

Export Instability and Economic Growth: A Comment

It goes without saying that anyone who ventures to write a review article on a certain subject should be not only thoroughly knowledgeable of that subject and familiar with the relevant bibliography, but also capable of analyzing and classifying the various empirical findings, comparing critically the different theoretical points of view, indicating recent developments and trends and, preferably, providing suggestions for further analysis and research.

Unfortunately, the author of the "Export Instability and Development: A Review of Some Recent Findings," that appeared in this *Review* recently, demonstrated very few, if any, of the above desirable traits.¹ Not only Leslie Stein excluded a significant part of the pertinent bibliography on the subject of export instability and growth in his review article, but also demonstrated a lack of understanding of the basic problems involved in measuring export instability, committed errors in the limited quantitative evidence he used to support some of his claims, made unfounded critical comments and confused certain issues by drawing conclusions based on misquotes of other authors.

The primary objective of the present Comment is to refute Stein's criticism of my article and to set the record straight on some of his misconceptions, especially with regard to the measurement of export instability.

1. Stein expressed skepticism about the reliability of the findings of my study by stating that "some doubts as to the validity of the results may be harboured... since 7 of the 40 countries placed in the LDC sample are not normally so regarded".² As is clearly explained in my article, for the classification of the countries in the sample I used the generally acceptable and objective, though admittedly arbitrary, criterion of per capita income measured in U.S. dollars for the year 1966. Since 1966 was the last year of the period covered in my study,³ the fact that some ten or fifteen years later a few of the countries in the LDC group have grown sufficiently rapidly to

¹ L. STEIN, [13].

² L. STEIN, *op. cit.*, p. 288. It should be clarified here that from the seven countries (Cyprus, Greece, Iceland, Portugal, Spain, Turkey and Yugoslavia) which Stein argues that bias my results, Iceland is placed in my DCs group, not the LDCs as he erroneously stated, and furthermore, Yugoslavia was not used for the estimation of the regression equations, due to the lack of sufficient income data.

³ See C. GLEZAKOS [3], footnote 10, p. 673.

be considered developed, should definitely have no bearing on the validity of the results of my study. Besides, all countries which Stein objects to have been classified as LDCs in my article, are also listed as developing countries by the Commission on International Development in its report *Partners in Development*.⁴ What is astonishing about Stein's criticism however, is the fact that he repeatedly cited the contrary findings and conclusions of the article by Kenen and Voivodas whose sample included not only all the countries he objected to having been in my LDC sample, but also Australia, Finland, New Zealand and South Africa. Yet Stein harboured no doubt whatsoever about the validity of their findings. Stein clearly uses a double standard to attack any conclusions different from his own and to support those in agreement with his position regardless of the relative merits of the methods used and evidence presented.⁵

2. Further, in comparing and evaluating the results of various studies regarding the effects of export instability on economic growth, Stein charged that "Glezakos took considerable pains to ensure that his per capita growth rates were calculated in real terms but there is no indication that in computing his instability indices, real export charges were considered... causing Glezakos' results to be biased."⁶ Since elsewhere I have explained the need for the use of real per capita income data in measuring economic growth, there is no point to repeat the argument here.⁷ The reason for using exports measured in current prices is simply that one of the principal *a-priori* arguments regarding the detrimental effects of export instability on economic growth is that the instability of export proceeds, through the balance of payments constraint, causes instability in the capacity to import. Any attempt to verify or refute such a hypothesis, therefore, should be made with export data expressed in terms of import capacity. Such adjustment could be accomplished, ideally, by deflating with the unit value index of imports for each country.⁸

⁴ See L.B. PEARSON [12], especially Table 7, pp. 368-369. But even more recent studies, like the one by M. Michaely, consider Cyprus, Greece, Portugal, Spain, Turkey and Yugoslavia as LDCs. (See M. MICHAELY [11], Table 1, p. 51). It is also interesting that several other studies quoted by Stein, such as the one by G. ERB and S. SCHIAVO-CAMPO [1], do not include any of the above six countries among their DCs group.

⁵ Incidentally, it is worth mentioning that with one exemption, none of the other studies included in Stein's review article have explicitly stated their criteria regarding country grouping. What is more important, not even one of these studies has used a systematic approach for sample selection. Yet Stein saw nothing questionable about the reliability of their results. (For a detailed and informative analysis of the importance of sample selection and related matters, see D. LIM [9].) A dangerous outcome of Stein's unfounded criticism is that other authors who have casually read his article, parroted its conclusions. A case in point is E. LANCIERI [7], p. 138, especially footnote 13.

⁶ L. STEIN, *op. cit.*, p. 288.

⁷ See C. GLEZAKOS, *op. cit.*, p. 671.

⁸ From the context of his critical statement, it does not seem that Stein himself has understood what is the appropriate adjustment of export revenues. If anything else, he probably considers that export proceeds should be deflated by the unit value index of exports, as it can be easily inferred from his complaint that I did not use "real exports."

In my study I opted for an alternative approach, i.e., the conversion of export proceeds into a common, internationally acceptable currency of relative stable value. This approach was dictated by the lack of unit value indexes of imports for a significant number of countries included in my sample and was facilitated by the fact that the U.S. dollar during the time period under consideration was quite stable. The use of export revenues, converted into U.S. dollars, for estimating the export instability indexes is adequate for the intended purposes and, consequently, Stein's criticism of my approach is out of place.

3. Stein is completely off base dismissing my disapproval of Coppock's instability index stating that "a rank correlation of the two indices for the 33 countries common to both papers, was calculated to be .83 ... which somewhat pre-empts Glezakos' criticism."⁹ To start with, there are 39 countries common to the two studies Klein utilized to estimate his rank correlation coefficient and their actual rank correlation is .756.¹⁰ Furthermore, the fact that the rank correlation coefficient between the two measures of instability is relatively high in a given instance should not be considered as a valid argument against the inappropriateness of Coppock's index as a measure of instability. The observed high rank correlation coefficient simply indicates that for the period under consideration most of the countries involved experienced strong trends rather than cyclical fluctuations in their export revenues. But this is simply coincidental. It is precisely in the cases in which time series of export revenues show pronounced fluctuations that the defects of Coppock's instability index become significant. This is because in such instances the choice of the first and last years of the series has a considerable effect on the value of the index.

Another weakness in Stein's review article is that he failed to stress the importance of the measurement of export instability as far as empirical studies are concerned. Nowhere did he mention the basic principle that the export instability index should not be an arbitrary measure but one that is congruent to the *a-priori* theorizing of the effects of export instability that the particular study attempts to test. If anything else, Stein weakened further the already inadequate "Choice of Index" section of his article when he attempted to show that there are mixed findings and opinions on the importance of the measurement of export instability. In support of this view Stein quoted Kenen and Voivodas' statement that "the method of measuring export instability does not seem to matter much,"¹¹ without recognizing that Kenen and Voivodas were referring to their own three different instability indexes and not to export instability indexes in general, as Stein seemingly implied. Aside from the *a-priori* reasons, however, it should be stressed that there are also empirical studies, such as the one by Lawson quoted in

⁹ L. STEIN, *op. cit.*, p. 281.

¹⁰ See C. GLEZAKOS, *op. cit.*, pp. 677-678, and G. ERB and S. SCHIAVO-CAMPO, *op. cit.*, p. 267.

¹¹ L. STEIN, *op. cit.*, p. 282.

Stein's article, which have demonstrated that when it comes to verifying or refuting various hypotheses regarding export instability, the conclusions "depend on one's definition of instability and hence the index employed."¹²

Overall, Stein missed a golden opportunity for a positive contribution to the export instability issue by failing both to make a systematic analysis and classification of instability indexes and to evaluate their desirable properties.¹³ So shallow is the treatment of export instability measures in Stein's review article that the well known problems associated with the instability indexes using moving averages to correct for the trend are not even mentioned.¹⁴

In passing, it is worth pointing out that the majority of the export instability indexes utilized in past studies, as Yotopoulos and Nugent correctly observed, are mostly designed for measuring the "uncertainty" rather than the "shortfall" of foreign earnings effect.¹⁵ However, one recent study has introduced the concept of expectations in the measurement of export instability.¹⁶ Such index, regardless of any misgivings one might have as far as the theoretical justification of the permanent income hypothesis in the context of LDCs and the utilized estimation method are concerned, is a welcome change from the "mechanical" nature of prior indexes. The inherent problem of "mechanical" export instability indexes is that their estimation is based on retrospective information. Indeed, all instability indexes using trend correction require the estimation of the trend (or growth rate) of the entire time series on exports before its instability can be computed. Since most of the scenarios regarding the deleterious effects of export instability on growth involve expectations of producers and/or planners about future export earnings, there is an obvious contradiction between the assumptions under which "mechanical" instability indexes are constructed and the hypotheses tested concerning the effects of export instability. This is why the employment of appropriate expectations models for the measurement of export instability is so important.

Finally, from the various desirable properties that have been suggested for the export instability indexes,¹⁷ the one that stands out in terms of importance is the independence of the instability measure from the size of the trend (or growth rate) of the series whose instability is estimated. Most studies have attempted to show (or at least have assumed) that the principal mechanism through which export instability affects economic growth is by its impact on the growth of the export sector. Therefore, if the measure

¹² J.C. LEITH [8], p. 286.

¹³ An adequate review article should have included such a section, not only because of its intrinsic importance for empirical studies, but also because there exists relevant, though not widely known, literature on this subject. See e.g., K. KUBOTA [6], and C. GLEZAKOS [2].

¹⁴ See O. KNUDSEN and A. PARNES [5] for a concise discussion of these problems (p. 11).

¹⁵ P. YOTOPOULOS and J. NUGENT [14], p. 330.

¹⁶ O. KNUDSEN and A. PARNES, *op. cit.*, Ch. 7, ff.

¹⁷ For an extensive discussion of these properties see K. KUBOTA and C. GLEZAKOS, *op. cit.*, pp. 14-19 and pp. 24-26 respectively.

of export instability is not independent of the growth rate of exports, any test of the above hypothesis would be unreliable. This is exactly the basic problem with the results of the Knudsen and Parnes study which reveal that export "instability has a positive relation with economic growth."¹⁸ But as has been accurately pointed out, "the transitory component of the export income will tend to be (positively) correlated with the rate of growth of exports."¹⁹ Therefore, the positive association between export instability and economic growth found by Knudsen and Parnes is a spurious one attributable basically to the positive relationship between their export instability index and the growth of exports, on the one hand, and that of the latter with the growth of income, on the other.

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¹⁸ O. KNUDSEN and A. PARNES, *op. cit.*, p. 124.

¹⁹ A. MACBEAN [10], p. 119.