Sustainable development: a comment

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Those of us who work in the field of development do welcome with open arms the August 2003 symposium of *Estudos de Economia Aplicada*, dedicated to *sustainable development*. As Antonio Pulido (2003) indicates, the concept poses a central challenge for the profession. In this comment I discuss two issues. The first one is conceptual and refers to whether the notion of *sustainable development* is still relevant. The second one is methodological, with very important implications for empirical work.

The World Development Report for 2003 is titled *Sustainable Development in a Dynamic World: Transforming Institutions, Growth, and Quality of Life*. Already in 1992, the World Development Report had been dedicated to the same topic: *Development and the Environment*. This confirms the significance of the theme. However, during the last few years, the idea of *sustainable development* has lost some ground, not necessarily because it has become irrelevant, but because first, it has been superseded by or incorporated into another notion; and second, because it has been made part of a series of objectives. The other notion is the concept of *pro-poor growth*. This is an idea that development economists, and in particular multilateral organizations such as the World Bank and the Asian Development Bank, began using a few years ago. It suffers, however, from the same problem the concept of *sustainable development* does, namely, that although it is a very appealing term in populist circles, there is no consensus concerning its definition (Asian Development Review 2000; Cord et al. 2003; Quibria 2003). When a concept cannot be defined properly, it becomes useless for purposes of serious economic policy analysis. In fact, Pulido resorts in section 2 of his paper to discussing the concept that we all, as economists, use, namely, *plain* growth.

The origins of the interest in *pro-poor growth* lie in the attempt to understand the empirical significance and factors behind the variable impact of growth on poverty reduction. *Pro-poor growth* has been broadly defined by a number of international organizations as growth that leads to significant reductions in poverty. Then the questions that arise are: what is a significant reduction in poverty?; or, how much must the poor benefit for growth to be considered pro-poor? In this respect, two definitions of *pro-poor growth* have emerged. First, growth is pro-poor when the

poor benefit disproportionately from it. In a strict sense, this implies that the rate of income growth of the poor exceeds the rate of income growth of the non-poor. The second definition is that growth is pro-poor if it reduces poverty.

The other reason that has made the notion of *sustainable development* lose ground in recent years has been its incorporation into the *Millennium Development Goals*, an ambitious agenda for reducing poverty agreed at the United Nations *Millennium Summit* in September 2000, with specific targets for 2015. These are as follows (http://www.adb.org/MDGs/default.asp):

1. Eradicate extreme poverty and hunger. The target is to halve the proportion of people living on less than a dollar a day and those who suffer form hunger.

2. Achieve universal primary education. The target is to ensure than all boys and girls complete primary school.

3. Promote gender equality and empower women. The target for 2005 is to eliminate gender disparities in primary and secondary education and at all levels for 2015.

4. Reduce child mortality. The target is to reduce by two thirds the mortality rate among children under five.

5. Improve maternal health. The target is to reduce by three-quarters the ratio of women dying in childbirth.

6. Combat HIV/AIDS, malaria and other diseases. The target is to halt and begin to reverse the spread of HIV/AIDS and the incidence of malaria and other major diseases.

7. Ensure environmental sustainability. The target is to integrate the principles of *sustainable development* into country policies and programs and reverse the loss of environmental resources. More specifically, the target for 2015 is to reduce by half the proportion of people without access to safe drinking water. And by 2020, to achieve significant improvement in the lives of at least 100 million slum dwellers.

8. Develop a global partnership for development. The target is, first, to develop further an open trading and financial system that includes a commitment to good governance, development and poverty reduction –nationally and internationally. Second, to address the least developed countries' special needs, and the special needs of landlocked and small-island developing states. Third, to deal comprehensively with developing countries' debt problems. Fourth, to develop decent and productive work for youth. Fifth, in cooperation with the private sector, make available the benefits of new technologies –especially information and communication technologies.

Overall, the problem with the concepts of *sustainable development* and *pro-poor growth*, and with the *Millennium Development Goals* is that as long as they are not made operational for multilateral development organizations to assess the progress of developing countries, they will be no more than statements of good intentions with a thin surface. In a recent article, Gaiha (2003) argues that while the *Millennium Development Goals* are "useful in drawing attention to pervasive deprivation in developing countries, and to the need for a determined and coordinated effort by the

development community in reducing it substantially in the non-too-distant future", their feasibility is not plausible. This is because the sustainability of the required growth rates to achieve the targets, though similar to those developing countries have achieved during the last few years, is not obvious. And moreover, "as income inequality has increased in recent years, the poverty reduction due to a given growth rate is lower."

The second point I would like to discuss is, as indicated above, methodological. Certainly, as Pulido argues, the notion of *sustainable development* is related to the ultimate causes of growth. I have no disagreement whatsoever here. Where we depart is in the usefulness of the neoclassical growth model in all its variants, i.e., basic Solow model and endogenous growth models, in which Pulido frames the central question, and implicitly the answer, of development economics: why are some countries richer than others? My contention is that such framework is inadequate to study questions of growth and development. The reason is simply that the neoclassical model depends on an untenable assumption. This is that an aggregate production function exists. Since my point is much more than mere rhetoric, let me briefly elaborate upon it to build my case via three related issues.

One is the Cambridge debates of the 1960s, which showed how damaging the conclusions of this debate were for the theoretical foundation of the aggregate production function and the notion of aggregate capital. This was a point conceded by Samuelson (1966). Cohen and Harcourt have reminded the profession of the importance of these results in a recent article in the prestigious *Journal of Economic Perspectives* (2003). A detailed discussion of why these results have been ignored in the orthodox literature is beyond the scope of this comment. Felipe and Fisher (2003) discuss the issue. Suffice to say that L. Pasinetti, one of the economists involved in the debates, was compelled in 1994 to remind the participants at a conference on economic growth that:

> "This result [that there is no unambiguous relationship between the rate of profit and the capital-labour ratio], however uncomfortable it may be for orthodox theory, still stands. Surprisingly, it is not mentioned. In almost all 'new growth theory' models, a neoclassical production function, which by itself implies a monotonic inverse relationship between the rate of profits and quantity of capital per man, is simply *assumed*" (emphasis in the original).

In the same vein, Sylos-Labini, in a clear reference to the efforts dedicated by modern economists to the development of neoclassical endogenous growth models, recently wrote:

> It is worth recalling these criticisms [Cambridge debates], since an increasing number of young and talented economists do not know them, or do not take

them seriously, and continue to work out variants of the aggregate production function and include, in addition to technical progress, other phenomena, for example, human capital" (Sylos Labini 1995, p.487).

This is a point that Sylos-Labini (2001) has brought up again in a worth-reading book that collects a series of lectures on development. The new neoclassical endogenous growth models being used to study development questions today suffer from the same problems discussed forty years ago.

Secondly, parallel to the Cambridge debates, there was another body of literature with devastating results for the notion of aggregate production function, namely the aggregation literature, recently summarized and discussed by Felipe and Fisher (2003). It was proven conclusively that the conditions to derive an aggregate production function with neoclassical properties are so stringent that one cannot believe that actual economies satisfy them. For all practical purposes, aggregate production functions do not exist. This simply means that there is no such a thing as the technology of a country in the form Y=F(K,L), where Y, K and L are aggregate output, capital and labor, respectively; and F(·) has the standard neoclassical properties.

Third and final, with the background of the Cambridge debates and the aggregation literature, the logical question to ask is why the aggregate production function is still widely used in neoclassical macroeconomics. Felipe and Fisher (2003) and Felipe and McCombie (see references) have argued that the only reason why aggregate production functions are used by the profession is that they appear to work, at least sometimes, when estimated econometrically. For example, the estimated elasticities are relative close the actual factor shares in the National Accounts (although see Sylos-Labini 1995 for a summary of results). This argument (that they continue being used because they work) is, however, methodologically indefensible.

The problem with the instrumentalist defense of the neoclassical production function on the above grounds is that an "equivalent expression" may be derived from an income identity that makes no assumptions at all about technology or an underlying aggregate production function, which in all probability does not exist. The critique is straightforward. Define value added for an industry as the sum of the wage bill, *W*, and total profits, *P*, so that $V_t \equiv W_t + P_t \equiv w_t L_t + r_t K_t$, where *V, w, L, r,* and *K* are constant-price value added, the real wage rate, total employment, the observed rate of return or rate of profit (not the user cost of capital) and the constant price value of the capital stock. Assume each firm sets prices by a constant mark-up on unit labor costs so that factor shares are constant. Totally differentiating the identity with respect to time and then integrating and taking antilogarithms gives $V_t = B \, w_t^a \, r_t^{1-a} \, L_t^a \, K_t^{1-a}$ where *B* is the exponential of the constant of integration and *a* is the share of labor in value added (i.e., $a = w_t L_t / V_t$).

If one further assumes that the real wage and profit rates grow at constant rates, denoted by λ_1 and λ_2 per annum, respectively, then the approximation to the identity will be given by $V_t = B\,e^{\lambda t}\,L_t^a\,K_t^{1\text{-}a}$, where $\lambda = a\lambda_1 + (1-a)\lambda_2$. In other words, it gives a form that resembles the Cobb-Douglas "production function," where the output elasticities are *definitionally* equal to the factor shares. Remember, however, that in deriving this expression, no assumption has been made about the underlying technology or the state of competition. Therefore, this expression is not a technological relationship but the identity rewritten under these assumptions.

If the assumptions of constant mark-ups, constant growth in real wage and profit rates happen to be correct in the economy in question, estimation of the putative aggregate production function in the form $V_t = B\,e^{\gamma t}\,L_t^{\alpha}\,K_t^{\beta}$ will give a misleadingly close statistical fit (potentially one) because it is an approximation to the underlying income identity. It will also erroneously suggest that the output elasticities are equal to the relevant factor shares, i.e., $\alpha=a$, $\beta=1\text{-}a$ (and $\gamma = \lambda$), hence "confirming" the neoclassical theory of factor pricing, and that constant returns to scale prevail. If, on the other hand, these assumptions do not hold exactly, then the estimates could suggest, for example, increasing returns, but only because of the biases involved. And the less correct the assumptions are, the worse the fit and the results will be. Since factor shares tend to be relatively constant in most econometric applications, what makes often the standard Cobb-Douglas form fail to the point that the point estimate of *K* may turn out negative, is the assumption that wage and profit rates grow at constant rates. Let me give you a hint: try a complex function (Felipe and McCombie 2002a, 2003a). What is the implication? The neoclassical model is untestable because it cannot be refuted statistically.

More general "production functions", such as the CES or translog, can be equally derived as transformations of the identity by making other assumptions about the data (Felipe and McCombie 2001, 2003a). And likewise, all production functions hypothesized in the endogenous growth literature can be derived as transformations of the accounting identity. We have evaluated in the light of these arguments some of the current discussions in macroeconomics, such as the existence of increasing returns in the context of the endogenous growth models, the procyclicality of Solow's residual, or the existence of market power (Felipe 2001a; Felipe and McCombie 2002b); the debate about the role of infrastructure in productivity growth (Felipe 2001b); and the difficulties in testing Solow's model (Felipe and McCombie 2003b). In all cases we have shown the futility of the neoclassical apparatus. Growth accounting exercises suffer from the same problem, namely they are straightforward applications of the accounting identity and thus, while correct, are tautological.

I conclude by saying that one of the reasons why still today we know very little about the process of economic growth, as recently argued by Kenny and Williams

(2001) is, precisely, the dominance of the neoclassical model and the manner in which problems of growth, technical progress and development appear formulated in it. In particular, the research program undertaken by a group of economists during the last decade using cross-country regressions has been seriously criticized and led the profession not very far in its understanding of why some countries grow faster than others (see Felipe and McCombie 2003b for an overview and discussion). I am convinced that more can be learned from detailed case studies than from cross-country regressions with weak data. Fortunately, some economists are making progress in terms of proposing alternative and more fruitful research avenues (e.g., Scott 1989; McCombie and Thirlwall 1994; Rodrik 1999, 2003a, 2003b; Sylos-Labini 2001).

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