

Bridging the 'macro'–'micro' divide in policy-oriented research: two African experiences

David Booth

Introduction

The gap separating the concerns and activities of development practitioners from those of development researchers in academic institutions is no longer the yawning chasm that it once was. Though by no means universally accepted as desirable, closer collaborative relationships between academic researchers and those making decisions about policy and practice for development in official and non-government organisations are now a reality – including, and perhaps even especially, in the field of social development. Several intellectual and practical dimensions of this convergence have been explored from various angles in recent publications and workshops (Schuurman 1993, Booth 1994a, Edwards 1994b).

There remains, however, a need for discussion about the kinds of research that are effective in closing the gap between the worlds of academic analysis and practice. Relaxing the tensions between the practitioner's need for timely and up-to-date intelligence on key topics and the normal requirements of academic professionalism requires more than good will and imagination on both sides. It calls for different ways of working, combining both known and untried techniques in new ways, and the deliberate testing out of fresh approaches with a view to their improvement. It means going beyond general considerations concerning the requirements for academic research to be considered 'relevant' (Edwards 1994a, Booth 1994b) towards a critical discussion of specific experiences in non-conventional research design. This article is intended as a contribution to such a discussion.¹

The article is based on the experience of two studies, both done on behalf of the Swedish official agency, SIDA. The first was carried out in Tanzania, by a team of Tanzanian and British researchers, in mid-1992. Its final report was published under SIDA's imprint as

Social, Economic and Cultural Change in Contemporary Tanzania: A People-Oriented Focus (Booth et al. 1993). The second was completed by another team, British and Zambian, during mid-1994. It is the subject of a draft report with the short title 'Coping with Cost Recovery', completed in November 1994. The present writer was the overall coordinator for both studies.²

Orientations – objectives – conclusions

The Tanzanian study (hereafter 'Change in Tanzania') had very broad terms of reference. It was conceived as a means of addressing the lack of up-to-date information on the ways in which ordinary people have perceived, coped with, and been affected by major changes in the 'macro'-economy and national political system of one of the countries accorded priority for Swedish development cooperation. In other terms, the focus was on local-level change against a background of economic liberalisation, implementation of structural adjustment measures and initial steps towards multi-party politics. One of the objectives was to give a trial run to a possible methodology for addressing this kind of gap in donors' understanding of contemporary change in Africa. The basic design drew on a literature survey, combined with some 'rapid' interactive field-work in five regions of Tanzania.

The Zambian study, 'Coping with Cost Recovery', had a narrower focus. It was concerned with the social implications of the 'cost-recovery' or 'cost-sharing' measures adopted recently in the Zambian health and education sectors. Although the government of Zambia has been committed to cost-sharing policies since at least 1989, the concern was especially with those introduced as part of the Chiluba government's economic recovery programme since October 1991. The main focus was on the impact of the new charges for access to basic health and education services among the poorest sections of the urban and rural populations. The project was designed on the basis of experience with rapid interactive methods of research in other recent studies in Zambia and elsewhere, including 'Change in Tanzania'.

The conclusions of both studies have proved controversial. 'Change in Tanzania' found, among other things, that trade liberalisation seemed to have brought benefits to poor as well as better-off rural consumers and appeared to have been particularly appreciated by women. We also found that most people, especially rural women, were facing the prospect of multi-party democracy with trepidation and a

strong sense of 'better the devil we know'. Within quite a short time, our report was being cited in World Bank circles as a new piece of evidence showing that structural adjustment was not invariably harmful to the poor. It was also criticised by socialist-inclined researchers in Sweden, who objected to the drawing of this sort of conclusion on the basis of the kind of field-work we had done.

'Coping with Cost Recovery' threatens to provoke similar controversy, although the ideological signs will probably be reversed. Despite our protestations that we are not opposed in principle to cost-sharing, the report is likely to be read as a damning indictment of a key aspect of the current reforms in Zambia. On the other hand, the recommendations are unlikely to offend anyone on the Left, and should be received enthusiastically by NGO activists inside and outside the country. This is in spite of the fact that the assumptions and style of research and analysis were essentially the same as those used in the Tanzanian study.

This observation could be the point of departure for a reflection on the rather simple polarities that tend to characterise many people's thinking about current policy issues in Africa, and the need for a more mature and even eclectic approach to such matters. However, this is not my concern here. The studies' conclusions and their reception are mentioned only to help give a rounded initial picture of the two experiences. The rest of the article focuses not on conclusions but on method, and in particular on some similarities and differences between the two studies, and the relevance of the experience as a whole for those interested in the interface between academics and practitioners.

Tanzania 1992: a 'macro'–'micro' perspective on economic and political change

The starting point of 'Change in Tanzania' was the perception that most research available to the donor community focuses exclusively on 'macro' dimensions of change. Attempts to elucidate the situation prevailing at the community level often involve presumptuous statements, based on assumptions about the responses of rural inhabitants to 'macro'-economic and political processes. This represents a poor substitute for studies carried out with the explicit aim of understanding how ordinary people perceive, handle, and are affected by external forces and processes of change.

What is lacking is not community-based research *per se*. Although not as numerous as they might have been, a fair number of such

studies have been carried out in Tanzania over the past 30 years by anthropologists, sociologists, historians, and geographers. However, anthropological monographs invariably adopt a narrow focus on a single ethnic group, village community, neighbourhood, or rural township. They also tend to be rather diverse, both thematically and in terms of the time-scales adopted.

One result is that it is usually difficult to draw together findings from such studies in a way that integrates the treatment of 'micro' and 'macro' issues, at a certain level of generality and over a definite period of time. Since the number of good field reports is comparatively small, it is difficult to get reasonably comprehensive information on *recent* changes, so as to begin to construct a picture of the local processes occurring in response to specified 'macro' events, such as a change of economic policy or a new political climate.

Thus there is a need for new styles of work that are capable of breaking out of these limitations and contributing to the development of a 'people-oriented focus' on contemporary change in rural Africa. It was with this methodological gap, as well as with the substantive issue of the nature of recent changes in Tanzanian communities, that the study was to be concerned.

Approach

The research commissioned consisted of a desk study, followed by six weeks' field research in a variety of locations, involving a team of seven local and expatriate researchers, among whom were several students of Tanzanian rural conditions with many years' experience and one senior anthropologist (Dr Alison Redmayne) who had been in almost continuous contact with her research sites since the early 1960s.

We visited twelve carefully selected rural locations in different parts of the country in June–July 1992, drawing on our own experience in gathering and interpreting information. Thus, the findings emerged from a combination of three main elements: suggestions about broad tendencies derived from previous studies; general conclusions arising from the field study; and the team's assessments of the validity and reliability of the different pieces of available evidence.

The study had obvious limits. It deliberately concentrated on those dimensions of change that tend to escape the more usual country reports, survey-based enquiries, and sectoral evaluations. A 'people-oriented' focus on contemporary change was not seen as a substitute for ethnography. Not only was the time at our disposal extremely limited, judged by normal academic standards; but, as

explained above, we were interested in specific issues which were only partly ethnographic in character.

The broad aim of the study was to provide a basis for provisional inferences about contemporary change and contemporary perceptions of longer-term processes, beginning with a survey of available documentation. In the event we found that previous studies provided a basis for certain suggestions about the way recent Tanzanian experience fitted into the 'structural adjustment controversy' in 'macro'-economic and sectoral terms. Earlier local studies also gave grounds for some worthwhile hypotheses about the direction of contemporary change at the community and household levels. But direct evidence on current responses to economic liberalisation at community level was still thin. There were also many unanswered questions about local responses to the arrival of 'multi-partyism'.

Twelve villages were visited, in eight Districts within five Regions of Tanzania. The areas for research had to be chosen partly with a view to feasible distances, and to the ethnographic knowledge and previous research experience of members of the research team; but the most important criterion was to provide a sufficient variety of socio-economic and ecological conditions. The field-work was carried out in six weeks. The research team included a core of two men and two women who were involved throughout the preparations as well as during the investigation and travelling. The others played leading roles in particular phases of the field-work, drawing on their previous knowledge of the sites and command of local languages.

We were able to spend about two-and-a-half days at each of the places we visited. The basic method was to arrange four to five group discussions with different kinds of representative of the village population, following as far as possible the 'focus-group discussion' approach. When feasible, the time before and after the group sessions was used for observing conditions and activities in the village, and for further conversations with individuals or small groups of villagers about the topics which interested us, so maximising opportunities for methodological 'triangulation' (cross-checking information in three different ways).

Generally, we sought to meet with one group of 'village leaders': a selection of members of the Village Government and some of the village-level technical specialists such as the agricultural assistant or primary school head teacher. Other groups consisted of villagers without leadership or technical responsibilities: one group of village

women, one of young people of both sexes, and up to two other groups of ordinary villagers, male and female.

In all but a handful of groups, our promptings produced informative and often vigorous exchanges of views. Most group discussions yielded much information, including disagreements about facts, and controversy about their significance. Despite some team members' initial misgivings about raising the more sensitive issues relating to culture, ideology, and politics, we found there was little that could not be discussed in the groups.

The study had a frankly experimental character. The objective was both to make a substantive contribution to understanding what is currently happening in rural and peri-urban areas of mainland Tanzania, and to try out a methodology for doing so. To what extent was the experiment fruitful, in our view and that of other specialists?

Assessment I

Our own assessment was fairly positive, but included some important reservations. Within the rather broad scope of a study of 'economic, social and cultural change', there was much about which we remained agnostic; but on a range of topics we felt confident enough to make definite claims. The study had relied a good deal on synthesising existing ideas and extrapolating from past investigations which employed more conventional methodologies. But it seemed clear that it could not have been done entirely on that basis. That is, the case for a combined methodology, drawing on documentary work *and* on a field-work basis, seemed to have been proved.

While the design of the study proved sufficient in terms of coverage of a range of rural and peri-urban conditions, it did not entirely resolve the difficulty of generating generalisable conclusions from location-specific material. Relatively little of the detailed material from the village studies could be included in the text of the report. In other words, the 'narrow focus' which we had described as a limitation of the traditional anthropological study was perhaps not so easy to overcome. In a similar vein, we were conscious that much less could be reported in general about 'culture' than we had hoped. This seemed partly due to the unsuitability of 'rapid' research techniques to the gathering of even moderately good ethnographic material, and partly to the difficulty of handling location-specific material within a general report about a country.

Use of focus-group work as a central technique, supplemented by observation and informal interviews, seemed to be fully justified by the

results of the study. Those of us with experience of traditional anthropological methods but not of focus groups were impressed by the power of the method to generate large quantities of information, and even insights about process, in a very short time. We were also aware of various risks associated with rapid-research techniques, but had guarded against them in various ways.

However, while the focus-group method itself contains some internal checks on reliability of information, these had not been sufficient to prevent some things being said and agreed that we knew to be untrue. More generally, we often felt that certain discussion themes – such as the deplorable state of the roads or the constant rise in the cost of living – were being developed at least partly for effect; that is, despite their disclaimers, the researchers were being addressed as potential benefactors. We made appropriate adjustments before reaching conclusions.

The possibility of checking the results of the group discussions with a member of the team who had extensive knowledge of the field-work area was a very important feature of our approach. This was a key dimension of our triangulation in several cases, and it was especially valuable in the four sites that were familiar to Dr Redmayne. We felt that capitalising on this sort of expertise should be an integral component of the design of rapid studies of the type we were undertaking.

Last but not least, the fact that the work was commissioned not directly by SIDA but through a practice-oriented academic intermediary (the Development Studies Unit at the University of Stockholm) seemed to make a positive difference to the outcome in a number of ways. It gave us an additional source of specialist scrutiny in drafting our conclusions – professionally expert, but also attuned to what the sponsors did and did not want to know. At the same time, it provided us with a cushion against any over-simple or narrowly administrative interpretation of our terms of reference that might have arisen.

Assessment II

Comments on our approach from other specialists were helpful and provoked further reflections in a number of respects. They concerned especially the scope and design of the study, and the appropriateness of field techniques selected.

Among academic commentators in general we found some impatience with the broad coverage of the study and more particularly

with our failure to distinguish between those of our findings that were 'new' and those that were generally well known and understood among specialists. There is some justification for this point of view. On the other hand, the report was received warmly in donor-agency circles (as 'readable', 'informative', 'giving a real feel of rural life' etc.).³ Unfortunately, we suspect that some of the features that commended the report to one set of readers were precisely those that worried the other set. This illustrates well one of the difficulties involved in crossing the divide between academics and practitioners.

In a sympathetic but challenging critique of our study, Peter Gibbon (1994) advanced a particular variant of the above argument. After commending the report as 'well-informed and extremely informative', he goes on to take issue with what he sees as two regrettable biases in our approach: towards 'average' or 'typical' rural conditions at the expense of various extremes; and, relatedly, towards elements of continuity at the expense of sources of change. Along with a correct emphasis on continuities in rural life before and after structural adjustment, Gibbon detects in the report 'a certain reluctance to identify and track down new elements in the picture, both positive and negative'. He would like to have seen more strategic sampling of areas of the country that are significant in relation to what are known to be factors of growing importance in the political economy of Tanzania.

These points should be carefully considered in the design of any future study on these lines. The brief for such a study should probably be clearer in this respect than ours was, since there undoubtedly is a tension between a strategy of portraying typical trends and one with a deliberate focus on change. This does not mean that systematic sampling to highlight novel or strategic factors is a bad idea; indeed this may be where the comparative advantage of rapid qualitative research as against 'proper' surveys lies. But there is clearly a choice to be made between prioritising that approach and taking the more obvious tack of focusing on 'typical' processes affecting large majorities of the population.

For many people, an obvious point of comparison was with 'rapid rural appraisal' (RRA) or 'participatory rural appraisal' (PRA).⁴ In various forums we were accused both of committing the same errors as RRA/PRA practitioners and of not taking seriously enough the rigours and precautions that are now standard in PRA.⁵

The first was the less serious suggestion. It seems to be founded on two mistaken assumptions: (a) that our method rested wholly on the

focus-group work, and (b) that it (therefore?) involved such fallacies as assuming that people do what they say they do, that observing behaviour and studying the wider context in which it occurs are unimportant, and so on. While obviously constrained by lack of time, our field-work approach had involved several methods, of which the group interviews was only one. Moreover, by placing the field-work rather firmly in the context of a literature-based analysis covering 'macro'-economic, institutional, and local-community studies over a decade or so, we had taken precautions to avoid the failing for which RRA practitioners among others have sometimes been criticised (cf. Bebbington 1994): that of detaching grassroots action from its 'macro' context. This could indeed be seen as the main objective and virtue of the design adopted.

The other type of criticism was more serious, being based on a full understanding of PRA techniques and some experience of their application under Tanzanian conditions. It pointed to one real limitation of our study. The heart of this objection was that in one important respect the study did not meet its terms of reference, and could not have been expected to do so, given the limited range of methods that we deployed.

Our terms of reference required us to give special consideration to SIDA's 'target groups', including 'the poorest' rural people. However, we did not make use of specific PRA techniques, notably wealth-ranking, which would have enabled us to identify the poorest people or households in the places we visited. Nor, on the other hand, had we been able to carry out any kind of rigorous sampling of richer and poorer villages, raising the possibility that our selection of sites reflected 'tarmac bias' as well as the almost inevitable 'dry-season bias' which Robert Chambers has warned against (Lindberg *et al.* 1993).

Our critics did us some injustices. On the basis of previous field-work in some of the locations (notably Redmayne's sites in Iringa), we are fairly confident that we did not 'miss' the poorest households entirely; and we would refute vigorously the suggestion that we were guilty of any of the grosser forms of tarmac bias. Also, the critics' suggestion that if rapid field-work is so rapid that it cannot employ wealth-ranking, then it is not worth having, seems a trifle inflexible. Nevertheless, they have a point. At the end of the day, we were not in a position to make any confident claims about 'the poorest'; we were compelled to formulate our findings in weaker (and, arguably, excessively vague) terms: 'poorer people', 'those locally regarded as relatively poor',

and so on. It certainly bears consideration that if practitioners sponsoring rapid research are specifically interested in changes and responses to change among 'the poorest', they must allow for sufficiently intensive field-work, and insist on a research design tailored to this objective.

Zambia 1994: an experience in rapid appraisal

The Zambia study was different in a number of respects. It aimed to cast fresh light on a relatively narrowly-defined issue of great concern among policy-makers and donor representatives in Zambia. It drew on the resources of a multi-disciplinary team which already had some training and experience in rapid-appraisal techniques. And the design of the work was able to reflect some of the lessons of the Tanzania study, as well as those of work of a similar kind done recently in Zambia.

The study originated from SIDA's concern, shared by UNICEF-Lusaka, about the implications for the urban and rural poor of the rapid extension of user charges in basic health services and education. The perceived dangers were of various sorts. The immediate danger was that the charges would contribute to a further deterioration in indices of morbidity, mortality and illiteracy following on a decade of declining social conditions. Less immediate, but no less important, was the danger of political backlash against the reform process in general, which would damage the chances of resolving these problems in the medium and long terms.

On both counts there was a need to increase the rate at which relevant data were being collected and fed into the policy process. In health there were plans to set up a regular monitoring system in the medium term; but the medium term might easily be too late. Therefore there was an urgent need both to summarise what was known already, albeit anecdotally, and to generate some 'rapid' results to flesh this out, to contribute to upcoming bilateral and multilateral discussions between Zambian and Swedish officials.

Apart from this substantive concern, there was interest at the SIDA Planning Secretariat in taking forward the methodological lessons of 'Change in Tanzania' and making connections with those of several studies recently completed in Zambia using rapid interactive methods. The key Zambian experiences were the *Participatory Poverty Assessment* (PPA) which formed part of the World Bank's Poverty Assessment for Zambia in 1993 (World Bank 1994), and three beneficiary assessments of social-rehabilitation projects carried out for different sponsors between 1992 and 1994. All of these studies had been coordinated by

a senior Zambian anthropologist, Dr John Milimo, who now headed the local team for 'Coping with Cost Recovery'.

Approach

A common feature of 'Change in Tanzania' and the Zambia Participatory Poverty Assessment was that they were based on a strategic sample of research sites treated as case studies, using rapid interactive techniques and methodological triangulation. There were also some significant differences. From the repertoire of RRA/PRA, the Tanzania study took only the overall methodological objective known as 'optimal ignorance'⁶ and an essential research tool, focus-group work. This was backed up in *ad hoc* ways by anthropological insights from more traditional sources. The PPA, in contrast, employed a full range of PRA techniques and included a major training effort to familiarise the field researchers with their use.

'Coping with Cost Recovery' involved a blend of these approaches. A range of standard PRA techniques was deployed, though a major effort was needed to adapt these techniques to the specific requirements of the study. Also, as in the Tanzanian experience, it was found useful to leaven the findings of the rapid-appraisal work with evidence from longer-term anthropological field-work wherever possible. The main way this was achieved was by securing a significant input to the study from Ginny Bond, a leading researcher in an ambitious longitudinal study of community coping-capacity in Chiawa, a rural area in the south of Zambia.⁷

'Coping with Cost Recovery' had a relatively narrow focus, but had to be completed in less time than any of the previous studies in which we had been involved. At an early stage it was agreed that there would be much to be gained from selecting a smaller number of sites and spending more time in each place. The costs in representativeness would be more than repaid by the opportunities to explore a wider range of techniques and opportunities for triangulation. There would be further gains from choosing research sites in the same areas as those studied during the PPA or one of the beneficiary assessments. This would avoid the need to start by establishing baseline characteristics.

The terms of reference specified that the field-work should concentrate on poor communities. In view of the population distribution of Zambia, it was agreed to carry out studies in an equal number of urban and rural sites. Two of each were initially selected, with a view to maximising the range of locations within a practical itinerary for two field teams. An additional dimension of triangulation would come

from commissioning some work on the themes of the study in Chiawa, where Ginny Bond was in a position to draw on a baseline survey and some intensive household studies carried out over several years. This was initially conceived as a means of 'piloting' some of the techniques of individual interviewing to be deployed. In practice, it produced sufficiently important results to be treated on a par with the other cases.

After an initial documentary search and a workshop for briefing and training in Lusaka, the teams carried out two weeks' field-work in each of the four main sites. Initially the teams – consisting of four local-language speakers, two men and two women – interviewed planners and staff at the provincial and district levels. They then took up residence in or close to the communities selected for intensive study, where they carried out individual and group interviews in fours and in pairs, and sometimes singly. Dr Milimo and the present writer accompanied the teams in different phases of the field-work.

Group interviews sometimes took the form of very loosely structured conversations; other times they were organised as focus-groups which followed a pre-determined interview route, usually including a mapping or ranking exercise. Specific techniques employed to facilitate the group interviews included social, institutional, and resource mapping; production of time-lines and seasonality charts; and pairwise and sequence ranking. Where possible these standard techniques were adapted to the particular purposes of a study of the social implications of cost-recovery, although in most cases they lent themselves 'merely' to setting a framework in which the topic of user charges could be approached concretely, in relation to specific aspects of the life-situation of the participants.

Individual interviews with community members, as distinct from 'key informants', were set up as far as possible on the basis of a wealth-ranking exercise which firstly indicated the extent and nature of social stratification in the area, and secondly allowed the interviewer to place the subject on a scale from 'very poor to not so poor' in local terms. Lines of questioning drew on the valuable experience of Chiawa study team in conducting household interviews on sensitive subjects such as illness and death.

Assessment

At this point I can provide only a tentative assessment of the experience of 'Coping with Cost Recovery'. Naturally, also, the assessment is that of the study team itself; it has not yet benefited from the kinds of external critique that I was able to cite in regard to the Tanzanian report.

As in the Tanzanian experience, the team felt reasonably confident at the end of the field-work that the methods used had been sufficient to support some worthwhile findings, and that these could not have been inferred from a documentary survey alone. Once again, although less so thanks to the more closely defined terms of reference, the field-work had generated a good deal of interesting information that would not find a place in the report because it was too location-specific. The combination of group interviews, individual interviews, and observation, with careful use of internal consistency checks and triangulation, had proved again to be a powerful tool for shedding light quickly on a specific policy issue.

The deployment of a range of mapping and ranking exercises, which was an innovation in relation to the Tanzanian study, proved worthwhile, but not unreservedly so. As an aid to the conduct of a focus-group discussion, they served well in several instances, providing a helpful means of exploring issues related to the new user-charges concretely, in relation to everyday problems. The pairwise and sequence rankings were the most useful in this regard, whereas the mapping exercises and seasonality charting tended to reconfirm important findings already reported in the *Participatory Poverty Assessment*, rather than breaking fresh ground. Occasionally, mechanical deployment of the repertoire of PRA techniques threatened to be a distraction from the main tasks of the study.

The field-work findings included in the draft report drew significantly on the PRA-assisted group interviews. However, they also depended, perhaps to an equal extent, on each of the following: the key-informant interviews; quantitative data supplied by hospitals, clinics, and schools; direct observations recorded by members of the research team; and individual or household interviews. The most powerful single technique, given the questions that needed to be answered, was probably the use of wealth ranking to select households to be the subjects of semi-structured interviews. Somewhat to our surprise, it proved possible to do effective rankings of wealth or well-being in sections of poor urban neighbourhoods as well as in rural communities, and tracking down and interviewing some of those identified in this way as highly vulnerable was a productive, if personally rather harrowing, experience. This seems to confirm the good sense of allowing enough time to carry out wealth-ranking if the objectives of the study are focused on the poorest.

Special mention also needs to be made of the input from more conventional anthropological work. From the Chiawa study, Ginny Bond was able to contribute two key things. One was a body of observations and insights about one rural Zambian community, including its health and educational facilities, accumulated over a period of several years — a short period by the standards of Redmayne's Tanzanian field-work, but quite long for rapid-appraisal purposes, and long enough to include all the main steps in the implementation of the policies that were our concern.

The other was a set of interviews in households that were well known to the researcher, having been selected as case studies on the basis of a sample survey two years previously. These were a source of a kind of information about behaviour and behavioural change (or the lack of it) that was well-nigh impossible to obtain by the means at our disposal in the other study sites. The conclusions of our report would have been both less confident and more generalised without this input from outside the rapid-appraisal framework.

Conclusion

This article has contributed material for a discussion about ways of working that are effective in closing the gap between academic research and development practice. An outline has been given of two recent experiences in which the author was involved that seem to provide one type of successful example of such bridging activity, involving different countries and somewhat different substantive issues. Both are instances of the use of local case studies based partly on rapid-appraisal techniques to highlight problems and issues arising from national policy measures. To this extent they are also efforts to bridge that other divide, highlighted in the title of the article, between the 'macro' and the 'micro'.

The conclusions that it seems possible to draw from these two experiences, and others mentioned in the article, are necessarily provisional. Both of the studies described were learning processes, with one drawing substantially on the lessons of the other; and this type of continuous adjustment can and should continue. With this proviso, the following seem to be the suggestions that are worth making at this point:

- There clearly is scope for academic researchers to become involved in innovative research designs that meet the needs of practitioners concerned about social development issues without ceasing to be

challenging, personally and intellectually, to those carrying them out.

- Combining rapid interactive field-work with documentary surveys seems to provide a way of bridging the 'macro' and the 'micro' that is both intellectually defensible and appealing to practitioners.
- The basic philosophy and technical repertoire of RRA/PRA represents a rich fund of thinking and experience in this sort of work. However, there is a very good case for combining PRA techniques flexibly with inputs from more conventional sources, including long-term ethnography, even when the time-scale is very short.
- Managing the balancing act that some of this involves may well be easier if the relationship between the sponsor and the research team is suitably mediated by a practice-oriented academic unit of some kind.

Notes

- 1 Our views of appropriate methods for development workers, and background to some of the terms used in the article, are given by Rudqvist (1991), Pratt and Loizos (1992), and Moris and Copestake (1993).
- 2 I am grateful to my co-authors for the privilege of drawing on our joint work in this article, that is, to those named in the reference list below and to John Milimo, Ginny Bond, Silverio Chimuka, Mulako Nabanda, Kwibisa Liywalii, Monde Mwalusi, Mulako Mwanamwalye, Edward Mwanza, Lizzie Peme and Agatha Zulu.
- 3 The first type of reaction was among those recorded at the seminar organised by the Swedish Development Cooperation Office in Dar es Salaam to review the report and its findings. The second was more prevalent at the international seminar on the report organised by SIDA in Stockholm.
- 4 The most comprehensive introduction is Chambers (1992).
- 5 The former came from some Tanzanian academics at the Dar es Salaam seminar; the latter was made by the Swedish critics mentioned earlier, initially at the Stockholm seminar and subsequently in Lindberg et al. (1993).
- 6 A good brief account is given in Chambers (1993: 18-19).
- 7 This is being sponsored by SAREC and carried out jointly by IHCAR, a department of the Karolinska Institute, Stockholm; Hull University's Department of Sociology and Anthropology; and the Institute for African Studies, University of Zambia.

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