TERMS OF TRADE, DOMESTIC DISTRIBUTION AND EXPORT-LED GROWTH*

A Rejoinder to Rejoinders

Graciela CHICHILNISKY

Columbia University, New York, NY 10027, USA

This note responds to rejoinders by Gunning (1984), Srinivasan and Bhagwati (1984), and to a new comment by Findlay (1984), all dealing with my 1981 results on export-led growth [Chichilnisky (1981)] and with my recent response to earlier comments [Chichilnisky (1984)]. The 1981 results on export-led growth are analyzed further, and both the comparative statics and stability propositions are shown to be correct exactly as stated. Errors and misinterpretations in the comments by Gunning, Srinivasan, Bhagwati and Findlay are traced to: (1) the erroneous identification of a cross equilibrium locus with a supply curve, an error akin to the basic identification problem in econometrics; and (2) the erroneous stability analysis which derives from using a cross equilibrium locus as a supply curve. Both of these errors arise from an attempt to understand with partial equilibrium tools what is essentially a general equilibrium phenomenon. It is also shown that all the examples these authors provide confirm the stability and comparative statics results of my 1981 paper. The only difference is in their erroneous interpretation of the adjustment process from one equilibrium to another. What is left after these errors are corrected, is pure rhetoric.

There are several ironical aspects, no doubt unintended, in the rejoinders by Gunning (1984), Srinivasan–Bhagwati (1984), and the new comment by Findlay (1984) on my 1981 results on export-led growth. One is a reaction to what is new, and perhaps perceived as threatening: in Findlay's own words, he attempts to provide 'a complete vindictation of orthodoxy'; Gunning actually titles his comment 'in defense of orthodoxy'. It would seem that only a rather vulnerable orthodoxy would require such an extended defense against one single publication.

In any case, this defensiveness is misplaced. My results, far from being an attack, constitute an attempt to update international trade theory both in terms of the tools used and in terms of the issues selected: the aim of my 1981 paper is to seek conditions under which an expansion of exports has a

beneficial effect. This is a very real policy concern and a challenging theoretical problem as well. Secondly, as shown in the footnote,[1] Bhagwati and Srinivasa's rejoinder contains an error in the analysis of the resolving equation which, when corrected, reverses their conclusions: hence increasing the demand for exports of basics must lead to lower prices.

Yet another aspect of the comments is a failure by these authors to understand certain methodological innovations which were introduced in the 1981 article, and which require a certain amount of familiarity with issues in general equilibrium theory, including stability analysis. I shall therefore aim to correct the errors of these comments, and to clarify their misunderstandings of what is, acknowledgedly, an important policy issue, so that we can move in a positive direction.

There are three major departures in my 1981 article from the standard analysis of gains from trade, all entailing a conceptual leap into modern general equilibrium analysis.

(a) I do not study moves from autarchy to free trade, but rather I compare two competitive free trade equilibria, each having a different level of trade. This is in fact what export-led policies are all about: the relevant question is not whether a country should export or not, but whether it should export more or less. This difference in approach leads to substantial analytical differences. For example, if one were using an Edgeworth-type analysis, one would have to note that changes in the underlying parameters of the model, such as preferences, would alter the indifference surfaces in the box. Furthermore, since factor endowments are in my model price-dependent, the size of the box would itself become price-dependent.

(b) A second departure is the explicit acknowledgement that there are a number of different approaches to the analysis of stability of a general equilibrium model with constant returns to scale in production. These are the models most used in trade theory. There is no need to elaborate on this point, which is explained in detail in Chichilnisky (1981), Chichilnisky (1984), Arrow and Hahn (1971), and Heal and McLeod (1984). This point also leads to substantial analytical differences, as a number of different adjustment processes are possible, each leading to different stable configurations, and to different explanations about how the equilibrium is established.

(c) A third departure deals with the question: How does an economy move from one equilibrium to another? This is not a stability issue, i.e., not the issue of adjustment from disequilibrium to equilibrium. The issue is

---

[1]They assume in analysing my (1984) eq. (26) that 'X_B^D dies not exceed A'. Under the conditions of my (1981) Propositions 1 and 3, the opposite is true. Hence their conclusion must be reversed.

rather a move from one full equilibrium position to another. My answer here is akin to Debreu's analysis of regular economies [Debreu (1970)]: one considers changes from a Walrasian equilibrium to another as the exogenous parameters of the model change. In my 1981 paper these parameters are preferences (since technologies remain unchanged). Comparative statics is therefore the study of changes in the equilibria due to changes in underlying preferences. In Proposition 1 (1981) preference changes lead to an increase in the volume of exports of basics from the South (X_B^S) at the new equilibrium; in Proposition 3 (1981) there is an underlying increase in the equilibrium level of industrial demand in the North, I^D, due to preference changes.

But I go one step further. I am not concerned so much with preferences, as with their observable manifestations, the levels of demand and exports at an equilibrium. I consider changes in X_B^S and I^D *directly*, and observe their general equilibrium implications. This means that underlying changes have occurred in preferences, but whatever these changes are, the results are the same if X_B^S and I^D have increased. This is an important methodological point: my results apply to *any* economy with the production and factor supply relations specified, whatever the underlying preferences, provided these lead to the same set of equilibria as X_B^S and I^D change. Changes in these two numbers account for our parametric description of the set of equilibria. The results are therefore rather strong, since they apply to a very large set of economies, provided they all have the same set of equilibria. A similar point was made by Heal and McLeod (1984). The results which emerge are therefore valid for a wide class of disequilibrium supply and demand specifications provided their intersections, i.e., the set of equilibria, are the same. This explains why despite the commentators' use of different disequilibrium specifications, *they obtain the same comparative statics results as I do. They also obtain the same stability conclusions: the model is Walras stable under the conditions of Chichilnisky (1981), see footnote 12 in Chichilnisky (1984).*

The results are that *whatever the reasons* driving the rise in the equilibrium level of exports from the South, X_B^S, the terms of trade p_B will be lower at the new equilibrium when labor is abundant (α large) and technologies are dual ($c_2/D < 2w/p_B$). The opposite is true if $c_2/D > 2w/p_B$ (i.e., if technologies are more homogeneous): as exports increase, the terms of trade improve for the South. Clearly the domestic characteristics of the South, embodied in these conditions on labor and technology, matter a great deal in deciding whether to export more or less. This is the main result of the 1981 paper, in Propositions 1 and 2. It is important to remark that all the examples given by the commentators confirm these results, as indicated in detail in Chichilnisky (1984). Therefore, *nobody disagrees with the main comparative*

statics results of the 1981 paper. There is also universal agreement on the stability of the model.

The last result, Proposition 3, states that 'if a new world equilibrium with an increased volume of exports by the South is attained due to a positive shift in the demand for basic goods by the North (e.g., higher growth rate by the North) then the terms of trade will worsen for the South and the purchasing power of wages within the South will also decrease. This takes place within a Walrasian stable world economy'. The shift in demand for basics by the North is explained as follows [Chichilnisky (1981, p. 180)]: 'As the (exogenous) demand for investment (industrial) goods $(I^D)^N$ is increased within the North, eq. (21a) implies a positive shift in (the North's) demand for basics, at each price level.' The chain of events is clear: as the driving parameter I^D increases, at the new equilibrium the South exports more $(X_B^S$ increases) and its terms of trade p_B drop. Furthermore, as I^D increases the international demand schedule for basics of the North shifts to the right at each price.

Now, there is no debate in any of the comments about the increase in industrial demand in the North I^D leading to a drop in the price of basics p_B. Neither is there any debate about the negative association of p_B with the volume of exports of basics from the South X_B^S when labor is abundant and technologies are dual. Elementary logic indicates therefore that there can be absolutely no disagreement with the comparative statics results of Proposition 3, which is just a conjunction of two acceptable propositions: I^D increases and p_B drops, p_B drops and X_B^S increases, i.e., I^D increases, p_B drops and X_B^S increases. Yet this is the proposition with which the commentators disagree — in fact, the *only* proposition with which there is any disagreement at all. What went wrong in the minds of these commentators?

The disagreement is in fact rather minor, but their loyalty to their idea of orthodoxy has apparently driven these commentators to blow the matter up completely out of proportion. There is no disagreement whatever with the comparative statics results, i.e., with the changes in equilibrium quantities and prices. Under the conditions I specified in 1981 all commentators acknowledge that the model is stable and that the reaction curve X_B^S is negatively sloped: across equilibrium *prices drop as exports expand.*

Under the same conditions, all commentators agree that *an increase in the equilibrium level of industrial demand in the North, I^D, leads to more exports from the South X_B^S, and to a drop in the South's terms of trade p_B.* The statement by Findlay 'orthodoxy is vindicated' [by the computer simulation of Eduardo-Jose Chichilnisky (1984) which reproduces numerically my Proposition 3] seems to include my results as part of the 'orthodoxy'. Yet they show clearly how an expansion of southern exports even if initiated by an industrial expansion in the North, can have negative consequences: terms of trade drop and real wages drop as exports from the South expand. In Findlay's formulation, *orthodoxy therefore does not endorse export-led*

policies, under my conditions of abundant labor and dual technology when the goal is to raise terms of trade and real wages. We agree: one must exercise great caution when proposing export-led policies under the conditions of my Proposition 1 of 1981.

The only disagreement, which is rather minor in view of the magnitude of what is agreed, is about how an increase in the exogenous parameter I^D leads to lower terms of trade and more exports from the South. Findlay claims (and Gunning, Srinivasan–Bhagwati, and Saavedra follow) that the *increase* in the equilibrium level of exports and drop in their price can only arise from a *decrease* in the North's demand for basics: 'the price of basics can only drop if the demand by the North decreases, because the model is Walrasian stable'. He is actually claiming that the South can export *more* at a new equilibrium, *only* when the demand for these exports *decreases*, which is quite an extraordinary contention. This contention is of course wrong: the price of basics drops as the international demand curve for these exports shifts outwards.

Findlay's claim is based on two completely erroneous beliefs: (1) that an increase in the industrial demand parameter I^D can only lead to a drop in the demand for basics, and (2) that in a Walrasian stable economy a change in an exogenous parameter which leads to a positive shift in demand can only lead to higher prices. Both beliefs are clearly wrong: a change in I^D leads in my model to a *positive* shift in international demand for basics from the North and to a *drop* in the price of basics, the reason for the drop in price being that the supply curve of exports shifts with I^D as well. As can be expected in a general equilibrium model, when an exogenous parameter shifts, everything shifts, not just one curve at a time. Findlay consistently ignores this point and analyzes the general equilibrium world in a model where only one curve shifts at a time. In his partial equilibrium world this leads him to the same stability conclusions and comparative statics results as mine, but prevents him from understanding the correct general equilibrium adjustment process. Fig. 2b in Chichilnisky (1981) and fig. 2 in Chichilnisky 1984 show exactly what went wrong in Findlay's and the other commentators' analyses; they thought that the shift in the parameter I^D could affect nothing else but the demand for basics. Eq. (41) in Chichilnisky (1984) shows how *both* the North's demand and the South's supply shift with I^D.

The disagreement is therefore narrowed down to the statement that 'in a Walrasian stable economy a rightward shift in demand can only lead to an increase in prices'. I claim that this is false in a general equilibrium framework. There is in fact a large literature about sixty years old dealing with an analogous error, now corrected, in industrial organization.[2] The issue was whether prices can drop in a competitive sector when demand expands. This famous debate still makes fascinating reading; see the articles

---

[2] I thank Michael Spence for drawing my attention to this analogy.

by Clapham, Pigou, and Robertson, reprinted in Stigler and Boulding (1952). After a long debate it was agreed than an expansion of demand can indeed lead to lower prices in a perfectly competitive sector with increasing supply curves, and we teach this now in our undergraduate micro courses [see, e.g., Nicholson (1978, p. 332)]. The answer lies, of course, in the changes in supply as demand expands. The negative relation postulated between quantities and prices is a cross equilibrium relation, and does *not* describe a supply curve. Similarly X_B^s in my 1981 paper is explicitly defined to be a cross equilibrium relation and *not* a supply curve. To identify a cross equilibrium relation with a supply curve is to make an error that an econometrician would only make if unaware of the identification problem. This is the source of Findlay's misunderstanding of my model.

It is possible to trace this error further to a misunderstanding of the Walrasian stability analysis of a general equilibrium model with constant returns to scale. Recall that a constant returns to scale economy does not have a unique disequilibrium supply specification, as explained in Chichilnisky (1981, 1984), Arrow and Hahn (1971) and Heal–McLeod (1984). Findlay's error (followed by Gunning and Bhagwati–Srinivasan) is to reconstruct a rather different supply curve for my model, namely X_B^s. This is obtained by postulating zero profits outside of an equilibrium, because the construction of the curve X_B^s uses the price equations $p_B = a_1 w + c_1 r$ and $p_I = a_2 w + c_2 r$, which are precisely zero profit conditions in the market for basics and industrial goods. As explained in Chichilnisky (1984) this assumption of zero profits outside an equilibrium violates both my specification of the supply curve and a traditional definition of disequilibrium supply for constant returns to scale economies of Samuelson (1947), Arrow and Hurwicz (1963) and more recently Mas Colell (1974) and Heal-McLeod (1984): in Chichilnisky (1981, 1984) as in Samuelson, Arrow and Hurwicz, *profits are only zero in equilibrium.* If one erroneously reconstructs the underlying supply curves of my model as Findlay does, so that X_B^s is a disequilibrium supply curve, one obtains the same equilibria, the same comparative statics results, but a different interpretation of the adjustment process. This is why we agree on the comparative statics but not on the adjustment process. But my point is that this is not a reasonable way to reconstruct supply curves in my model, and indeed not the way I defined disequilibrium supply curves in 1981. I produced in 1981 a perfectly valid adjustment process which leads to an upward sloping supply curve when X_B^s is downward sloping, and to Walrasian stability. My supply curves are constructed around each equilibrium by postulating that factor markets are always at an equilibrium, and only commodity markets adjust. In this case, since factor markets always remain clear, there is no tendency for factor prices to move in a Walrasian adjustment process. w and r remain at the equilibrium levels, while the prices of basics p_B adjusts. In particular eqs.

$p_B = a_1 w + c_1 r$ and $p_I = a_2 w + C_2 r$ are *not* satisfied outside of the equilibrium, and therefore profits are only zero at an equilibrium. X_B^S is the locus of the intersections of supply and demand curves, both of which shift with $I^D(N)$ *In my 1981 results an expansion in the equilibrium industrial demand parameter in the North I^D leads to an expansion in the North's demand for basics both in the sense of the quantity demanded in equilibrium and in the sense of the disequilibrium demand schedule*, to a drop in the South's terms of trade, and an expansion of the South's exports of basics. All this happens in a Walrasian stable economy. This is Proposition 3, the final, albeit not the most important, result in the 1981 paper. In Chichilnisky (1984) I extended these results to show that they arise even with constant factor endowments, and gave necessary and sufficient conditions for this case. I showed also that the North may exogenously expand its domestic consumption of basics B^D or of industrial goods I^D and in both cases cause the South to export more basics, at lower prices, and to consume less domestically. I showed that the economies are not just stable but have a unique equilibrium [in Chichilnisky (1981, p. 188) it was stated that there were 'at most two', which is trivially consistent with there being one].

Finally, the results published in 1981 have been submitted to econometric testing and were supported by the data of Sri-Lanka and the U.K. [Chichilnisky et al. (1983)] and they were extended elsewhere to deal with exports of resources rather than labor intensive goods [Chichilnisky (1982)] and of an enclave good which is not consumed domestically [Chichilnisky (1983)].

References

Arrow, K., 1981, Evaluation of the UNITAR Project on Technology, Domestic Distribution and North–South relations (UNITAR, New York) Aug.

Arrow, K. and F. Hahn, 1971, General competitive analysis (Holden Day, San Francisco, CA).

Arrow, K. and L. Hurwicz, 1963, Decentralization and computation in resource allocation, in R.W. Pfouts, ed., Essays in economics and econometrics in honor of Harold Hotelling (University of North Carolina Press, Chapel Hill, NC).

Chichilnisky, G., 1981, Terms of trade and domestic distribution: Export-led growth with abundant labour, Journal of Development Economics 8, no. 2, 163–192.

Chichilnisky, G., 1982, Resources and North–South trade: A macro analysis in open economics, Working paper (University of Essex, Colchester).

Chichilnisky, G., 1983, North–South trade with export enclaves: Export crops vs. food staples, Working paper (Department of Economics, Columbia University, New York).

Chichilnisky, G., 1984, North–South trade and export-led policies, Journal of Development Economics, this issue.

Chichilnisky, G., G. Heal and J. Podivinsky, 1983, Sri-Lanka and the U.K.: A case study in North–South trade, Working paper (Graduate School of Business, Columbia University, New York).

Debreu, G., 1970, Economies with a finite set of equilibria, Econometrica 38, no. 3, 387–392.

Findlay, R., 1984, A comment on 'North–South trade and export-led policies', Journal of Development Economics, this issue.

Gunning, J., 1984, Export-led growth with abundant labor: A defense of orthodoxy, Journal of Development Economics, this issue.

Heal, G. and D. McLeod, 1984, Gains from trade, stability and profits: A comment on Chichilnisky's 'Terms of trade and domestic distribution', Journal of Development Economics, this issue.

Mas Collel, A., 1974, Algunas observaciones sobre la teoria del tatonement de Walras en economías productivas, Anales de Economía.

Nicholson, W., 1978, Microeconomic theory: Basic principles and extensions, 2nd ed. Dryden Press, Hinsdale, IL).

Samuelson, P., 1947, Market mechanism and maximization, in: J. Stiglitz, ed., Collected papers of Paul Samuelson (MIT Press, Cambridge, MA).

Srinivasan, T. and J. Bhagwati, 1984, A rejoinder, Journal of Development Economics, this issue.

Stigler, G.T. and K.E. Boulding, 1952, Readings in price theory (Irwin, Homewood, IL).