Dear T.N.

I have now had a chance to read your paper with Jagdish. Here are my comments. I hope they are of some use.

Your main points seem to be the following: (a) there is nothing substantively new in the "new" growth theory; (b) the older detailed country studies of the NBER-OECD-WB series have convincingly demonstrated that trade liberalization induces sustained growth; and (c) cross-national regressions are not useful to shed light on the relationship between trade policy and growth.

With regard to (a), I think your fight is with the mainstream of the profession rather than with me. I have no stake in the issues, have not claimed novelty for myself in any of my statements about the theoretical relationship between trade policy and growth, and am perfectly happy to let what I think are largely semantic issues pass. You obviously accept the proposition that as a matter of theory the relationship between open trade policies and economic growth is ambiguous. In my own work I ask readers to accept no more than that.

With regard to (b), I think you underestimate the degree to which the country studies you refer to are open to diverse interpretations. I have great admiration for these studies, which I think have taught us two important things. First, the actual pattern of incentives generated by the policy regimes in place--as measured by the dispersion in ERPs for example--have been much more haphazard (and inefficient) than what any policy maker, whatever his beliefs in infant industries or import substitution, could have rationally aimed to achieve. Second, exchange control regimes based on a combination of inappropriate monetary and fiscal policies and foreign currency rationing have been very costly, leading to stop-go macroeconomic cycles, periodic crises, and slow growth.

But to say that these studies have demonstrated that countries with lower levels of trade protection grow faster than other countries would be a very big leap. For one thing, it is in the nature of these country studies that the policy episodes under consideration involved a mix of institutional, commercial, exchange-rate, and macreoconomic policy changes, rendering precise attribution very difficult. In fact, none of the NBER studies that I have read was able to disentangle the effects of macro and exchange-rate policies (i.e., the elimination of macroeconomic disequilibria) from those of reforms in commercial policy proper.

For another, the evidence presented in the original country studies themselves is highly ambiguous on some dimensions. Let me cite just two examples. The Little et al. project found that Taiwan--the archetypal "outward oriented" (OO) economy--had a higher average ERP in manufacturing, as well as greater variation in ERPs, than Mexico--a leading examplar of an ISI country--long after Taiwan's trade reforms were introduced. What do we make of this evidence if we believe that OO countries had lower trade protection or that a high level of inefficiency in resource allocation was what set ISI countries apart from the OO countries? The second example is from Korea. The very useful book by Frank et al. in the NBER series that Jagdish co-directed found that anti-export bias (EERm/EERx) was not significantly higher in Korea during the 1950s than in the 1960s; in fact, the relative price of exportables was higher in 1959-60 than at any time during the 1960s. How then can we attribute the export boom and rapid growth that starts in full force in the mid-1960s to the trade reforms of the early 1960s?

The point of these examples is that the evidence is not so clearcut, and that it is in fact possible to construct a different account of East Asian growth (and of disappointing performance elsewhere) based on the very same evidence presented in the underlying country studies in the NBER-OECD-WB projects. My 1995 paper on Korea and Taiwan (published in Economic Policy) does exactly that: it relies heavily on the Frank et al. book to sketch an argument in which trade policy plays a largely supportive and secondary role. My account of the ISI countries in the 1999 ODC book--why they did well for a while and why they collapsed--is also based on the evidence from these country studies. My stories may well be wrong. But they are not inconsistent with the evidence presented in the NBER-OECD-WB projects, and in fact are based on interpretations of that evidence.

Your own interpretation of the reasons for EP countries' success (on pp. 25-31) is, I think, no less speculative than mine. (I actually think it is more so, but that debate will have to wait for another occasion.) My main disagreements with your story would be the following: that it conflates macro with micro (when you associate ISI regimes with "overvalued exchange rates" (p. 26); that it is not fully supported by the available evidence (as indicated, for example, by the examples from Korea, Mexico, and Taiwan cited above); and that it embodies a lot of implicit or explicit, but in either case empirically unverified theorizing about the political economy of trade protection, the role of rent-seeking, the quality and quantity of DFI under ISI versus EP, and the investment-promoting effects of EP. I apologize for using short hand here, but since I have written about these issues elsewhere there is little point in repeating them here.

As regards (c), I think you are being way too harsh on the cross-national literature. Yes, of course, there are lots of pitfalls here--pitfalls, which I might add are well recognized by the empirical researchers in this area. But it is one thing to point out the problems, and another thing to write off the whole literature. One thing I learned in the very first econometrics course I took (it was from Deaton) is that you do not shrug off a regression by simply saying the data are bad or the methodology is inappropriate. You have the burden to demonstrate how these shortcomings (in measurement or in method) bias the results you are criticizing in a particular direction. Your cavalier dismissal of the empirical growth literature does not pass this test.

My own views on the matter are quite pragmatic. For reasons I discussed above, case studies can rarely settle matters on their own. Typically, case studies--even when they are comparative--suffer from negative degrees of freedom. For example, as I mentioned above, the NBER studies leave entirely unresolved the question of whether it is macro policies or trade policies or both that really matter. On the other hand, any result from the cross-national literature that is not validated by high quality case studies is probably not worth much. In the end, we have to use both kinds of evidence.

Let me now turn to some specific problems I have with the way that you have characterized my views and writings. There are many statements in the paper that mislead the reader about what I have or have not said. Some examples:

pp. 4-5: You say here that I have fallen victim to a "common form of error." You are wrong. I have not cited the Solow model or any other model "to argue that ... we all believe only what is true in that model." Nor have I argued that certain parametric limits of a particular model constitute the only relevant range for policy discussions. If you are going to make blanket characterizations of this sort ("fall victim to ... error") you owe it to your readers and to me to provide a citation or a quote that demonstrates your point.

p. 18: You say that I criticize you by implication for being unaware of nuances or for having suffered amnesia about such nuances. I do not. If you disagree, again please provide a citation or a quote.

p.17: You say that I am mistaken in claiming that there is not a long-run growth effect in traditional theory. Leaving aside again the issue of what is new and old growth theory, the point that the marginal product of capital may be bound away from zero even in the traditional theory is explicitly recognized in the Rodriguez and Rodrik (1999) paper (see fn. 7), citing Srinivasan!

p. 7: You claim that I rely "exclusively" on cross-country regressions in deriving the conclusion about the importance of macro stability and investment. This is a gross distortion of my 1999 ODC book as well as of my work more generally. The conclusion is in fact based on my reading of the East Asian experience (as discussed in the 1995 Economic Policy article on Korea and Taiwan). It also happens to be supported by the cross-national evidence. This is a good example, in my mind, of how cross-national work and country studies can complement each other. Moreover, your "counter-example" of Soviet bloc countries overlooks the fact that throughout the book my focus is on private investment and on strategies for increasing the return to private capital.

p. 31: You make the same mistake on p. 31 where you attribute my views on East Asia to cross-country regressions, whereas in fact they derive from the interpretation of detailed country evidence contained in the NBER country study on Korea among other sources. You dismiss my argument simply by hypothesizing another channel of causation, but you provide no evidence in support of your thesis and no discussion of the evidence that I have brought to bear in support of my own interpretation.

p. 25, fn. 10: You indicate that I seem unaware of the earlier influential OECD and NBER studies. As I have indicated above, I am of course aware of them, cite them, and have used them to construct my own interpretations.

With best wishes,

Dani