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Participatory rural appraisal: a quick-and-dirty critique

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• Introduction

Others may have different historical accounts, but to my knowledge debates about Participatory Rural Appraisal and Rapid Rural Appraisal (henceforth PRA/RRA) began with a workshop on RRA (in which I was a participant) organised in 1980 by Robert Chambers at the Institute of Development Studies in Sussex. That workshop brought together three distinct themes:

- A populist concern to introduce a more 'people-oriented' dimension to data gathering in rural development;
- A practical focus on speed and efficiency in gathering such data (so-called 'rapid rural appraisal') to deal, among other things, with the perceived problem that development agencies were not going to wait for anthropologists and others to complete time-consuming, in-depth studies; and,
- An interest in projective methods where the rural poor (ostensibly the 'object' of development) were offered some scope to set the research agenda, influence the kinds of questions asked by researchers, and perhaps control the results.

The wheel has come full circle. Many development and research funding agencies have now absorbed PRA and RRA as elements of their institutional culture, and in so doing have made routine and safe what in the first instance began as an approach intended to subvert development orthodoxy.

The other day a colleague showed me what seemed a professionally competent research proposal, rejected by the funding agency because it failed to include any discussion of PRA in the section on research methods. It seems that all research proposals with a rural development dimension now require explicit discussion of PRA to be acceptable to the agency concerned. Since the colleague was a competent and experienced researcher, with a sound research design, it hardly required much ingenuity to package and re-label the social research elements already within the proposal as 'PRA' and re-submit. What was striking about this episode, however, was that PRA now seems to have become some kind of 'flag of necessity' without which development-oriented 'research vessels' cannot acquire seaworthiness certificates.

It is pertinent and timely to ask what is being 'flagged' in this way. Does current PRA practice relate to what its original proponents had in mind? And if not, what has it become? And is current practice defensible, either as methodology, or by results (an increased capacity of the poor to act in their own interest)?

There are four particular areas for concern, and I will comment briefly on each, as a way of stimulating discussion.

Has PRA undermined support for long-term social research in rural development?

Six years after the Cold War ended the world is faced with an urgent need to understand patterns of global social change. In all probability, present incomprehension in Europe and North America concerning social process in large parts of the globe will only be

dispelled by new investment in careful, long-term and comparative on-the-ground social research.

Let me pursue this point in relation to African Studies, the case I know best. Britain has haphazardly but sharply dis-invested in its academic expertise on Africa over the past 15 years (the life time of PRA/RRA). The same is true of North America, where serious African Studies research has never been harder to fund. Nor has this short-fall been made good by other international players (Japan excepted), or by African countries themselves. PRA/RRA cannot itself be blamed for what some would consider a general weakening of methodological resolve by the field-work oriented social sciences. But it does not help to stiffen that resolve when proponents of PRA/RRA insist that serious field investigation (inevitably time-consuming and expensive) is some kind of ivory-tower self-indulgence. If the social sciences are not much further forward than they were in 1980 in understanding some of the deepest dilemmas of poverty (some would say understanding has diminished) then surely there is a need for more, not fewer, high-quality long-term comparative studies. There is, therefore, a problem of coexistence. What can PRA enthusiasts do to ensure that their own methodological attainments are not seized upon by research bureaucrats as an excuse to limit further investment in in-depth studies of social change among the rural poor?

The bureaucratisation of PRA: a contradiction in terms?

Does the routinisation of PRA within the bureaucratic processes of development agencies contradict or divert the original aim of giving more voice and control to the rural poor?

We live in a label-conscious world. This enthusiasm for labels imparts an air of innovation to research tools that are not really new at all. Have PRA/RRA enthusiasts forgotten that social scientists long ago regularly used such projective devices as mental mapping and informant-based social ranking? 'RRA' itself is surely nothing more than the old preliminary survey re-labelled to

disguise the fact it is no longer preliminary to anything, but the work the anthropologist is expected to do on an appraisal mission while the economists are assembling the really important data. Producing a report on the final day of a ten-day mission, with coverage of social as well as economic factors, may be the reality of the jet-set consultancy world, but forcing social scientists to work like economists and accountants is part of the problem, not part of the solution!

The labelling phenomenon is a particular worry because, seemingly, it responds to bureaucratic rather than research requirements - the need to find a 'methodology' for the 'soft' social sciences that roughly matches in scale and scope the other items in the kit-bag of the modern development management team. This is not how practitioners of 'hard' sciences go about their problems. They also work in teams, and difficult and time-consuming research processes are recognised as bottlenecks to team performance. But the response is to invest in the bottleneck area, not to hide low standards of data acquisition by giving the troublesome procedure a fine-sounding name.

Understanding the dilemmas of the rural poor is extremely difficult because they lead exceedingly complex lives. They have to, in order to survive. The cultural dimensions of poverty are hardest of all to study, because this involves understanding commitments that pay off only in the longer term (perhaps over an entire life-time). To expect data gathering in these areas to fit short-term schedules is to travesty the issue.

My impression was that economists were beginning to listen to this point when the message was garbled by the emergence of PRA/RRA as an 'answer' to the 'problem' of where to fit the social studies perspectives into the busy schedules of development consultancy teams. Fortunately, theoretical developments within economics itself - especially the economics of institutions - seem to be coming to the rescue. It is clear (from, for example, the work of Douglass North, 1990) that economists have begun to tackle the thorny issues of how to assess 'informal constraints' and measure long-term tendencies within an 'institutional culture'. This brings

economists on to empirical terrain where anthropologists have long been conspicuous in their isolation. If economists (the trend-setters in development methodology) have become more sensitive to the special problems of studying long-term social change this may serve to weaken the bureaucratic logic that requires PRA/RRA to be pressed into service (perhaps against its designers' best intentions) as 'quick-and-dirty' anthropology on economists' terms.

What is the social theory underpinning PRA?

It is not often clear where PRA/RRA practitioners stand in relation to the major debates in social theory. What would PRA/RRA look like as seen from these different theoretical standpoints?¹ The importance, or difficulty, of such an exercise should not be minimised, since it raises the central issue of what the framers of PRA/RRA conceive 'participation' to be. Consider this example.

PRA/RRA now has an impressive repertoire of specific 'methods' (at least judged by the numbers of labels in circulation), but is there any independent evidence to demonstrate that these methods really achieve the data-generating and empowerment goals their proponents claim? At first glance, the added value of PRA/RRA over established social science method, seems to lie in PRA/RRA's emphasis on projective methods (such as ranking, mapping and so on). The projective element provides the 'handle' offered to the rural poor to participate. But what does this mean in practice?

In a famous instance, Bourdieu (1977) showed that the Berber farm calendar is not a seasonal template that guides agricultural decision making, but a product of that decision-making process. It is in appreciation of this point,

¹ What would PRA/RRA look like from, say, a Durkheimian perspective (perhaps some of the methods used by the 'culture theory' research group of Mary Douglas, Michael Thompson, Steven Rayner and others would already count as a type of PRA?), or from the standpoint of 'structuration theory' (Giddens) or 'practice theory' (Bourdieu)?

Hardin's study of indigenous aesthetics argues, that the Krono people of Sierra Leone do not waste energy on teaching in words, but mediate cultural values through practice.

What, then, are we to make of a participatory development exercise that assumes that there is a clear split between structure and action, and where structure takes precedence over action? Put explicitly, what kind of muddle are we in if one set of participants - the organisers - holds the view that the farm calendar being plotted on the flip-chart is a template for agricultural action, and the other group - the rural poor - sees it as an outcome of what they do? In short, is the real worry about PRA/RRA not the legitimacy of its short-cut methods, but rather the implausibility of its (unstated) theoretical frame?

The context of participation

Who then is participating in what? Perhaps the answer to this puzzle will be found if we take seriously the question "*what do the rural poor make of 'participatory development' as practice*"? Do participative initiatives ever break free from the context of local politics? One possible set of answers is to be found by analysing the micro-politics of rural consensus formation. The anthropological literature contains a number of important examples. None is more appropriate to this debate than William Murphy's analysis of a public meeting among the Mende of eastern Sierra Leone (Murphy, 1990). Murphy is not writing about PRA/RRA in action (though he might as well be), but about established local political procedures for consensus formation. He describes how village elders create space for people to express differences of opinion in public meetings, and how through managing this space the decisions made generally favour village élites.

For those of us concerned to rescue the concept of PRA/RRA as a means of empowerment for the rural poor, the first question must be "*was it ever realistic to think that a discourse-oriented PRA/RRA would evade cooption by local politics?*" If Murphy's example induces pessimism on this score, I think the answer, nevertheless, must be "*let's look and see*". Clearly, much work needs to be

done, using the kind of methodology used in Murphy's research, to calibrate and validate PRA/RRA in specific cultural and political contexts. It seems obvious that this work requires the full application of in-depth social analysis. In the absence of such a body of case-study analysis any confidence that PRA/RRA operates independently of established local structures of political discourse, and is therefore effective in reaching goals of participative enfranchisement, is based on faith, not science.

• Conclusion

My sceptical remarks above should not be taken to imply despair concerning PRA/RRA. I still hold to the original ambitions of the 1980 workshop that a social science could be emancipatory for the poor, by involving rural communities in analysis of their own predicament. I am hopeful that the bureaucratic infatuation with PRA/RRA is a passing phase.

The most urgent task facing PRA/RRA, in my estimation, is to come to terms with action. Modern social theory insists that the rural poor, like the rest of us, ceaselessly engage in the business of creating and re-creating life worlds. Discourse (discussion, mapping, ranking...the entire panoply of projective analysis), whatever its value as role-reversal therapy for arrogant professionals (a value I would not deny), is but one restricted dimension of this practice. The serious recognition being given to farmer experimentation among agricultural innovators, as distinct from older approaches to agricultural extension based on the idea of 'information' as a commodity to be 'exchanged', seems to offer some suitable pointers to the direction in which PRA/RRA must move.

In my own estimation, the first major task of this new PRA/RRA will be to wean itself from a desire to document - to 'know things' in ways capable of sustaining discussion, or filling boxes in consultants' reports - and move towards interventions in which attention focuses on action as a key component in the establishment of an emancipatory learning environment.

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REFERENCES

- Bourdieu, P. 1977. *Outline of a Theory of Practice*. Cambridge University Press, Cambridge, UK.
- Hardin, K.L. 1992. *The Aesthetics of Action*. Smithsonian Institution Press, Washington DC.
- Kaplan, R. 1994. *The coming anarchy*. *Atlantic Monthly*, February 1994.
- Murphy, W. 1990. Creating the appearance of consensus in Mende political discourse. *American Anthropologist* 92 (1): 24-41.
- North, D.C. 1990. *Institutions and Institutional Change and Economic Performance*. Cambridge University Press, Cambridge, UK.