Columbia University

Department of Economics
Discussion Paper Series

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Discussion Paper No.: 0809-13

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New York, NY 10027

January 2009

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January 25, 2009

*Revised draft of a discussion of Rodrik (2008) presented at the *Brookings Papers on Economic Activity* conference, September 2008. I would like to thank Neil Mehrotra for research assistance, and the National Science Foundation for research support through a grant to the NBER.

Dani Rodrik (2008) offers a provocative argument for policies that seek to maintain an "undervalued" exchange rate in order to promote economic growth. The key to his argument is the empirical evidence that he presents, indicating correlation of his measure of undervaluation with economic growth in cross-country panel regressions.

Rodrik does not really discuss the measures that should be undertaken to maintain an undervalued exchange rate, and whether it is likely that a country that pursues undervaluation as a growth strategy should be able to maintain persistent undervaluation. For example, he remarks (as justification for interest in the question of a causal effect of undervaluation on growth) that "one of the key findings of the open-economy macro literature is that nominal exchange rates and real exchange rates move quite closely together." But while this is true, and while it is widely interpreted as indicating that monetary policy can affect real exchange rates (since it can obviously move nominal rates), it hardly follows that monetary policy alone can maintain a weak real exchange rate for long enough to serve as part of a long-run growth strategy.

Indeed, conventional theoretical models with short-run price stickiness, that are perfectly consistent with the observed short-run effects of monetary policy on real exchange rates, imply that monetary policy should *not* have long-run effects on real exchange rates. Rodrik also cites evidence showing that sterilized interventions in the foreign-exchange market can affect real exchange rates. But economic theory suggests that interventions not associated with any change in current or subsequent monetary policy should have even more transitory effects. And the experiences of countries that have sought to use devaluation to boost economic growth have often found that the real exchange rate effect of a nominal devaluation is not long-lasting.[1]

Nonetheless, the point of the paper is to provide evidence that undervaluation favors growth, on the assumption that policies to maintain undervaluation are available, and it is that central contention that I shall examine here. I find the evidence less persuasive than the paper suggests, for two reasons. First, I believe that the paper exaggerates the strength and robustness of the association between the real exchange rate and growth in the cross-country evidence. And second, even granting the existence of such a correlation, a causal effect of real exchange rates on growth is hardly the only possible interpretation.

[1] The case of South Korea, discussed further in section 3, is an example.

1 How Strong Is the Association of Undervaluation with Economic Growth?

Rodrik's central findings derive from variations on a panel regression of the form

$$growth_{it} = \alpha + \beta \ln RGDPCH_{it-1} + \delta \ln UNDERVAL_{it} + f_i + f_t + u_{it}, \qquad (1.1)$$

in which $growth_{it}$ is the annual rate of increase in country i's per capita real GDP from the five-year period $t-1$ to the next five-year period t, $RGDPCH_{it}$ is the average level of per capita real GDP over period t, $UNDERVAL_{it}$ is a measure of the degree of undervaluation of country i's currency over that period, and f_i and f_t are country and time fixed effects. His key result (shown in his Table 1) is that the estimated coefficient $\hat{\delta}$ is found to be significantly positive and substantial in magnitude. The relation, he argues, is in fact confined to developing countries, as the coefficient is near zero when the sample is restricted to countries with per capita GDP greater than $6000 per year; but for the sample consisting only of countries with incomes less than $6000 per year, the coefficient is both larger and has an even larger t-statistic.

However, it is quite possible that Rodrik's measure of undervaluation exaggerates this association. Apart from the constant and fixed-effect terms, his measure of "undervaluation" is equal to

$$\ln UNDERVAL_{it} = \ln RER_{it} + 0.24 \ln RGDPCH_{it}, \qquad (1.2)$$

where RER_{it} is the real exchange rate of country i in period t (based on data from the Penn World Tables, as further discussed in his paper). But since lagged per capita income is also included as a regressor in equation (1.1), and since[2]

$$growth_{it} \equiv (1/5)[\ln RGDPCH_{it} - \ln RGDPCH_{i,t-1}],$$

his specification is *equivalent* to a regression of $growth_{it}$ on the variable

$$\ln RER_{it} + 1.2\, growth_{it}$$

and lagged per capita income, and his coefficient estimate $\hat{\delta}$ would be the coefficient on the "growth-adjusted real exchange rate" in this alternative specification. This way of viewing Rodrik's regression specification (1.1) makes it evident that a positive

[2]Note that t refers to a five-year period in Rodrik's panel regressions.

estimate $\hat{\delta}$ need not indicate any association between real exchange rates and growth at all — it may simply reflect the positive correlation between the growth rate *and itself.*

Rodrik defends the use of his constructed measure *UNDERVAL* on the ground that it is necessary to correct for the Balassa-Samuelson effect. One should expect a lower real exchange rate (more expensive nontraded goods) for higher-income countries, owing to the Balassa-Samuelson effect; Rodrik then defines the degree of "undervaluation" of a country's exchange rate as the degree to which its real exchange rate is higher than it would be expected to be given the country's level of per capita income. The latter prediction is made by regressing $\ln RER_{it}$ on $\ln RGDPCH_{it}$ in a panel regression with time effects but no country fixed effects, so that the correlation between countries' average real exchange rates and their average income levels can be used to estimate the relation. The coefficient on per capita income in this first-stage regression is (the negative of) the 0.24 appearing in the definition (1.2).

However, there are two objections that must be raised to this argument. First, Rodrik's panel regressions already include *country fixed effects.* Hence average differences in the level of the real exchange rate associated with particular countries (for example, the developing countries with low real exchange rates for the reason explained by Balassa and Samuelson) would have no consequences for the regression coefficient $\hat{\delta}$, even in the absence of Rodrik's proposed "adjustment" of the *RER* measure. There is only a need for a further adjustment if the Balassa-Samuelson effect is expected to create a higher-frequency correlation between income and the real exchange rate as well — if the five-year periods in which a country's per capita income is relatively higher are ones in which it should correspondingly have a relatively lower exchange rate — and the fact that the Balassa-Samuelson effect is well-established as a factor explaining long-run average differences between countries does not make it obvious that such a high-frequency effect should be important. (As a theoretical matter, this should only be true to the extent that it is also true at higher frequencies that variations in the rate of productivity growth in the production of tradeables are an important source of variations in both aggregate output growth on the one hand and the relative price of tradeables on the other.)

Second, even supposing that the high-frequency Balassa-Samuelson effect exists, the proposed correction will not necessarily be the correct one, and will generally introduce an upward bias in the estimated coefficient $\hat{\delta}$. This is because the Balassa-

Samuelson effect is not a direct causal effect of income on the real exchange rate (or equivalently, on the relative price of tradeables). Instead, it is a mechanism according to which both income and the relative price of tradeables are affected by a third variable (the rate of productivity growth in the tradeables sector), which creates a negative correlation between the two variables (to the extent that other factors do not also simultaneously affect both variables).

The correction proposed by Rodrik would be appropriate if one believed that income and the real exchange rate were determined by a structural model of the form

$$E = -\beta Y + P + u \tag{1.3}$$

$$Y = d\,E + v \tag{1.4}$$

where I now simply write E for the log real exchange rate and Y for log per capita income, P is a policy variable (treated as exogenous), and u and v are additional exogenous disturbances. Here (1.3) is a structural model of real exchange rate determination, in which the term $-\beta Y$ represents the (high-frequency) "Balassa-Samuelson effect" for which Rodrik apparently wishes to correct, and the term P indicates the kind of policy that can influence the degree of undervaluation, the effects of which upon growth Rodrik wishes to determine. Equation (1.4) is a structural model of income determination, in which the term $d\,E$ represents the growth effect of the real exchange rate as such (*i.e.*, independent of what has caused the exchange rate to vary) hypothesized by Rodrik. Though no such model is spelled our or defended, something of this form is implicit in Rodrik's empirical strategy.

Suppose that (1.3)–(1.4) were a correct model, and suppose furthermore that one has a strategy that allows one to identify the correct value of β (say, from the countries' long-run differences in incomes and in real exchange rates, on the supposition that there are no long-run cross-country differences in the terms P or u).[3] Under these assumptions, the "adjusted" real exchange rate

$$U \equiv E + \beta Y \tag{1.5}$$

will provide a measure of the composite disturbance $\tilde{u} \equiv u + P$. Under the further simplifying assumption that v is orthogonal to $\tilde{u}$, the coefficient $\hat{\delta}$ from a regression

[3] To simplify the discussion, I shall abstract from the problems created by the use of a generated regressor, and treat the true value of β as known with certainty.

of Y on U will be a consistent estimator of

$$\delta \equiv \frac{d}{1+\beta d} = \frac{\partial Y}{\partial P}. \tag{1.6}$$

This is precisely the interpretation that Rodrik wishes to give to his estimate of $\hat{\delta}$.

But among the several assumptions required for this approach to yield a consistent estimate of $\partial Y/\partial P$, note that the "Balassa-Samuelson effect" is treated as a direct effect of Y on E in equation (1.3). In fact, this is not the nature of the Balassa-Samuelson theory. Even treating the theory as one that refers to purely instantaneous and static effects (that therefore have the same quantitative form at all frequencies), the model should instead be one of the form

$$E = -a\,T + P + u \tag{1.7}$$

$$Y = c\,T + d\,E + v \tag{1.8}$$

where T is a measure of productivity in the tradeables sector, and according to the Balassa-Samuelson theory, the coefficients a and c are both positive. Here P is again a policy that is hypothesized to directly affect the exchange rate and $d\,E$ again indicates the hypothesized effect of exchange-rate variations (from whatever source) on national income. I shall suppose that T is an exogenous disturbance, independent of all of the factors P, u, and v.

Suppose now that the true structural model is of the form (1.7)–(1.8), but that one is able to correctly estimate the elasticity of the real exchange rate with respect to variations in per capita income *due purely to variations in productivity of the tradeables sector,* which is what one needs for the Balassa-Samuelson adjustment proposed by Rodrik. That is, suppose that one has a correct estimate of the coefficient

$$\beta \equiv -\frac{\partial E/\partial T}{\partial Y/\partial T} = \frac{a}{c-ad}.$$

(This could be estimated by a cross-country regression of long-run average real exchange rates on long-run average levels of per capita income, under the assumption that there are no cross-country differences in the long-run average values of either $\tilde{u}$ or v.) And again suppose that one constructs an "adjusted" real exchange rate, defined as in (1.5). What will be the economic interpretation of the coefficient $\hat{\delta}$ obtained by regressing Y on U? In particular, will it provide a consistent estimate of $\partial Y/\partial P$?

Under the assumption that β is correctly estimated, U will be a measure of "undervaluation" that has been purged of any effects of variations in the productivity of the tradeables sector; specifically,

$$U = \frac{c}{c - ad}\,\tilde{u} + \frac{a}{c - ad}\,v.$$

In this sense one has controlled for variations in the real exchange rate due to the Balassa-Samuelson effect. But this does *not* suffice to make $\hat{\delta}$ a consistent estimate of $\partial Y/\partial P$. Even under the assumption (for simplicity) that v is orthogonal to $\tilde{u}$, $\hat{\delta}$ is in this case a consistent estimate of

$$\delta \; + \; \frac{1}{\beta^2}\frac{(a/c)\sigma_v^2}{(c/a)^2\sigma_{\tilde{u}}^2 + \sigma_v^2}, \tag{1.9}$$

where δ is again defined as in (1.6). But this quantity is not equal to

$$\frac{\partial Y}{\partial P} = d,$$

for two distinct reasons. Even if $\sigma_v^2 = 0$, (1.9) will equal δ rather than d, but because the Balassa-Samuelson effect is *not* a direct effect of income on the exchange rate (as represented in (1.3)), the policy-relevant elasticity is d rather than δ. But likely more importantly, if $\sigma_v^2 > 0$, the second term in (1.9) represents an upward bias in the estimate $\hat{\delta}$. One would find a positive estimate for $\hat{\delta}$ even if the true policy elasticity d were equal to zero.

Not only is the coefficient obtained from a regression on U likely to be biased; it is far from obvious that this should be a more reliable estimate than would be obtained by simply regressing on the unadjusted real exchange rate. Assuming again that v is orthogonal to $\tilde{u}$, our simple model implies that the coefficient $\hat{d}$ obtained by regressing Y on E should be a consistent estimator of the quantity

$$\frac{d\sigma_{\tilde{u}}^2 - \beta^{-1}a^2\sigma_T^2}{\sigma_{\tilde{u}}^2 + a^2\sigma_T^2}.$$

This will be an under-estimate of the true policy elasticity d (if $\beta > 0$ and $\sigma_T^2 > 0$), owing to the failure to correct for the Balassa-Samuelson effect. But the bias will be relatively small as long as

$$a^2\sigma_T^2 << \sigma_{\tilde{u}}^2,$$

which is to say, as long as productivity growth in the tradeables sector accounts for a relatively small share of total high-frequency variation in the exchange rate. This last assumption seems a fairly reasonable one, except over quite long time periods.

How dependent are Rodrik's results on the use of the *UNDERVAL* measure? His Table 3 presents results for corresponding panel regressions using a variety of simple real exchange rate measures instead of his "adjusted" measure. In most cases, the measure of undervaluation is no longer a significant explanatory factor when the entire sample of countries is used. Rodrik instead stresses that when one restricts attention to the sample of developing countries, there remains a significantly positive[4] coefficient on the measure of undervaluation in three out of the four cases (albeit a substantially smaller coefficient than when *UNDERVAL* is used).[5]

These results indicate that within the sample of lower-income countries, there is a positive association between the level of the real exchange rate and growth, after one controls for country effects and time effects; Rodrik's basic finding is not purely an artifact of the way in which his preferred measure of undervaluation is constructed. Nonetheless, if one were to emphasize the results using the real exchange rate (as I would prefer), one would not only obtain a smaller numerical magnitude for the estimated effect, but more reason for concern for the robustness of the finding.

For example, when one uses the real exchange rate as the measure of undervaluation, it becomes more important to restrict attention to the sample of "developing" countries in order to find evidence of the association between undervaluation and growth. But this in turn leads to questions about what should define the sample of countries that are included. Table 1 illustrates the consequences for the value of the estimated coefficient $\hat{\delta}$ of alternative choices of the set of countries included in the sample. Here the measure of undervaluation used is the real exchange rate measure

[4]Here I mean significance at the 5 percent level or less. In the fourth case, the coefficient remains significant at the 10 percent level; see the next footnote.

[5]In Rodrik's baseline regression, using the measure *UNDERVAL*, he obtains an estimate $\hat{\delta} = 0.026$ with a t-statistic of 5.84. If the real exchange rate (the same measure RER_{it} based on the Penn World Tables that is used in constructing $UNDERVAL_{it}$) is instead used as the measure of undervaluation, the estimate of $\hat{\delta}$ falls to only 0.016, but with a t-statistic of 3.74, this remains a highly significant positive value. Depending on the measure of the real exchange rate used, the coefficient estimates reported in Table 3 are as low as 0.012, but the lowest t-statistic among the four measures considered is 1.92, so that the coefficient is close to being significant at the five percent level even in that case.

Table 1: Consequences of the choice of sample of developing countries for the estimated value of $\hat{\delta}$.

sample	coeff	s.e.	t-stat
$Y < \$6K$	.0144	.0038	3.77
$Y < \$8K$	.0091	.0037	2.50
$\$1K < Y < \$8K$	.0077	.0040	1.91

from the Penn World Tables (the one used in columns 1 and 2 of Rodrik's Table 3).[6] Line 1 of the table essentially replicates the result reported by Rodrik in Table 3.[7] Line 2 shows, however, that the estimated coefficient is reduced by one-third if the income cutoff is raised from $6000 to $8000. It is not obvious that only countries with per capita income less than $6000 should be regarded as developing countries; in particular, if the justification for expecting the effect that one is interested in to be present only in lower-income countries is that these countries have weaker institutions, it is not obvious that countries with incomes between $6000 and $8000 do not also have suffer from many of the institutional weaknesses that are common in the developing world.[8] But the evidence for a positive association between the real exchange rate and growth is considerably weaker when these additional countries are included in the set of "developing" countries. Moreover, the evidence becomes weaker still if the lowest-income countries (those with per capital incomes less than $1000) are excluded from the sample. (One is surely not much interested in using the experiences of these desperate countries as illustrations of a successful growth strategy.) When these countries are dropped from the sample, the estimated effect is only about half as large as for the "developing" sample used by Rodrik, and no longer significant at the 5 percent level.

[6]Note that among the real exchange rate measures that Rodrik considers in Table 3, this is the one that results in the most significant positive value for $\hat{\delta}$ when the sample is restricted to countries with per capita income less than $6000.

[7]As noted above, Rodrik reports a slightly larger coefficient (0.016) and a t-statistic of 3.74.

[8]The countries in this set are Swaziland, South Korea, Poland, South Africa, Turkmenistan, Chile, Kazakhstan, Taiwan, Venezuela, Bulgaria, Uruguay, and Mauritius.

2 Does the Correlation Indicate Causality?

Even granting the existence of a positive correlation between the level of a country's real exchange rate and its growth rate, is it legitimate to interpret this as evidence of a *causal effect* of the exchange rate on growth? In particular, is it evidence of a causal mechanism that can be relied upon in predicted the effects of a policy seeking to maintain a depreciated exchange rate?

I should begin by admitting that I suspect that at least some of the positive association found in the data does reflect episodes in which policies that manipulate the exchange rate have significant consequences for growth — specifically, examples of a familiar sort, in which policies that maintain an *overvalued* exchange rate create distortions that stifle economic activity. But Rodrik stresses that this well-known lesson is not the *only* connection between exchange-rate policy and growth; the declared purpose of his paper is to establish that policies leading to *undervaluation* are also *beneficial* to growth. Yet this is hardly established merely by observing that countries are able to reduce their growth rates by intervening to maintain an overvalued exchange rate. For example, the policies that maintain a severely overvalued exchange rate typically involve rationing of access to foreign exchange, and one may suppose that it is these controls, rather than the level of the exchange rate as such, that accounts for much of the reduction in economic performance; but if so, one can hardly argue on this ground that other types of interference with free convertibility will instead increase efficiency, as long as the controls maintain an undervalued exchange rate rather than an overvalued one. One might instead expect growth to be favored by a policy that does not create distortions of either sign.

Rodrik offers several comments on the issue of causality. The first is an assertion that while an inference of causality from real exchange rate depreciation to growth would be problematic "in a world where governments did not care about the real exchange rate and left it to be determined purely by market forces," in fact "most governments pursue a variety of policies with the explicit goal of affecting the real exchange rate." But there is a great leap between the observation that real exchange rates are affected by policy and an assumption that the real exchange rate is *purely determined* by policy, and by policies that are *exogenous* with respect to the state of the economy at that. Yet it would only be under the assumption that the real exchange rate is an exogenous policy choice that one would be able to sidestep the issue of causality.

In fact, Rodrik admits that endogeneity of the real exchange rate is an issue, and proposes two ways of dealing with it. One is an extension of his regression model to include additional explanatory variables, such as the inflation rate, government consumption as a share of GDP, and gross domestic saving as a share of GDP. Inclusion of additional variables lowers the coefficient $\hat{\delta}$ on the *UNDERVAL* variable, but the coefficient remains significantly positive;[9] this is taken to suggest that there is indeed a positive effect of undervaluation on growth, even after one has controlled for possible sources of endogenous variation in the real exchange rate. In fact, Rodrik suggests that some of the endogenous variation in the exchange rate that has been controlled for ought really to be counted as policy-induced exchange rate variation: "To the extent that [policies that reduce government consumption or increase saving] are designed to move the real exchange rate in the first place, they are part of what I have in mind when I talk of a 'policy of undervaluation'." This last point, however, is hardly convincing: if it is shown that policies that increase saving, for example, increase economic growth even when policymakers adopt them because of their anticipated consequences for the real exchange rate, it would hardly follow that policymakers should therefore be advised to attempt to depreciate the real exchange rate *by whatever means possible*; for the growth effect of the increased saving might occur through other channels than through the effect on the real exchange rate. Moreover, the mere fact that one has controlled for *some* possible kinds of endogeneity of the real exchange rate is hardly a proof that the remaining variation is exogenous.

Rodrik's final argument is an assertion that "many of the plausible sources of bias ... would induce a negative relationship between undervaluation and growth, not the *positive* relationship that I have documented." This, in his view, makes an interpretation of the positive value of $\hat{\delta}$ as reflecting omitted-variable bias implausible. It is accordingly perhaps worth discussing a simple example of how endogeneity of the real exchange rate could result in a positive correlation between the real exchange rate and growth, even under circumstances where devaluation would not stimulate economic activity at all.

I shall illustrate my point using a purposely oversimplified model of equilib-

[9] Of course, this robustness of the significantly positive coefficient may reflect the bias resulting from use of the *UNDERVAL* measure, discussed above.

rium real exchange rate determination.[10] Consider a two-period ($t = 1, 2$) small-open-economy model, with two sectors ($j = T, N$), producing tradeables and non-tradeables, respectively. I assume a competitive world market for the T good (which will also be the numeraire), and a world real interest rate $r > 0$ (in terms of the T good, between periods 1 and 2) that is unaffected by the net capital flows of the small country. Let the production technology in each sector j and each period t be of the Cobb-Douglas form,

$$Y_{jt} = K_{jt}^{1-\alpha_j} H_{jt}^{\alpha_j},$$

where K_{jt} is the capital stock in sector j, H_{jt} is hours of labor in that sector, and the coefficient $0 < \alpha_j < 1$ may be sector-specific. The initial capital stocks K_{j1} of both sectors are given as parameters, and I assume that K_{N2}, the capital stock of the N sector in the second period, is given exogenously as well. (To simplify, I shall assume a constant exogenous value, $K_{Nt} = K_N$ for both periods t.) The second-period capital stock of the tradeables sector instead depends on investment spending I, according to the law of motion

$$K_{T2} = I + (1 - \delta)K_{T1},$$

where $0 < \delta < 1$ is the rate of depreciation of capital in the T sector.

I assume that the representative household in the small economy seeks to maximize

$$U = U_1 + \beta U_2,$$

where the contribution to utility in period t is of the form

$$U_t = \gamma \log C_{Nt} + (1 - \gamma) \log C_{Tt} - \frac{\lambda}{1 + \nu} H_t^{1+\nu},$$

in which expression C_{jt} is consumption in period t of the sector-j good, and H_t is hours worked, and the preference parameters satisfy $\nu > 0$ and $0 < \beta, \gamma < 1$. For simplicity I assume competitive domestic spot markets each period for both labor and the N good, neither of which is traded internationally. Finally, the government sets

[10] In particular, my use here of a model in which monetary policy cannot affect the real exchange rate does not mean that I believe that in reality, monetary cannot influence the real exchange rate, at least for a time. My point is simply to show that a positive empirical correlation between the real exchange rate and real activity need not imply anything about the magnitude of the growth effects of exchange rate policy; and the point is made most simply with a model in which there is no scope at all for monetary policy to affect real variables, even in the short run.

the nominal exchange rate each period, which then determines the domestic-currency price of the T good in that period (by the law of one price). I shall suppose that the government also imposes a proportional tax τ on savings in period 1, so that the real return received by domestic savers is $(1-\tau)(1+r)$. I abstract from government consumption; hence the government revenue raised by the tax is assumed to be simply rebated lump-sum to households.

In any period t, given values for (K_{Tt}, Y_{Tt}), one can solve uniquely for equilibrium values of $H_{Tt}, H_{Nt}, Y_{Nt} = C_{Nt}, C_{Tt}, w_t$, and P_{Nt}, where here both the wage w_t and the price of non-tradeables P_{Nt} are quoted in units of the T good. (Thus w_t is a real wage and P_{Nt} is actually the relative price of non-tradeables.) One can easily show that there is a unique, differentiable solution for each of these variables, and that the solution functions satisfy (among other properties)

$$\frac{\partial C_T}{\partial Y_T} < 0, \qquad 0 < \frac{\partial \log C_T}{\partial \log K_t} < -\frac{\partial \log C_T}{\partial \log Y_T},$$

$$\frac{\partial Y_N}{\partial Y_T} < 0, \qquad 0 < \frac{\partial \log Y_N}{\partial \log K_T} < -\frac{\partial \log Y_N}{\partial \log Y_T},$$

$$\frac{\partial GDP}{\partial Y_T} \equiv 1 \; + \; P_N \frac{\partial Y_N}{\partial Y_T} > 0$$

$$\frac{\partial P_N}{\partial Y_T} < 0, \qquad 0 < \frac{\partial \log P_N}{\partial \log K_T} = -\frac{\partial \log Y_N}{\partial \log Y_T}.$$

Using these solution functions, an *intertemporal equilibrium* can then be described as a set of values for the endogenous variables (Y_{T1}, K_{T2}, Y_{T2}) that satisfy the three equilibrium conditions

$$\begin{aligned} C_T(K_{T1}, Y_{T1}) \;\; &+ \;\; [K_{T2} - (1-\delta)K_{T1}] \; + \; \frac{C_T(K_{T2}, Y_{T2})}{1+r} \\ &= \;\; Y_{T1} + \frac{Y_{T2}}{1+r} \end{aligned} \qquad (2.1)$$

$$C_T(K_{T2}, Y_{T2}) \;\; = \;\; \tilde{\beta}(1+r) C_T(K_{T1}, Y_{T1}) \qquad (2.2)$$

$$(1-\alpha_T) Y_{T2} \;\; = \;\; (1+r) K_{T2} \qquad (2.3)$$

given values of the exogenous parameters $(K_{T1}, r, \tilde{\beta})$, where

$$\tilde{\beta} \equiv \beta(1-\tau).$$

Here (2.1) is the requirement that there be intertemporal balance in the country's capital account (assuming zero net foreign assets at the beginning of period 1); (2.2) is the Euler equation for an optimal saving decision by the representative household; and (2.3) is the first-order condition for profit-maximizing investment demand, stating that the anticipated marginal product of capital in period 2 must equal 1 plus the required real rate of return.[11] One can again show that there is a unique solution to these three equations for the endogenous variables as differentiable functions of the exogenous parameters.

Consider now the consequences of an exogenous increase in the composite parameter $\tilde{\beta}$, which implies an increase in domestic households' willingness to save, either as a result of a change in preferences (an increase in β) or a change in policy that increases incentives for saving (a reduction in τ). Total differentiation of the system of equations consisting of (2.1) – (2.3) reveals that

$$\frac{\partial Y_{T1}}{\partial \tilde{\beta}} > 0,$$

which implies in turn that

$$\frac{\partial GDP_1}{\partial \tilde{\beta}} > 0, \qquad \frac{\partial P_{N1}}{\partial \tilde{\beta}} < 0, \qquad \frac{\partial (Y_{T1} - C_{T1})}{\partial \tilde{\beta}} > 0.$$

Hence an increase in the willingness to save in period 1 (whether due to changing attitudes or to changing incentives) will simultaneously increase the production of tradeables (Y_{T1}), the small country's exports ($Y_{T1} - C_{T1}$), and its real GDP (GDP_1), while reducing the relative price of non-tradeables (P_{N1}) and hence increasing the real exchange rate.

Note that this equilibrium scenario resembles the phenomenon often interpreted as "export-led growth": a real depreciation of the country's exchange rate coincides with an increase in exports and an increase in total GDP (hence an increase in the growth rate). Moreover, if one were to compare a panel of small open economies, to each of which the above model applies, with identical parameter values except for cross-country variation in the value of $\tilde{\beta}$, one would observe a positive correlation between a country's real exchange rate in period 1 and its growth rate in that period.[12] Yet the

[11]Note that since period 2 is the last period of the model, there is effectively 100 percent depreciation of capital in this period.

[12]The exogenous parameters taking identical values for the different countries are assumed to include the level of GDP in the period prior to period 1, with respect to which the period 1 growth rate is calculated.

high-growth countries would not be in this situation because of their exchange-rate policies; their higher growth rates would be due to other factors (factors that favor a higher saving rate) that happen to lead *both* to a lower equilibrium real exchange rate and to higher GDP growth. Moreover, the model is one in which if a country were to use monetary policy to depreciate its nominal exchange rate, this would not affect growth (or any other real variables, including the real exchange rate) — it would only raise the nominal domestic prices of both tradeables and non-tradeables (without affecting their relative price).

It is true that there *is* a policy intervention, in the simple model, that would depreciate the real exchange rate; this is a reduction in the tax rate on savings τ, which is one of the factors determining the value of $\tilde{\beta}$. And such a policy change would increase GDP (through its effect on saving), in the same way as an increase in households' patience would. But it does not really make sense to call this a demonstration that a deliberate policy of exchange-rate depreciation can be used to stimulate, since the most obvious example of a policy with that intent would be completely ineffective.[13]

The example shows that it is certainly possible for an omitted variable to move both the real exchange rate and GDP in the same direction, so that this is a potential interpretation of a positive coefficient $\hat{\delta}$ in Rodrik's panel regression. But is this theoretical possibility likely to be of practical relevance? Here it is worth noting that Rodrik's regressions (reported in his Table 10) show that a country's gross domestic saving rate (as a share of GDP) has a significant positive effect on his *UNDERVAL* measure of undervaluation of the country's currency; and of course, a higher saving rate is also correlated with higher growth, as many authors have noted, and as Rodrik's panel regressions in Tables 4 and 5 show. (The latter regressions show that the saving rate is a significant variable in explaining differences in growth across country-time pairs, even when the undervaluation measure is also included in the regression; and that inclusion of the saving rate as an explanatory variable reduced the size of the estimated coefficient on the measure of undervaluation.)

Rodrik notes that the inclusion of the saving rate in the growth regressions does not completely eliminate the significance of *UNDERVAL* as an explanatory variable, and concludes from this that endogeneity resulting from factors of the kind illustrated

[13]Moreover, some other policies that would result in real exchange rate depreciation as a byproduct would lower GDP growth rather than raising it.

in the simple example do not fully account for the association between undervaluation and growth. But the fact that inclusion of a single proxy for factors of the kind represented by the simple example eliminates only part of the association between *UNDERVAL* and growth[14] hardly establishes that endogenous mechanisms of this kind are not responsible for the correlation — in particular, for the cases in which undervaluation coincides with strong growth, as opposed to the cases in which overvaluation coincides with weak growth.

The simple example also illustrates another important point. The mere existence of a positive correlation between the real exchange rate and growth (across some class of developing countries) need not be evidence of any *greater distortions* in the tradeables sector, that can in turn justify policies that essentially act as subsidies to that sector. Ultimately, this is Rodrik's argument for the pursuit of an undervalued exchange rate: one would like to subsidize the production of tradeables, but for political economy reasons, it may be most practical to do so by manipulating the exchange rate rather than through industrial policy. But the main evidence that is offered for the hypothesis of an inefficiently small relative size of the tradeables sector in developing economies is the evidence for a stimulative effect of real exchange rate depreciation. Yet in the simple model, a positive correlation exists between the real exchange rate and growth — and higher growth is associated with a shift of resources from the non-tradeables to the tradeables sector — but this does not mean that the equilibrium production of tradeables is suboptimal. In the case that $\tau = 0$, the intertemporal equilibrium maximizes the welfare of the representative household (subject to the constraint that trade with the rest of the world must satisfy intertemporal balance of the capital account; and the introduction of a subsidy for the production of tradeables would *reduce* welfare, relative to that optimum. Similarly, the introduction of other sorts of market distortions that represent indirect ways of subsidizing the tradeables sector would most likely reduce welfare, whether or not they would increase GDP.

[14] Again, one should remember that it is only the association of *UNDERVAL* with growth that is shown to be robust to inclusion of the saving rate in the regression, not the association between simple measures of the real exchange rate and growth. One should not expect the association between *UNDERVAL* and growth to be completely eliminated by the inclusion of any number of regressors representing determinants of the real exchange rate, because *UNDERVAL* also reflects the economy's growth rate, as explained above.

3 A Case Study: South Korea

Ultimately, the issue of causality is unlikely to be settled using panel regressions of the kind that constitute Rodrik's main results, owing to a lack of suitable instruments for exogenous changes in exchange-rate policy. Case studies can often be more illuminating in this regard. Here I consider only one, the case of South Korea. I select this country for further consideration because it is one of the countries which Rodrik displays as an illustration of the association of growth with undervaluation of the exchange rate (see his Figure 3). A more complete picture of the degree of support for Rodrik's thesis that is provided by the example of South Korea can be obtained by looking at higher-frequency data (rather than only the five-year averages shown in his figure) and at additional time series besides those for *UNDERVAL* and the growth rate alone.

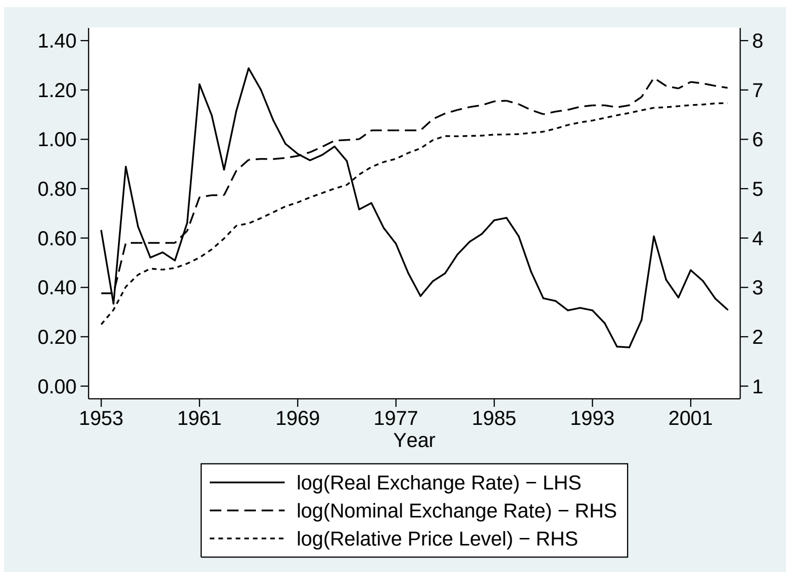

Figure 1: The nominal and real exchange rate: South Korea, 1953-2004.

Figure 1 plots annual data for both the (official) nominal exchange rate (between the won and the U.S. dollar) and the implied real exchange rate, as well as the relative price level between South Korea and the U.S. In the plot of the nominal exchange rate, one sees that there were several large devaluations in the 1950s and 1960s — in particular, those in 1955, 1960, 1961, and 1964.[15] These do each result (at

[15]There was a 30 percent devaluation in February 1960, followed by another 100 percent devalua-

least temporarily) in substantial depreciations of the real exchange rate, and so closer examination of the Korean experience clearly shows that at least some of the relatively high-frequency variation in the real exchange rate represents effects of exchange-rate policy. But at the same time, the plot also makes it clear that devaluations need not have any long-lasting effect on the real exchange rate. In the case of the 1955 devaluation, much of the effect on the real exchange rate has already been undone by increased inflation by 1957; in the case of the 1961 devaluation, much of the effect has been undone by increased inflation by 1963. Indeed, this fact explains why the Korean government found additional large devaluations to be necessary so soon after the previous ones.

It is true that the 1964 devaluation might appear to have been more successful; for the next decade, Korea maintained a real exchange rate that was substantially weaker than it had been during most of the 1950s. And of course this was also the decade over which Korea's real GDP growth accelerated to a rate of 6-8 percent per year (as shown in Figure 2), which Rodrik interprets as supporting the view that an undervalued currency was the key to the Korean growth "miracle." But in order to attribute the sustained real depreciation to the 1964 devaluation, one must explain why earlier devaluations did not have similarly long-lasting effects.

An obvious interpretation would be that while in the earlier cases, there had not been any change in the *equilibrium* real exchange rate, so that monetary policy could weaken the real exchange only temporarily, the 1964 devaluation coincided with a weakening of the equilibrium real rate, so that the devaluation — rather than resulting in a true undervaluation — facilitated a shift in the real exchange rate that would have had to occur in any event. Why might the equilibrium real exchange rate have weakened? A clue is provided by the fact that the gross domestic saving rate surged after the early 1960s, as is also shown in Figure 2.

Prior to 1965, ceilings on bank deposit rates depressed household saving, since (under the high inflation of the time) the implied real interest rates on deposits were negative. Instead, households lent funds to the informal financial sector, where interest rates were quite high. By raising interest rate ceilings in 1965 and at the same time reducing inflation, the government brought household savings back into the banking system, and so reduced the cost of capital for businesses through more efficient in-

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tion in February 1961, though the annual data are not high-frequency enough to show two distinct steps in the plot.

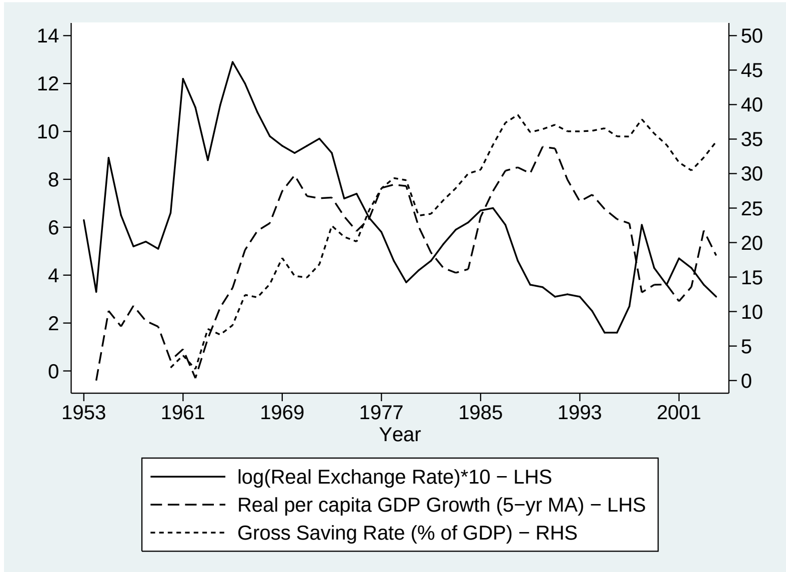

Figure 2: The real exchange rate, saving, and growth: South Korea, 1953-2004.

termediation (Kim, 1991, p. 137). In addition, tighter fiscal policy increased public saving, further contributing to the sharp increase in overall domestic saving.

This increase in domestic saving — which coincided fairly closely with the acceleration of economic growth — was likely an important cause of the growth miracle. Moreover, the simple model presented above shows that increased incentives for saving can also increase the equilibrium real exchange rate. This may be one of the reasons for the equilibrium real exchange rate for Korea to have been higher in the late 1960s and early 1970s than it had been earlier, which would explain why the effects of the 1964 devaluation on the real exchange rate were not quickly reversed. Indeed, Kim (1991, p. 132) argues that Korea's persistent current account deficit and buildup of external debt in the decade after 1965 point to won overvaluation, not undervaluation, in this period.[16]

Of course, the view suggested by this discussion of the Korean case does not imply that exchange rate policy is completely irrelevant to a country's development strategy. Overly tight regulation of financial flows can be an important impediment to growth, as seems to have been the case in Korea before the 1960s; and extensive controls are often required by policies that seek to maintain an overvalued exchange rate.

[16]This is a further reason to doubt the accuracy of Rodrik's *UNDERVAL* measure.

Hence creation of conditions conducive to growth will, among other things, mean refraining from attempts to maintain a seriously overvalued currency. Moreover, the Korean case shows that the process of development may involve a reduction in the equilibrium real exchange rate (that is, the one that would result from fully flexible wages and prices and an absence of impediments to capital flows). In such a case, a nominal devaluation of the currency can be valuable, as a way of allowing the necessary real depreciation to occur without the more painful process of forcing wage and prices down in response to insufficient aggregate demand. But such a policy is not correctly described as the pursuit of an "undervalued" currency; rather, it is again an example of the wisdom of avoiding overvaluation, with the important proviso that the equilibrium rate, with respect to which overvaluation must be defined, can easily change as the economic structure changes.

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