The Use of Innovation Systems in a Technology Development Aid Programme

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1. Introduction

The meeting has been called to assist in identifying the ways forward in advancing thinking and theory on Innovation Systems (IS) and building research capacity for influencing policy and practice. This short paper focuses on my own struggles to understand the term and how it may be used; I have done so through reference to a personal history, outlined in section 2, that began (during my PhD period) with trying to understand how growth and development takes place in economic systems and has evolved through a tortuous path in which I often had to rethink my grasp of fundamental economic principles. Its final expression for me personally has been as part of a DFID programme on "putting research into use" (RIU in short), a programme funded by the UK DFID's Research and Evidence Division (RED). In the early 2000s DFID had become increasingly concerned about its research expenditure in the natural resources sector. Under its Renewable Natural Resources Research Strategy (RNRRS) it had funded some 1600 projects costing some $350 million between 1995 and 2005, largely in UK research organisations; but it seemed impossible to demonstrate how and to what extent the resultant "knowledge" had resulted in practical low income country (LIC) development. The RIU was initially implemented in 2006 as an attempt both to "scale out" this knowledge and at the same time to understand better how to improve associated science policy. This was budgeted at some $60 million. It ended in December 2012.

At its inception in 2006 the RIU decided to adopt an "innovation systems" approach for its activities though even at that stage it was unclear what defined such an "approach" or indeed what an "approach" (or indeed an innovation system) actually is. In early discussions there appeared to be a wide variety of views ranging from seeing an innovation system as a scientific theory (with definable parameters that could be estimated through experiment) to a loose metaphor based around general systems theory and used to justify an analytical style that emphasised behavioural networks of stakeholder groups involved in technological change. In the end attempts to return to first principles were abandoned and the programme eventually took the form of 3 linked sub programmes focussing on Sub-Saharan Africa (SSA) and South Asia. But underlying all of the discussion was an issue that has bedevilled science policy really since the Second World War; viz how far should scientific research get involved directly in practical development. Section 3 describes briefly how we used the idea of an innovation system in the RIU programme while section 4 summarises some of the broad conclusions we have reached regarding its use in policy analysis.

2. Causes of Economic Change

My own early interests concerned economic growth and change. For my PhD I had set out to estimate a technological production function for the UK using national R&D statistics as a proxy for innovative investment. By then it had become accepted that national investment data could explain very little of observed growth. Incremental capital/output ratios varied widely across countries and well known economists like Robert Solow and Moses Abramowitz had established at least for US manufacturing, that differential investment rates explained only small proportions of observed change[1]. Also a major problem for me, at least at that time, was that "growth economics" told us very little about what actually brought change about. Formal growth models were of little help since they were really about cataloguing forms of macro economic instability in the long term and did not engage with causation and related policy factors. My advisers felt that building such a production function might make a (small) contribution to the debate and had it not been for a withdrawal of co-operation from the relevant ministry (who refused to divulge data on the grounds of national security) I would no doubt have continued in this (statistical) direction.

However, after a year or so reading into the topic I was forced suddenly to change tack and ended up writing a thesis about institutional co-operation between public research bodies and economic production. Doing this brought me into contact with a new institute called the Science Policy Research Unit (SPRU) at the University of Sussex (itself also very new) where I was lucky enough to be appointed to one of two post-doctoral fellowships in science policy (along with Martin Bell). Both Chris Freeman (Director) and Geoff Oldham (Deputy Director) were keen that I worked on LIC development and so I ended up working on aspects of Indian technology development. From then on the concentration for me was development economics with a strong science policy (and institutional) flavour.

Over the following twenty years or so the idea of an innovation system began to take root internationally. At SPRU Chris Freeman suggested that a major explanatory factor in Japan's rapid recent development was that it had built up a networked set on institutions that helped promote sustainable innovation and that these crossed public/private sector boundaries. His view was and remained, that the research agenda should take the form of exploring the relevant institutional context of Japan and other East Asian NICs with a view to making policy assessments that could be generalised in a qualitative sense. I took a different view in so far as I felt a break should also be made with economic theories that subscribe to the notion of general equilibrium. I could not bring myself to accept that economic systems can easonably be portrayed as tending towards equilibrium positions when the reality surely is that they are evolving rapidly in a non-equilibrium manner.

[1] See for example Solow (1957)

At the same time I felt that relegating policy analysis entirely to qualitative modes of intervention still left the so-called Washington consensus occupying the high ground of theory, so that before long we should be hearing yet again about the need to return to free market principles. Along with Calestous Juma we put an alternative view together in a book *Long Run Economics* (1987) that attempted to use an evolutionary systems metaphor to explore how economic systems might evolve in the long run using new technology as a guiding force. At the time in did not go down too well even with Schumpeterian economists, but was re-issued by Frances Pinter in paperback in 1992 and is now coming out again under Bloomsbury Academic this year (2013). One of its drawbacks, however, was that in my view at least it did not fully summarise the underlying mechanics of growth.

Around that time I met a physicist called Peter Allen who had worked closely with Prigogine, the chemist based at the Solvay Institute in Brussels who had won a Nobel Prize for his work on "far from equilibrium" chemical systems. Peter had moved to Cranfield University and was exploring how he could use Prigogine's ideas to model change in socio-economic systems. He had built a computer-based model for the Senegal economy and we agreed to collaborate in putting the argument together in a development context using a simple macro economic analysis as its starting point. The method used was to extend the conventional circular flow of income model (that underlies national income accounting) into the long run using two sets of interactive agents. These are the producers who make long term regional investment decisions and households who move regionally to access better job prospects. Their respective behaviour would inter-relate dynamically in so far as regional investment decisions would encourage regional migration and vice versa. Specifying these relations in parametric form over the recent past would better inform decision makers in relevant policy analysis and decision making. The mathematics was a little complex but Peter had shown how computer simulations could be generated as changing bar charts on a screen.

The idea was to use these models to focus discussion among different professional groups thereby minimising the kinds of cross disciplinary problems that often bedevil policy debate. The models themselves were only intended as a guide to reality, not as precise representations of it. But we felt they would insulate us from the "free market v state control" types of tension that often intrude. Having calibrated the model for Senegal we then (along with Chico Perez-Trejo—now at FAO) extended it to incorporate environmental influences. The result was a book published by Elgar in 1995. In retrospect it was clearly a highly ambitious venture that really needed more development to be accepted, far less understood. It probably also came up against the types of problems faced by general systems theory throughout the 20th century, very great complexity where much simpler positions like neo-liberal economics at least provide a coherent basis for policy (no matter that it has been often very misleading, as we have now learned through the travails of the globalised credit crunch!)

Nevertheless, the exercise had one major benefit for me personally since it solidified the position that Calestous and I had reached earlier regarding the role of scientific knowledge in the making and conduct of development policy. Academic disciplines have often become so well entrenched (both ideologically and institutionally) that professional policy interaction can become confused and often counterproductive. Certainly validated knowledge plays an important role but unless it is harnessed directly to specific problems/issues its productive use can be minimal and sometimes negative.

Finally during much of this period (and more recently) I have been empirically involved with research and consultancy into agricultural production and related issues in LICs (in Africa and South Asia). All of this (hopefully) kept me grounded in the real world; but one particular aspect struck me forcibly and that concerns the position taken by formal agricultural science. Unlike in industry where formal scientific knowledge is usually accessed by economic production agents to deal with specific knowledge gaps related to an investment decision, in agriculture the position is reversed. Here science comes first and is then "pushed through" a complex institutional process intended finally to impact poor farmers. I found this hard to understand, though I found out later that it probably stemmed from the early successes of the *Green Revolution* in India.

Hence by the time I was invited to be part of the RIU and was told that the idea of an innovation system would be an important informative tool, I had effectively come to the following conclusions about economic change and related factors, particularly in small poor developing countries:

1. Economic growth and change depend only to a limited extent both on investment and on formal scientific knowledge. Both are necessary conditions but by the same token far from sufficient
2. The centre of gravity lies with innovation and this is a much broader concept. It involves wider forms and types of knowledge and institutional capacity and these to be effective need to be closely networked
3. The idea of an innovation system should not be used as a formal scientific theory (in the Kuhn/Popper sense at least) but rather as a broad metaphor used to inform policy analysis and decision making. Accordingly the best short run position to adopt is to use the notion of an innovation system to inform the resolution of specific policy issues in well defined contexts
4. While I believe personally that the development of relevant general systems theory with "far from equilibrium" and reflexive content, will prove to be a better heuristic guide to long run policy than neoclassical economics in its current form as taught in our universities, the time is not yet ripe for it to play much of a role.

5. However, there is a sense in which formal science could play a more effective role provided it is allowed to engage more directly (and institutionally) with practical development issues as they affect the very poor farmer in poor countries. Experience with RIU projects has borne this out.

3. The Research into Use Programme

As outlined in the Introduction the RIU has been about putting research into meaningful use. The DFID RNRRS global 11-year Programme run by its Central Research Department (CRD) as it was known then, had come to an end in 2005. An RNRRS evaluation had also informed the development of DFID's new Strategy for Research on Sustainable Agriculture (SRSA)[2] In short the CRD determined that research targeted at development was necessary but not sufficient – what was required was additional effort aimed at putting the knowledge into use. Accordingly it launched a call for consortia whose remit was precisely to fill this gap. Hence within its proposed design for SRSA, CRD included a £37.5m for a programme to get more research into use. It was "*designed to validate and promote the best innovations from previous research funded by DFID through the Renewable Natural Resources Research Strategy (RNRRS)*". And it was meant to "*follow on from existing research programmes using similar management arrangements and utilising established institutional networks including governments, NGOs, national research systems, universities, international centres, donors and farmers*". It was also meant to work closely with the sub-regional programmes such as the CAADP.

The RNRRS had seen significant evolution over its life DFID (2005). This included a shift in focus from generating research and producing scientific publications to emphasizing the impact of research on poverty. The focus also moved from outputs to outcomes and long-term impacts. At the same time, interdisciplinary research, the policy environment and the livelihoods of the poor began to receive greater attention. One of the most influential legacies of the RNRRS was the use of innovation system principles in the development of new partnerships, products, processes, markets, institutions and organisations that are better equipped to put research into use.

The RIU approach was therefore to shift the focus of attention away from the important tasks involved in the generation of new knowledge to the ways in which that knowledge can be put to productive use (in this case knowledge previously generated through the RNRRS). An innovation system is usually seen as a network of organisations and individuals involved in generating, modifying, and using new knowledge. The networks might be national, sub-national, regional or international. They comprise not only the users of the knowledge (farmers, consumers, artisans, labourers and traders) and the producers of new knowledge

2 DFID (2006).

(researchers) but a host of intermediary organisations including extension workers, NGOs, enterprises in the supply chain, credit agencies and government. This systems approach considers not only the totality of the entire research, development and extension spectrum, but also the institutions, systems of production, and social relations in which these activities take place.[3] In practice for the RIU this took the following forms:

(i) *Africa Country Programmes.* RIU established programmes operating in six countries in Africa – Rwanda and Tanzania in the east, Nigeria and Sierra Leone in the west, and Malawi and Zambia in the south. These involved establishing local "innovation coalitions" of stakeholder groups headed by a lead organisation. These would decide (alongside RIU management) on the creation of "innovation platforms" best suited to local needs. These platforms were often commodity based but not necessarily so. Funding of them then took place in a phased form

(ii) *An Asian Challenge Fund.* In Asia, RIU would undertake a cluster of projects, drawing on its established portfolio of 13 initiatives commissioned under an Innovation Challenge Fund working in Bangladesh, India and Nepal, with subsidiary activities in Cambodia and Vietnam. These projects would be clustered to reinforce thematic clusters across the countries around (a) dissemination of crop varieties developed through participatory research; (b) promoting use of current research in the value chain; and (c) promoting natural resource management research.

(iii) *Global Best Bets*. These were conceived (finally) as consortia designed to leverage private sector entrepreneurship in public/private sector partnerships. The mechanism chosen here was to solicit proposals using a variant of the BBC Programme entitled the "Dragons Den". Short listed proposals were required to be pitched to a panel consisting of East African scientists, entrepreneurs and venture capitalists. In this way 9 projects were funded out of an initial complement of some 125 proposals

The lessons emerging from these intervention approaches would be collated to inform future technology development policy. The diversity of interventions was expected to generate useful evidence on how best to promote research, in order to achieve pro-poor impacts. An essential component under this output was to learn lessons on promoting research – what works, where, when and how. Research fellows were employed to undertake documentation and analysis of lessons in every case with intellectual oversight provided by two specified centres in Africa and India backstopped by a core team (Central Research Team [CRT]) led by Andy Hall. The employment of research fellows was also intended as an explicit capacity

[3] Again there is now an enormous literature on the idea of an innovation system as applied to agriculture in poor countries. A good summary account is contained in Hall (2009)

building activity. The Research Fellows were expected to work closely with African country and Asian project teams and the 9 best bets.

4. Some Concluding Points

What have we concluded from these activities? The RIU has been about how to use the notion of an Innovation System as a guiding metaphor (or heuristic) for technology development helping the poor LIC farmer (and the role of scientific research in so doing). Details may be found with reference to the RIU Website (and especially the discussion paper series that resulted) plus two forthcoming monographs[4]. There is still debate within the programme and no doubt this will continue as the results move into the wider arena. But I personally have come to the following broad conclusions. Using the innovation systems notion as an organising principle has enabled us to focus directly upon cognate science policy. This may be seen especially in programme (iii) on the Best Bets. At the RIU outset it proved impossible to identify RNRRS "technologies" that could be "put into use". We actually commissioned a special study which concluded in 2009 that there were no "low hanging fruit" in this category. But by the end of the Best Bets Programme we had identified over 60 RNRRS projects in the 9 funded proposals that have been used. What this shows I believe is the inherent asymmetry of the market for publicly funded knowledge. Such "knowledge" has a unique supply price (based largely on its cost of production) but the demand side is very much a matter of context. Such a market by definition therefore, cannot clear leading inevitably to (potentially considerable) resource waste[5].

On the basis of these cases, it is clear that "putting research into use" is by no means something that occurs spontaneously. But this does not mean that funding research has been a wasted effort. Far from it; the RIU has shown (albeit on a small scale initially) that given the right networks and environment, much valuable research can be put to use in the developing world. In the context of LIC agriculture it is a complex process that needs to operate and be managed as a necessary development activity. And since public resources are involved this means developmental aid. There is therefore a continued need for support to related science and technology activity. However, on the basis of the RIU experience my colleagues and I do not believe that bilateral agencies such as DFID should continue traditional funding patterns at current levels by way of handing out grants and standing back. Simply funding universities and other research institutions to conduct yet more disinterested research is by itself insufficient. Rather what is needed is investment on a sustainable basis that ensures this knowledge is actually put into developmental use. In the RIU case the Best Bets Programme applied a hands-on operating approach. Rather like a private equity firm seeking value for its

[4] See Clark et al (2013) and Gildemacher et al (2013)

[5] See Clark et al (2011)

investors the programme sought value for DFID investments in research. When this environment was established then successful outcomes took place.

What this implies for the wider science policy agenda is a subject for creative analysis. Certainly the conventional *mode 1* approach is insufficient[6]. Following this approach, research-based knowledge would continue to remain "on shelves" and contribute only marginally to development. But public support to alleviate commercial risks still requires complementary input from the scientific community. I suggest that current UK moves to integrate institutionally research council activities with overseas aid, is probably the right way to go. But it will need a pattern of appropriate incentives that encourages scientists not only to undertake applications engineering research but also to link more closely to other bodies involved in practical development, including especially finance and private enterprise. If this is too much for scientific bodies to accept at once agencies could continue to fund cognate RIU activity on an experimental basis until such time as this more systemic approach become more widely accepted. It is my firm belief that a policy shift along these lines will create better "value for money". In other words it will improve the efficiency of the knowledge market in the context of low income country development.

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