

Reply to the Referees

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I thank the two referees for their thoughtful comments. Both obviously read my paper carefully and have provided well-considered comments. Naturally, I find some of the comments more important than others and do not agree with all of them, but in any case the referees have done a professional job. Here are my replies to the two sets of comments.

Referee #1

The referee makes ten points. I respond to them in the order in which they were presented in the referee report.

1. *Eliminate Section II.A.* I agree that this section can be eliminated from the main text. I either will move it to the Appendix or simply make it available on request. I put it in the paper because when I first started working on this paper I got questions about some of the issues discussed. However, I think the issues are pretty clear and can be handled with a couple paragraphs or so in the main text.

2. *Put the FOCs (15) and (16) in the Appendix.* I agree.

3. *Include more intuition for corner solutions.* I agree and will add some discussion. I actually am surprised that I didn't include some discussion to the existing mathematical derivation. The intuition is simply that no unit of purchase is any different from any other in terms of which currency should be used to buy it. As a result, if the first unit is most cheaply bought using currency #1, then all other units also will be most cheaply bought with that currency. That seems sensible. It is hard to imagine paying for the groceries partly with cash and partly with a credit card. To get the result that a combination of monies would be used for a single purchase would require that the net benefits of using a currency somehow declined in the amount of that currency being tendered. It is hard to imagine conditions under which such a property would obtain.

4. *Expand discussion of which goods are bought with which currency.* I will add a brief expansion. Even with both goods' rates of returns set to zero (and therefore the same), the goods differ in two ways: proportion of income spent on them *and* shopping costs required for buying the goods. The goods have different shopping costs B_g (see p.7 of the paper).

5. *Same issue applied to meaning of extensive and intensive margins.* The different shopping costs make the distinction between the goods meaningful.

6. *Endogeneity of goods produced and consumed.* This is a good point. The second referee brought to my attention a paper by Sturzenegger that touches on this issue in the context of a standard cash-in-advance (CIO) model. I will add relevant discussion to qualify my results and

relate them to Sturzenegger's paper.

7. *Effect of income on conversion costs and convenience yields.* I will have to think about this point some more. It is not obvious to me that there will be any such effects. At the very least, though, I will mention the fact that I am assuming the absence of such effects.

8. *Empirical implications hard to accomplish.* The referee is correct, but I see nothing to respond to. Both structurally and conceptually, the theory is quite simple. Nonetheless, many of the theory's implications are indeed hard to derive and sometimes cannot be signed without knowing the values of the parameters. Those characteristics may be unpleasant, but they are not proper grounds for criticizing the theory. If the theory is a reasonable description of reality, then the characteristics of its implications also are reasonable. That they are inconvenient to economists is irrelevant to their validity or the validity of the theory.

9. *More discussion of the empirical evidence.* I will add more discussion. In addition, I have received a private communication from Ed Feige bringing to my attention some empirical articles that I had not seen. I am reading those now and will add discussion of them where relevant.

10. *A weakness of the model is the cumbersome nature of some of the implications.* I disagree. The same argument as in point (8) above applies. As Einstein is reputed to have said (though I never have seen a citation of precisely where he said it): "Make everything as simple as possible, but not simpler." If my simple theory produces results that are not simple, maybe that means the world is complicated in unexpected ways and cannot be made simpler. I regard such an insight as a strength of the theory, not a weakness. The Black-Scholes model of option pricing is a second-order non-linear partial differential equation that cannot be solved analytically except in a few limited cases. Talk about cumbersome to work with! Yet no one would conclude that Black-Scholes is a "weak" result.

The referee also made some additional comments regarding exposition, which I will incorporate.

Referee #2

The referee makes seven points, the last of which is not numbered. I respond to them in the order in which they appeared in the referee report.

1. *Dynamics; inflation.* This point actually makes two unrelated points.

a. The theory is static. That assertion is neither true nor false because the theory is neither static nor dynamic. Dynamics can be studied only in the context of an equilibrium model. The theory is restricted to developing a model of the demand for currency substitution and nothing more. The resulting model can be used as one component of a dynamic general equilibrium model (indeed, it is my hope that it will be used that way by someone in the future),

but that exercise is not the purpose of the theory in the present paper. See the discussion below of Point 6 for related discussion.

b. The role of inflation. The referee notes that in the theory currency substitution arises only from differential inflation rates. He then argues that the empirical evidence is at odds with that characteristic. The referee may be right about the evidence (though it is hard to say because he cites none of it), but even so, the point is largely irrelevant. As I said in the introduction to the paper, this theory studies currency substitution that arises from excess money, that is, from inflation that is high relative to the inflation rates of other countries. Given that the purpose of the theory is to study inflation-induced currency substitution, evidence that currency substitution may arise for other reasons is irrelevant. Indeed, I mention in footnote 1 another theory of currency substitution that applies to situations where there is *insufficient* money. I can move Footnote 1 to the main text and expand the discussion a bit to make clearer what the theory does.

2. *The role of financial institutions.* Unfortunately, this point is garbled and largely unintelligible. It appears that the referee wrote a draft and then modified it one or more times but then forgot to check that the changes left the comment coherent. One thing that is clear is that the referee is concerned that financial development may not be related to costs of engaging in currency substitution. The referee asserts that in Bolivia and Peru the costs of holding foreign currency savings accounts are “similar” to those of domestic currency [accounts]. He does not cite studies or data and does not explain precisely how “similar” the costs are, so I have no way to evaluate his argument. I would like to respond better to Point #2, but its garbled presentation makes doing so impossible.

3. *Better justification for some assumptions.* The referee discusses two assumptions.

a. Transactions costs associated with buying goods. The referee states:

“For instance, the assumption that transactions in the goods market generate a cost seems to be an unnecessary assumption, particularly because this cost does not depend on the amount of money held by the agent. In all Baumol-type models, it is costly to convert deposits into money not goods into money.”

The referee apparently is unfamiliar with the branch of the Baumol-Tobin literature that includes commodity inventories. Most Baumol-Tobin models assume continuous purchases of goods. That is grossly unrealistic. To make the model more realistic, one can allow the household to hold commodity inventories, which may be purchased on shopping trips that involve a shopping cost. Having a shopping cost guarantees that the number of shopping trips will be finite, as they are in reality. The shopping cost is the per-trip cost of converting money into goods. For the model to be complete, it also is necessary to allow the possibility of converting commodity inventories back into money, which requires specifying a cost of liquidating inventories. That is the structure of my model, and the treatment is standard in any model that allows commodity inventories. See Santomero (J. Finance, 1974) for a full discussion of commodity inventories in a Baumol-Tobin model. Also, no transactions cost in a Baumol-Tobin model depends on

the amount of money held. The transactions costs all are constants. The transactions cost of buying goods is no different in that regard from any other cost in the model.

b. Fixed costs of holding currencies. The referee considers the possibility of fixed costs of holding currency extreme and in need of justification. I disagree that it is extreme, and I did justify it. As I said in the paper:

“For currencies, the fixed costs may be zero; however, monies, even foreign ones, need not be currency but rather may be bank accounts which often do carry fixed fees.” (p.8)

First, I said that those particular fixed costs could be zero, so anyone who doesn't like them can set them to zero and ignore them. Second, I was careful to explain that positive values of such fixed costs are natural if we allow money to include checking accounts. Checking accounts routinely carry fixed costs (although those costs usually can be avoided by holding some minimum balance, which merely converts the form of the fixed cost from an explicit outright payment to an implicit payment through an interest opportunity cost). So the possibility of fixed costs is not extreme.

I see no need to change anything in this part of the paper.

4. *Ranges of parameter values.* The referee complains that I do not discuss the range of parameter values for which certain results hold and do not provide a sensitivity analysis that evaluates which costs are important. This complaint is asking for another paper, in fact a sequel to this one. This paper is a theory piece, not an empirical piece. Furthermore, this paper explicitly discusses data in several places and notes in particular that readily available market data cannot be used to quantify the theory because market data arise from the interaction of supply and demand, whereas the theory applies to the demand side only. Doing the exercise the referee (and I, too) would like to see requires data that are not yet available. One of the benefits of the theory is that it provides guidance on what kind of data should be collected. Until such data are available, however, it is not possible to do the kind of calibration the referee would like to see. See the discussion below of point 6 for related comments.

5. *Exogenous versus endogenous expenditures.* I agree with the referee completely on this point. He notes that the theory takes expenditures to be given when in fact they would be endogenous and would be jointly determined with the demand for currency substitution. In light of that, he suggests that I qualify my conclusions about certain aspects of the demand for currency substitution. I should have been more careful about explaining this aspect of the theory and will make the necessary changes in a revised version.

6. *Partial versus general equilibrium.* The referee criticizes the theory for being one of partial equilibrium. That criticism is incorrect and, given that I discussed the issue explicitly in the paper, surprising. The theory is not one of partial equilibrium. It is a theory only of the *demand* for currency substitution. I quote from the introduction:

The theory presented here is limited to the demand side. It is not even a partial equilibrium model, so one cannot test it with market-generated data, such as aggregate money holdings. Doing that would require coupling the theory with a complementary theory of the supply side of the currency substitution market. Developing such a theory and then deriving the market equilibrium is a worthwhile endeavor, but it is far beyond the scope of the present paper, which should be regarded as a first step toward a complete theory of currency substitution.

I believe it is legitimate to write papers that develop theories of just demand or just supply and that never discuss market equilibrium or general equilibrium. See, for example, Milton Friedman's work on the permanent income hypothesis and Modigliani's related work on the life cycle hypothesis. Those were theories of the demand for consumption. No one criticizes them for not discussing market or general equilibrium. There are many similar examples in the literature.

7. Omitted references. The referee cites two theoretical studies that I did not mention and that I was unaware of. I will add discussion of them to a revised version. From a quick reading, it seems that the article by Uribe is not especially relevant to my paper because Uribe is concerned with network effects and transition dynamics in a general equilibrium model, something my paper does not get into (because, as explained above, the theory applies only to the demand for currency substitution). The article by Sturzenegger seems more closely related but is still quite different from my theory. Both Uribe and Sturzenegger use standard cash-in-advance models. Those are useful for studying general equilibrium because of the way they simplify the monetary aspects of the model. By the same token, however, they lose the detailed structure that is precisely the object of my theory. The Baumol-Tobin framework is a type of cash-in-advance model, but it has a much richer micro foundation than the standard cash-in-advance form. Its richer structure provides nice insights into the details of behavior but also makes it harder to use in a GE setting. I have some discussion of that point in the paper and can expand it to make clearer the costs and benefits of each framework.