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## 'Development studies, a progressive research tradition?' Comments

A recent paper (Parayil 1990) published in *Science Studies* (2/90) raises questions that are of import to science studies and development studies. "Development studies, a progressive research tradition" breaks new ground in that it applies criteria from philosophy of science to evaluate a field of inquiry (development studies) which hitherto has received little attention from science studies in general. By arguing that the theoretical contributions of development studies are more progressive than those of development economics, Parayil's paper also converges on a point made by several other critiques, which is that the orthodoxy of the received view of development research and planning has been refuted (Crocker 1991; Harding 1991).

The paper identifies two research traditions within the broad field of development studies: the first which the author names 'development studies' is based on neo-classical methods and the second 'development studies' is an interdisciplinary field of inquiry. It is the theoretical contributions of the latter which Parayil uses Laudan's theory of re-

search traditions to reconstruct and evaluate. The main theses of the paper are that: (a) the theoretical contributions of development studies constitutes a autonomous research tradition distinct from development economics; (b) development studies is a progressive research tradition with a core commitment to the investigation of development studies from the perspective of the people and their institutions (Parayil 1990: 54); and (c) "the evolution of development studies vindicates the claim that a vertical or unit approach through a uniform notion of change and progress is he wrong methodology for analyzing the intellectual history of interdisciplinary fields of inquiry. A horizontal, cross-disciplinary and integrative approach to intellectual history provides a better basis for understanding change in such interdisciplinary fields as development studies (Parayil 1990: 55). While Parayil's claim that development studies is a progressive research tradition is one with which I concur, there are some flaws in his reasoning which seriously undermine his argument. In the following paragraphs I shall attempt to elucidate these problems.

## Laudan's Problem-solving Criteria and Development Studies

Parayil's main thesis, i.e. that development studies is a progressive research tradition, rests on showing that its problem-solving capabilities are greater than those of its rival tradition, development economics. In order to do this, he relies on some concepts which are central to Laudan's theory of scientific change (Laudan 1977). The first of these is the notion of a 'research tradition'. According to Laudan a research tradition is:

a set of general assumptions about the entities and processes in a domain of study, and about the approaches and methods to be used for investigating the problems and constructing the theories in that domain. (Laudan 1977: 81)

Laudan argues – and Parayil's approach implies that he concurs – that the content of the individual theories of a research tradition do not matter in the process of appraisal. Arguing from this perspective alone, Parayil's reconstruction of development studies may at first appear legitimate since he does not focus on the content of its theories. There is a problem here, however. Even at the level of generalization required by Laudan, Parayil appears to be a bit fuzzy on the details of what constitutes development studies. He tells us that: (i) all participants in this field of inquiry are bound by the understanding that development studies is an inter-disciplinary inquiry in which the views of all participating disciplines are respected (Parayil 1990: 53) and (ii) the core commitment of development studies is to the investigation of development studies from the perspective of the people and their institutions (Parayil 1990: 54).

It is doubtful whether readers unfamiliar with development studies would be able to grasp even the contours of the field from such a description. For example, what does Parayil mean when he states that development studies studies the people and their institutions? Can one conclude from this that development studies is some kind of anthro-

pology? Is it limited only to the study of informal institutions such as community based groups or non-governmental organizations? For those of us who are familiar with development studies, this description seems unnecessarily vague and a rather shaky foundation from which to start building a claim that it is a progressive research tradition.

The second concept which is pivotal to Parayil's argument is the notion of 'problem-solving effectiveness'. Returning to the source, Laudan claims that the:

problem-solving effectiveness of a theory depends on the balance it strikes between its solved problems and its unresolved problems. (Laudan 1977: 71)

Here Parayil's evaluation runs into two sets of problems which we will deal with separately. The first is inherited, Laudan treats the issue of what counts as a 'solved problem' as unproblematic. This means that his methodology provides no criteria for determining when a given scientific problem is solved. The gravity of this lacuna becomes obvious when one considers that even in mature research traditions scientists often disagree on whether a given problem is solved (a fact which Laudan concedes). In nascent research traditions such disputes can be intense and the arguments advanced by different sides of the dispute are often equally persuasive. With development studies one can find several instances of disagreements of this type. One example is the different meanings attached by different theoreticians to the achievements of the newly industrialised countries for the development prospects of other Third World countries. Scholars from the world system analysis perspective in development studies such as Amin argue that:

In our view, the so called newly industrialised countries are the real periphery of today and tomorrow, while the other – 'de-linked by default' – are passively undergoing the fate of the 'fourth world' as it is called nowadays. (Amin 1990: 52)

Theorists from the modernization tradition

(Balassa 1981) however regard the newly industrialised countries as proof that the real obstacles to development in the Third World are internal to the countries, while still another group remain undecided as to the import of the newly industrialised countries for the status of existing theories (see Browett 1985 for a review of the different positions). In view of the above, one would have to argue that showing that a given problem is solved in development studies would require one to give some indication that practitioners have reached some kind of consensus as to the nature of a good solution to the problem in question. Parayil however follows Laudan's lead in ignoring this issue. Laudan's use of mature research traditions for examples in *Progress and its Problems* meant that he was able to rely on history to guide him. However, Parayil is not so fortunate. Our example above, and other disputes among those involved in research on economic development, indicate that the assumptions about the nature of reality held by the respective research traditions necessarily lead them to develop different criteria for what would be an adequate solution to a given problem.

The second set of problems are entirely of Parayil's own making. Although this is not explicated in his paper Parayil appears to be operationalizing 'problem-solving effectiveness' as: (a) a comparison of the fit between the theoretical description provided by the research traditions in question and the reality of their empirical referents and (b) the success of the problem solutions of the respective research traditions once translated into praxis. His evaluation of Lewis's theory vis-a-vis development studies is a good example. Parayil states that:

Lewis postulates that the traditional sector (which is mainly the agricultural sector) can supply a potentially unlimited supply of workers to the modern sector at depressed wages. Lewis assumes that the marginal productivity of the worker in the agricultural or traditional sector is low or even negative compared to the mod-

ern sector.... But the reality of Third World countries proved the "unlimited supplies" theory incorrect.... In the Third World, the massive rural migration to the urban modern sector did not create more employment and rapid industrialisation. Instead such migrations resulted in urban ghettos in Third World urban centers.... (Parayil 1990: 53)

From the above, the paper concludes that "the lack of problem-solving effectiveness of Lewis's theory is more than apparent" (Parayil 1990: 53). On development studies he states that:

The new approaches introduced by many of the practitioners take into account the factors that Lewis failed to include in his model. Modern development thinking largely, takes an "informal" approach to analyzing development problems rather than using rigid formalized and mostly quantitative models. (Parayil 1990: 53)

There is however no evidence provided in the paper to substantiate this claim of superior problem-solving effectiveness for development studies on the grounds of its descriptive power. To what new approaches is Parayil referring? What is the relationship of these new approaches to Lewis's model, do they build upon his hypothesis or are they autonomous? These data are important if the reader is to know whether Parayil is really discussing theories from different research traditions as he asserts or whether we are evaluating the improvements arising out of the normal scientific activity within the same research tradition.

On the relative performance of development studies and development economics in practice, Parayil's appraisal is non-uniform. By this I refer to the fact that Parayil clearly indicates that the failure of the policy prescriptions derived from Lewis's theory is an indictment of the theory itself. However, on the performance of the policy prescriptions deduced from the development studies the reader is only told that they took into account factors which Lewis's theory did not.

Thus one is left in suspense on questions such as: 'did the new approaches recommend/lead to policies which slowed the pace of rural-urban migration?' Parayil seems to be relying solely on the pre-analytic intuitions and casual observations of development studies scholars to evidence the claim that the theories of development studies are superior in this regard. One possible reason for this is that although many Third World practitioners of development studies intuitively believe that the models of development studies are superior to those of development economics, policy-makers rarely implement them. Lewis's model and other models based on the much villified neo-classical mode of thinking have been more influential in practice. Thus, the practice criterion of evaluation cannot be applied to appraise the problem-solving effectiveness of many of the new approaches.

One may argue that this is a minor point since one could not very well expect an evaluation of the effect of development studies on praxis if there has been so little acceptance within the policy arena. However, if we return to the objectives of the field, it becomes apparent that this is a very significant issue. Parayil cites Griffin who states that: "the enterprise of development is as much about changing the world as understanding it." (Parayil 1990: 54) Implicit in the above is the understanding that the main intent of development studies is to develop theories and methods that would assist in the creation of policy directives that would bring about change in Third World societies. Put differently, theory was to be a route to praxis. While there have been many theories in development studies, one has to divorce them from the praxis criterion of evaluation if one wants to conclude that development studies is a progressive research tradition.

This brings us to a central theoretical problem which Parayil missed the opportunity to explore. If the claim of superior problem-solving effectiveness is really valid for development studies, how does one explain the bias towards development economics in

praxis? Investigating this question is not only interesting because of its import for development studies but it would also allow for some reflexion on the model of evaluation itself.

As intimated by Parayil's quote from Griffin discussed above, development studies is a field where social and other situational contexts play a very important role in the development and choice of theory. One has only to take a brief survey of the debates in the field to evidence this argument. For those unfamiliar with development studies we shall digress to provide a few instances. Scholars of development studies such as Prebisch, Gunder-Frank and others have consistently demonstrated both empirically and theoretically that the preference in praxis for neo-classical models is an outcome of situational factors such as the dependent position of Third World countries vis-a-vis their First World counterparts (Gunder-Frank 1981). The importance of situational factors in theory choice is evidenced also in the underlying reasons behind the differing stances adopted by North American and Third World dependency theorists on the issue of subjecting dependency hypotheses to quantitative tests (Ragin 1985). Studies of development institutions such as the World Bank also bear out the hypotheses of Frank and others about the role of situational factors in cognition in development studies (Payer 1974; 1982). Recent work in science studies by feminist and other perspectives within theory of science advance a similar hypothesis about the role of social and other situational factors in scientific cognition generally (Longino 1990; Harding 1991). It is therefore ironic that Parayil's reconstruction of development studies appears to be blind to a theoretical insight on which both science studies and development studies converge.

Perhaps in recognition of the fact that development studies does not conform completely to Laudan's description of a research tradition, Parayil makes a case that it does Laudan's other category, i.e. non-standard

research traditions. in evidencing this Parayil claims that:

Laudan insist that "non-standard" research traditions do have ontologies and methodologies. Other than offering a caveat "much research" is needed, Laudan does not shed further light on "non-standard" research traditions. (Parayil 1990: 49)

Here Parayil inadvertently misreads Laudan since what the latter does say on this subject is that:

Whether, on further investigation, it will turn out that these "non-standard" research traditions do have ontological and methodological elements or whether, failing that, they will behave differently from richer research traditions are still unanswered questions. (Laudan 1977: 106)

One can hardly characterise this argument as an instance on the ontological and methodological elements of "non-standard" research traditions.

Further to the above, anyone familiar with development studies would have difficulty in understanding why Parayil felt it necessary to characterise its theoretical contributions as a "non-standard" research tradition. This brings us back to an issue mentioned earlier, i.e. the way in which Parayil defines his field of development studies for the purposes of reconstruction. As mentioned earlier it can hardly do justice to the field. Moreover it inadvertently misleads the reader. By pitting development studies against development economics Parayil implies a simple dichotomy between an interdisciplinary field of inquiry (development studies) and a unitary disciplinary focus. But is this reality?

For all intents and purposes development economics is part of the field of development studies. Parayil himself provides some evidence for this when he claims that a defining characteristic of development studies is that it respects all disciplines. Even if we were to interpret development studies to be only that body of economic research on development guided by neo-classical orthodoxy, as Parayil does, the comparison is a

bit unfair since this body of work belongs to a much broader and interdisciplinary research tradition on development known as the modernisation paradigm.

Perhaps a more fruitful comparison, and certainly a more faithful description, of development studies would have been achieved if Parayil had focused on modernisation theory as an exemplar of the orthodoxy or received view and dependency theory as an instance of the new development studies. One example will suffice to illustrate and explain. A critical assumption of modernisation theory for instance is that the pattern of historical development of the Third World will be identical to that of the so-called industrialised countries. From this assumption, the return to neo-classical orthodoxy of development economics is not only understandable but may be regarded as a progressive rather than a degenerative shift as Parayil implies. In order to argue otherwise, as Parayil seems to want to do, one should have focused on the situational factors which determined cognition in the research traditions under study.

This brings us to my final point which is that Laudan's model of scientific progress is an inappropriate tool of evaluation for this field of inquiry. I shall posit two reasons for suggesting this. The first is that Laudan's model is premised on the assumption that 'rationally held beliefs' are autonomous from social and emotional contexts. This implies that his model would be unable to provide insight into cases where:

- (1) actor's beliefs are both situationally and cognitively rational, where the situational context is a necessary condition for the cognitively rational processes to occur, and (2) where cognition 'rationalizes' an emotional or situational state or where an actor transforms a situationally or emotionally rational goal into a cognitive one. (Westman 1978: 2)

As shown above, evidence from development studies suggests that it is a field in which the above two factors are important sources of cognition.

Secondly, Laudan's definition of 'problem-solving effectiveness', taken together with the fact that the examples cited in *Progress and its Problems* come predominantly from mature traditions as the Copernican theory etc., suggest that this model is designed to evaluate mature research traditions. This bias toward mature research traditions is more pronounced when one considers that Laudan provides no criteria for the methodologist to evaluate whether a given problem set by a research tradition is 'solved'. This leaves the methodologist with the evaluations of the involved scientists or historical hindsight as the only guides. This lacuna is not unique to Laudan's model, it is also a problem with the methodologies of Kuhn (1970) and Lakatos (1970). Interestingly, Laudan advances his methodology as an improvement on these two predecessors.

### **Towards the Implementation of Development Studies in Praxis**

The field of inquiry which has as its main objective the development of a theoretical basis for policies to ameliorate the conditions of the Third World is once again at a crossroads. Practitioners are being given another opportunity to undermine the bias towards the received view. By concluding that there is progress in development studies without adding the important caveat of its disengagement from praxis, Parayil leaves us with some unsatisfactory implications about policy and metatheory in development studies. Notable among these are: (i) if development studies has been a progressive research tradition, how is it that many developing countries are now in a worse position with regard to social and economic indicators than thirty years earlier? (ii) if the problem-solving effectiveness of development studies is superior, then the bias towards the received view among policy-makers is irrational because a research programme that has in fact been degenerative has triumphed over its more progressive counterpart.

Parayil's contribution is nevertheless an

important one if only for the fact that it brings to the foreground an issue which has been on the backburner of development studies since the introduction of dependency theory. This is, how can the theory of development studies exert more influence on praxis? One step towards finding the answer to this question is that researchers recognize that the development of theory in the field is not an end in itself, but one part of a two-pronged strategy. The second part of this strategy would be the education of the addressees of development studies theory, i.e. policy-makers and the public in general. An initial step towards realisation of this second task would be for researchers in development studies to forge alliances with actors involved in praxis. Possible coalition partners include non-governmental organisations in developing countries. Such coalitions would be of benefit to both parties. For academicians, it would provide the basis for a linkage between development theory and praxis by introducing alternatives to the received orthodoxy in the political discourse. For non-governmental organisations in developing countries, it would provide a base of resource personnel who would help in generation and interpreting scientific research on relevant policy issues.

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