmark. First, there actually exists a literature that looks into trade costs as an explanation for these puzzles. Using complete markets as the benchmark, introducing trade costs alone does not appear to get us very far in resolving the puzzles. The second reason I emphasize it is that while OR naturally gravitate toward the complete markets model as a benchmark, in several instances they subsequently inveigh against the complete markets model on the grounds essentially that in the real world markets are not complete. True, but the complete markets model is a useful benchmark. I address the literature in this section. In section 4, I return to the benchmark issue.

The careful reader might have noticed footnote 2 in OR. It makes reference to Backus, Kehoe, and Kydland (1992), which is the piece that brought the consumption correlation puzzle to the attention of the profession. That paper actually devotes an entire section to whether the introduction of trade costs of precisely the type OR propose can explain the consumption correlation puzzle. Their model is a fairly detailed Walrasian, complete-markets model. They can assess directly the impact of trade costs on consumption correlations. And, they find that the introduction of trade costs into their model actually makes the consumption correlation puzzle worse, not better. Further investigation by the same authors in a subsequent study using alternative specifications of

trading costs (Backus, Kydland and Kehoe (1995)) confirm that the consumption correlation puzzle is not solved by trading costs.

In fact, however, the Feldstein-Horioka problem is partly explained by Backus, Kydland and Kehoe when trading costs are introduced. And, as OR note in footnote 25, one can interpret some of their results as supporting the contention that moderate transportation costs help resolve the home-bias-in-equities puzzle. However, this illustrates where we need to go with the observations of OR. Does the solution to one puzzle make things worse for the others? When Backus, Kydland and Kehoe build a benchmark complete-markets free-trade Walrasian model, they find that introducing trade costs help in some dimensions but not others. And, as I shall discuss in the next section, there are some other dimensions along which the trade costs make things much worse.

I agree with OR that the dichotomy in many papers between traded goods and nontraded goods is not a useful one. As they say, we can probably think of all consumer goods as having a nontradable component. The problems they discuss in sections 6.2-6.5 ought to be at the core of what we do research on in international macroeconomics. But, still, one wonders whether the literature in which nontraded goods are introduced as an explanation for these puzzles might be instructive as to how far trade costs will get us. By and large, the nontraded goods models have not been particularly useful in resolving these puzzles. OR do provide a helpful description of the shortcomings of the nontraded goods model with the portfolio diversification paradox, and show how trade costs might get us further. But what about the other core puzzles? And what about the Backus-Smith paradox?

3. Other variables

As OR note in equation (15), the complete-markets models they introduce imply perfect correlation of the log of relative consumption levels internationally with real exchange rates. Backus and Smith (1993) were the first to derive this implication in a model with trade imperfections. (Theirs was a model with nontraded goods.) But, the condition arises in a wide-variety of contexts in which the law of one price fails.

The problem is that in the data there is virtually no correlation between relative consumption levels and real exchange rates. Backus and Smith document this in a fairly simple way for G7 countries. But, Kollmann (1995) and Ravn (2000) thoroughly demolish the notion that these two variables are connected. Kollman shows that, generally for advanced countries, real exchange rates and relative consumption levels are not cointegrated and that there is no discernible short-run relationship.

Of course, models sometimes have ancillary implications that are not supported by the data but are not critical to the issue of interest. But here, the implication is central to the resolution of the puzzles. In the OR models of this paper, trade costs lead to deviations from the law of one price, and deviations from the law of one price are the sole reason for the failure of purchasing power parity. The changes in the real exchange rate that are generated are, in turn, what break the link between consumption levels across countries. That is, it is precisely the non-constancy of real exchange rates in their models that explains why there does not appear to be a great deal of risk sharing.

My sense is that it is knowledge of the empirical findings of Backus and Smith (1993) and Kollmann (1995) that has convinced researchers that trade costs per se, or more generally models with law-of-one-price deviations, are not the sole solution to these

riddles. Perhaps researchers should not have been scared away from this avenue, but OR do little to help us out on this problem. They say that "Trade costs would play essentially the same role in a world with, say, trade in debt and equities but not a complete set of Arrow-Debreu securities." That may be true, but it needs to be demonstrated. Can trade costs play a quantitatively significant role in resolving the puzzles in such a model? At this stage, this seems not much more than a conjecture. The models that are presented in this paper all have the implication that relative consumption levels are perfectly correlated with real exchange rates. OR provide us with no evidence about models in which this link is broken.

It is also a bit disconcerting that OR focus exclusively on the implications of their models for the puzzles that the model is meant to address, and not on other implications of the model. The type of discipline that we rightly demand from the purveyors of general equilibrium Walrasian models (that is, the RBCers) is that they show us that the models can explain moments of some variables without generating unreasonable correlations among other variables. For example, would the OR models with trade costs imply negative correlation of inputs, as arise in many of the RBC models (with and without trade frictions or nontraded goods)?

4. The benchmark

OR seem to shrug off the Backus-Smith puzzle: "We do not take this as too damning, since for us the complete markets assumption was only a useful device for calibration, and not a religious conviction." Of course that is true for me too. But, where are we left? Apparently we need to concede that there is some deviation from complete

markets to be able to accommodate the Backus-Smith problem. How far from incomplete do they have to be? At what point have we stepped over the line and made capital market imperfections part of the solution to the problem? In short, how can OR say that we can solve these riddles "without appealing to capital-market imperfections"?

5. The other puzzles

Let me briefly comment on some of the other issues raised by OR. First, I am not convinced that allowing for high elasticities of substitution goes that far in solving the home bias in trade puzzle. There are small frictions in within-country trade, as well, and one would suspect that goods produced within a country's borders are even closer substitutes than internationally traded goods. Yet, the small intra-national trading costs do not seem to impose much of a barrier to intra-national trade. Indeed, the revised version of Evans (2000) concludes that the story in which "high border effects arise almost entirely from high elasticities of substitution provides at best a partial explanation" of the home bias in trade.

The misleading thing about the OR examples in this regard is that there are no intra-national frictions in trade. So, they tell us that .25 is a modest value for proportional international trade costs, but implicitly assume that 0 is a modest value for intra-national trade costs. It is easy to set up a model parallel to the one described in equations (1)-(6) of OR, but with two regions within each of two countries. Consider their calibration, allowing the elasticity of substitution intra-nationally and internationally to be equal to six, but introduce within-country trade costs of .10. Then the ratio of intra-national trade to international trade in the model falls to 2.5. If, in addition, one allows the intra-

national elasticity of substitution to be greater than the international elasticity (equal to 12 instead of six), the trade costs model goes only a small way toward explaining the home trade bias. The ratio of international to within-country trade explained by the model is merely 1.3.

I found the OR discussion of the final two puzzles engaging and stimulating. Let me make just two comments. First, I think even in trying to explain exchange-rate volatility it might turn out that we need more than just goods market imperfections. Here is why I make this conjecture. Betts and Devereux (1996) consider exchange-rate volatility in which consumer goods markets are completely segmented and the law of one price fails. In their static model, indeed they find exchange rate volatility is much larger (6 times larger) than a parallel model in which the law of one price and PPP hold. But when they move to a dynamic model with capital mobility (Betts and Devereux (2000)), the volatility effect is much smaller. Exchange-rate variance is only 1.7 times larger in the segmented-markets model compared to the model with integrated goods market. OR's intuition is that the goods-market frictions modify the dampening effect that capital markets have on exchange rate fluctuations. But, in a dynamic setting, Betts and Devereux's results suggest that the modification may not be large.

The second comment is that I think it is a mistake to link the "exchange-rate disconnect" puzzle with exchange rate volatility. One way of putting it is that the exchange-rate disconnect puzzle is about why exchange rates are not correlated with fundamentals. It is a puzzle about correlations not variances. In other words, I believe the case that OR are trying to make is that unobserved shocks might have a large effect on exchange rates if exchange rates are highly volatile. But observed shocks in the

money supply and other fundamentals also should have large effects. It is not immediately clear that high volatility in the exchange rate implies a weak link between the exchange rate and fundamentals (which is what the exchange-rate disconnect puzzle is all about.)

6. Concluding comments

I think there may be a close link between the type of goods-market frictions OR describe and possible failures in the capital markets. Because the discipline imposed by goods markets on the equilibrium exchange rate is so weak, there may be more room (particularly in the short run) for noise in exchange rates. That is, "chartists" as in Frankel and Froot (1990), or noise traders in Jeanne and Rose (1999), or order flow from foreign exchange traders as in Evans and Lyons (1999), might influence the exchange rate in the short run because misalignments in the exchange rate do not provoke a large immediate response from the real side of the economy. OR may be hinting at this in their section 6.7 (or they may not be.) I think this is a promising avenue to explore to help understand exchange-rate volatility and the disconnect between exchange rates and fundamentals. But it will require formal modeling and testing.

While it may seem that I am very skeptical of the ideas OR have presented here, I am not. My hunch is that their view and mine on these issues are very close (at least compared to the huge lack of consensus in international macroeconomics.) I am more cautious than OR about the degree to which trade costs alone have solved the puzzles. But this difference in tone probably mostly reflects the distinct roles of paper-writers versus paper-discussant.

One final thought: it may be that over the next 50 years or so, international goods markets become much more integrated and efficient through cyberspace, making the types of goods-market frictions that OR discuss less important over time. By the time we have built the models that explain the puzzles, the models and the puzzles might be obsolete.

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