A Kaldorian Approach to Greek Economic Growth: A Reply

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The author of the comment makes three general points in the beginning and then proceeds to state his or her own econometric findings. In our reply we take each Law separately dealing first with the theoretical points and then with the empirical ones.

First Law

The author feels there is a problem with the definition of the industry and particularly with the inclusion of the construction sector. The definition of the industry to include construction is an established practice followed by a great number of theorists and by students of the Greek economy (see for instance, Agapitos,1989,p.85). Moreover, studies which have tested the behaviour of the construction sector in the context of a Kaldorian model found that its behaviour is very similar to the industrial or manufacturing sector (see for example, McCombie and DeRidder,1983, pp.376-383). This implies that the construction sector is unlikely to have distorting effects. It is also argued that the construction sector is an overgrown sector in Greece. Although it is difficult to see the real significance of this point, it might be useful to mention that the construction sector had lower rates of growth than the manufacturing sector and also lower than the energy sector (electricity, gas and water; see Agapitos,1989,pp.76-88).

Furthermore, both manufacturing and industrial output are used in the paper. The reason for that was to accommodate some studies which used one or the other or even both of the terms. In addition, a number of commentators explicitly state that the choice of the term is not important (e.g Thirwall,1983). In any case, the results of the two separate formulations are very similar. This implies that even if we exclude construction the validity of the model is not affected.

Finally, the author asserts that the share of manufacturing industry in GDP has dropped in 1988. This is also reflected in our table on p. 1683 (Drakopoulos and Theodossiou,1991) and it is hardly surprising since in the last decade there has been a general tendency of de-industrialization in a number of countries (Gershuny and Miles,1983).

The author of the comment divides the empirical points concerning the first law into two parts. Our reply to a) and b) is the following: a) Since our own results indicated no first order autocorrelation (table 3, p.1686), there in no justification to use Autoregressive Least Squares (AR1). The author does not explain why he or she uses AR1 here. Also there are no D.W statistics and this is an important omission. b) The author uses a dummy variable to take into account the alleged structural instability of the model. Using our estimation and data, the Chow F test rejected at 5% significance the null hypothesis of structural instability. This implies that it is not appropriate to use dummy variables in this case. Furthermore, the author justifies the inclusion of dummy variables in terms of recursive residuals. However, according to econometric theory, recursive residuals may not be a reliable method to

detect instability when the samples are relatively small -as in our case with 22 observations- (see also Spanos,1986).

A final point here is that in order to present the existing equations 2 and 3, Chow F test on number 1 is necessary.

Second Law

The second general point according to the author, concerns "certain underlying conditions of the Kaldorian model which are crucial for the model". It is argued that these conditions did not hold for the Greek economy. The most important of these "conditions" is the strong demand for labour in manufacturing. We believe that the author has misunderstood Kaldor's second Law since he or she implies that demand for labour in manufacturing is endogenous. Quite to the contrary, Kaldor clearly saw demand for labour in manufacturing (or employment in manufacturing) as exogenous (Kaldor, 1966, Thirwall,1983,pp.352). In particular, Kaldor thought that export demand was partially responsible for the growth of manufacturing output which in turn determines the demand for labour in manufacturing (Stoneman,1979,p.314; Mizuno and Ghosh,p.11). Although there is no universal agreement on the previous points, our paper followed the established approach. Also, our empirical results involving manufacturing employment, manufacturing output and growth of exports, support the Kaldorian line of argument.

The third point makes some criticism on the formulation of the second law. The author states that while we "apply the first specification (regression of output on productivity) to manufacturing and the second specification (output on employment) to agriculture, [we] do not present both specifications for any particular economic sector." Actually there are a number of views about the appropriate specification of the second law including the view that productivity

on manufacturing should be regressed on manufacturing employment (Rowthorn,1975). However, our formulation (regression of productivity on output) is consistent with the point made in the previous paragraph, and is a very common one used by many specialists (Stoneman,1979; Thirwall,1983; Mizouno and Ghosh,1984;). Moreover, we thought that this specification is more appropriate for the Greek economy since the second one involves manufacturing employment, and as was stated on pages 1683-1684, this concept is problematic because of the role of family business and non-recorded employment. Our result concerning the second Law, was also reinforced by the testing for the presence of Okun's law by using capacity utilization procedure. The regressions on page 1687, clearly support the relevance of the second law.

Another line of criticism of our formulation has to do with "the low employment growth in manufacturing and the measurement of productivity as above [p = q - e], results in that the regression of output on productivity growth is really not very different from regressing output growth on itself". The first observation here is that the above productivity equation is almost universal in the literature on Kaldorian approach (for a review see Thirwall,1983). Second, the criticism implies that in order for q to be equal to p, e (the growth of employment in manufacturing) should be equal to zero. However, if we look at the author's figures on employment growth (p.2) we can see that it varies from -2.4% to +0.2%. Even these indicative figures signify that the p will certainly be different than q. In addition, an estimation of productivity from another source gave similar results.

As far as the authors' empirical results of the second Law are concerned, we note the following: The results of the first formulation of the second Law as

presented by the author is very similar to ours. As far as the second formulation is concerned, our previous points should be adequate.

Third Law

The author also presents some empirical results of the third law. In particular, it is argued that one of the formulations of the third Law -the regression of output on manufacturing employment - provides better results than ours. However, the recorded D.W statistics imply first order autocorrelation and it must be corrected before any point can be made.

In the regression of output on services equation, no DW is reported and consequently we cannot comment whether it is appropriately estimated. Furthermore, since T is used as a dummy variable for structural change, the author should first have used Chow F test for structural stability of the original equation (without the dummy variable).

Before we close this reply, it should be useful to make two important general observations concerning the empirical part of the comment. First there is no reference to data sources. Second the author should report both OLS and AR1 estimations with all the appropriate statistics. Also all equations should be numbered consequently. The existing reporting is quite erratic.

Finally, in view of the above, the author's use of the term "dangerous" to refer to our policy suggestions, is at best overemphatic.

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