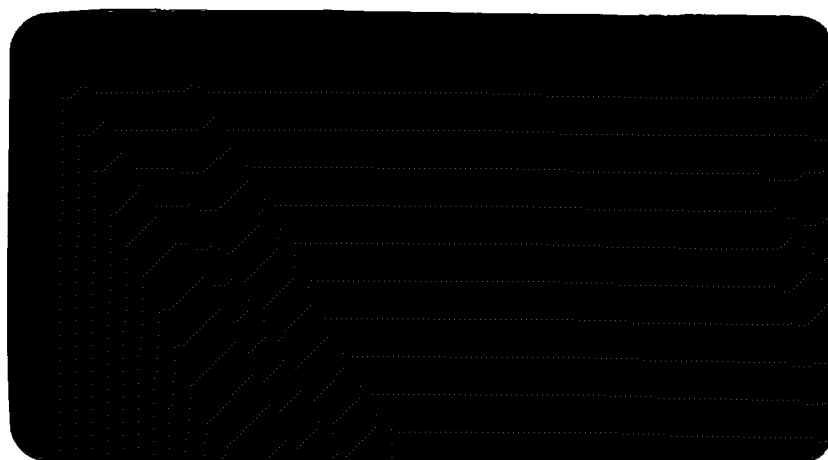


WORKING PAPER



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WORKING PAPER

No. 9

THE ACTION RESEARCH APPROACH TO PROBLEM SOLVING,
WITH ILLUSTRATIONS FROM IRRIGATION MANAGEMENT

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Introduction

An earlier version of this paper was produced for a professional audience with a specialist interest in the organisation and management of irrigation schemes in developing countries.¹ However, it was felt that it could usefully be reworked into an ODI Working Paper for a wider readership, on the grounds that it deals with a research/problem-solving methodology with applications that extend well beyond irrigation. Though this new version is still permeated by references to irrigation - to illustrate the potential for applying action research in a particular management context - its main purpose is to encourage thought and discussion about action research as a tool for organisational change in general. For all of us who are not solely concerned with the planning of development programmes and projects but also with their implementation (and that should include all good planners), a good understanding of action research techniques would seem essential. When it comes to trying to improve the way decisions are made, conventional 'positivist' research often has remarkably little impact on prevailing practice. This may have nothing to do with the quality of the research as such - it may be of the highest intellectual calibre. It's just that it's the wrong kind of research for that kind of job.

The main objectives of the Agricultural Administration Unit of ODI are to carry out comparative research into the organisation and management of agricultural support services in developing countries and, through dissemination of research results and more direct advisory work, to help bring about improved performance of those services. My own particular concern has been with the management of irrigation schemes. In a recent study for the World Bank of the organisation and management of large irrigation schemes in Asia, I found major weaknesses in scheme performance,

1 'Action research towards improved water distribution', Paper 1/31/2 circulated to members of the Agricultural Administration Unit's Irrigation Management Network (October 1981).

principally as a result of deficiencies in government agencies' management of water distribution between the head of the irrigation system and the watercourse outlets.¹ On very conservative assumptions, I concluded that in the predominantly rice-growing areas of Asia, programmes designed to improve the management of water distribution could be expected to generate average production increases of at least 20%. This would mean an increase in rice production in the South/South East Asian region of about 20 million tons of rice without any major capital expenditure (Bottrall 1980a: 24).

Given the very high cost of constructing new irrigation works and the scarcity of capital resources in most developing countries, one might expect an enthusiastic response to measures which could bring benefits of such magnitude through the substitution of management for capital. However, for reasons explained later in the paper, governments and aid agencies have tended to be markedly reluctant to address themselves seriously to the problem of poor main system management. As a result, I have become increasingly convinced that research of the kind I did for the World Bank - which involved fairly detailed evaluations of scheme performance and management in four countries - is unlikely to be enough on its own to have a significant impact on the way major decisions are taken. To break down the widespread resistance to improving the management of irrigation schemes, and of water distribution in particular, another requirement (though not necessarily the only one) is that such evaluations should be followed and reinforced by further research of a different kind - and that is *action research*.

My conception of action research in the irrigation context has been *a pilot action programme involving experiments in alternative management methods which a research team helps to design and monitor, with a view to the subsequent replication of the approach on a larger scale after field tests have shown it to be viable*. Having advocated the idea for some time in fairly general terms and found that it was attracting increasing interest

1 Referred to in the rest of the paper as 'main system management'. On large canal networks, the watercourse outlets are usually the points at which responsibility for system operation and maintenance passes from government agencies to farmers - government being responsible for the main system canals and farmers for the relatively small watercourse channels whose purpose is to convey water from the canals to individual farms.

in several countries, I felt the need to look in greater detail at the real meaning of action research and its likely implications in practice. This led me to consult the writings of social science analysts who were familiar with the use of action research as a tool for organisational change in other contexts - in most cases commercial or industrial enterprises in rich countries. It was reassuring to find that my understanding of the action research process was consistent with theirs. However, it was also apparent that additional elements would need to be introduced into the process if it was to be fully effective in the context of irrigation schemes in developing countries.

The first part of this paper discusses the peculiar characteristics of action research, with special emphasis on those which distinguish it from 'positivist' social science research. This is followed by a more detailed look at the context of large-scale irrigation management in developing countries and the consequent need for adaptation in action research methods. The last section deals with some of the most common pitfalls and dangers of action research.

What exactly is action research?

Action research has had quite a long history within the social sciences. First use of the term is attributed to Kurt Lewin in 1946 (Susman and Evered 1978: 506). One of its essential features is its dependence on close collaboration between researchers and clients in seeking solutions to problems of organisation and management. In this it differs radically from more conventional approaches to organisational analysis, in which researchers, adopting the positivist stance of physical or biological scientists, have sought to learn about organisational structure and behaviour from the position of disinterested, detached and neutral observers. From this stance the people in the organisations concerned are seen as objects of external enquiry or experiment rather than as potential collaborators in decision-making.

Proponents of action research have tended to direct their criticisms of the positivist approach at two main targets: conventional management consultants on the one hand and certain rigorously 'scientific' academic theoreticians on the other. Both are criticised for seeking to analyse organisations from the outside instead of entering into an equal and collaborative relationship with them and for failing to establish strong links between theory and practice. In conventional consultancy work, key decisions tend to be taken by the client, who defines the problems to be investigated and draws up the often narrow terms of reference under which the consultants are expected to operate. The consultants' overriding objective is to solve particular problems of immediate concern to the client and they attach little importance to the development of general theory (cf Clark: 3-24). Meanwhile the academics devote themselves to studies whose results are of minimal relevance to decision-making in the real world. The chief charges made against them are that they have encouraged the divorce of theory from practice and created a communication gap between themselves (the 'experts') and the members of the organisations they study.¹

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1. 'Many of the findings in our scholarly management journals are only remotely related to the real world of practicing managers and to the actual issues with which members of organisations are concerned, especially when the research has been carried out by the most rigorous methods of the prevailing conception of science' (Susman and Evered: 502).

By contrast, action research programmes are intended as 'learning laboratories' for both clients and researchers (cf D. Norton 1980: 507, fn 64). They require the direct involvement of both parties in identifying problems, planning new approaches designed to overcome them, and evaluating the results. If properly executed, action research should be of much greater utility to a client organisation than conventional research or consultancy, not only in identifying solutions to immediate problems but also (through the learning it entails) in helping to develop the organisation's capacity to deal with other problems that arise later. Moreover, by giving the researchers privileged access to knowledge about the inner workings of an organisation, it should also provide much better opportunities to generate practically relevant theory (Clark 1972: 125 - 126).

The potential advantages of action research stem from its rejection of the view of the researcher as sole expert, investigating and experimenting on an essentially passive world. Instead, the active involvement of clients in the research process makes it possible to synthesise contributions to knowledge by *both* parties:

'The action researcher brings theoretical knowledge as well as breadth of experience to the problem-solving process. The clients bring practical knowledge and experience of the situations in which they are trying to solve problems. Neither client nor researcher has better knowledge; in a sense, they are both experts' (Susman and Evered: 597).

Levin conceived of the action researcher process as 'a spiral of steps, each of which is composed of a circle of planning, action and fact-finding about the result of the action'. In a later formulation it has been represented as a cyclical process with five phases:

- Diagnosing (identifying or defining the problem)
 - Action planning (considering alternative courses of action for solving a problem)
 - Action taking (selecting a course of action)
 - Evaluating (studying the consequences of an action)
 - Specifying learning (identifying general findings).
- (Susman and Evered: 597-8)

The extent of collaboration between researchers and clients during each of the five phases can and does vary in different circumstances (ibid: 588). However, in the context of irrigation management with which I have been particularly concerned, I would see the following allocation of responsibilities, involving close collaboration between both parties throughout, as approaching the ideal:

Diagnosis. Research team to conduct independent, objective appraisal of client organisation's existing structure and management performance; subsequent joint discussion of findings between client and research team and agreement on definition of principal problems.

Action planning. Joint consideration of alternative courses of remedial action. Joint agreement on course of action to be followed.

Action taking. Client organisation to take agreed action; research team to stand back from action, monitoring client's decision-making processes and their effects.

Evaluation. Research team to present evaluation of action programme to client for joint discussion.

Specifying learning. Client to extract lessons from evaluation of particular concern to itself (which may be fed back into further cycles of action planning, action taking and evaluation). Research team to extract lessons for general theory and for its application in action research programmes elsewhere.

Conceived in this way, the action research process has evident affinities with the planning process (Susman and Evered: 588). Compare, for example, the following 'ideal' sequence of decisions which an organisation might follow in the course of a full planning/management cycle: *Plan formulation* (identification of alternatives - design - appraisal - selection) → *Plan implementation* (budgeting - programming - monitoring - adjusting) → *Plan evaluation* (data collection - data processing - policy analyses) → *Plan reformulation* (Belshaw 1976: 418). The essential difference between the two processes is that in the action research case the organisation enlists the help of external researchers in performing diagnostic, planning and monitoring activities which in normal (ie less experimental) circumstances would be carried out internally by its own staff alone. The ultimate aim of an action research programme must be to evolve an improved management system

over which the staff of the organisation concerned, through their participation in the learning laboratory, will be capable of reassuming total control. On the final withdrawal of the research team, responsibility for planning and management will once again become fully 'internalised'.¹

Another point worth noting about social scientists' perceptions of, and experiences with, action research is that interventions designed to bring about organisational change may take a wide variety of forms. Organisations can be viewed as complex systems containing four salient interacting variables, each of which may provide appropriate points for intervention: *task* (which refers to the objectives and functions of the organisation), *technology* (its physical equipment), *structure* (systems of authority, information systems, coordination and communication), and *people* (the actors in the enterprise, their attitudes and expectations). Since these four variables are highly interdependent, a change in one will almost certainly elicit change in the others. Depending on local circumstances and opportunities, one or other may be selected as an intervention point - or there may be scope for intervention from several points together (Clark: 27 -30).

Adapting action research to the Irrigation context

The general principles of action research which have been evolved by social scientists through experience in other contexts seem fully applicable to the context of large-scale irrigation management. However, there seems little doubt that, in translating these principles into specific programmes, additional elements will usually need to be incorporated into the action research process if it is to succeed in bringing about significant organisational change. This is because irrigated smallholder agriculture has at least two distinctive characteristics which will not have been encountered by action researchers elsewhere. One is the presence of a large number of farmers with powers of independent decision-making, who add a complicating third dimension to the usual researcher-client relationship. The other is that, instead of being concerned with achieving relatively subtle shifts of emphasis within a single relatively independent and flexible enterprise, the aim on irrigation schemes is to effect very substantial changes in practice

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1. of David Korten's three stages in the 'learning process' approach to institution-building-learning to be effective, learning to be efficient, and learning to expand. These involve a similar progression from a high degree of externally-assisted experimentation towards ever-increasing administrative 'normality' (D. Korten: 499 - 501).

within public sector agencies which are accustomed to applying rigid, uniform patterns of organisation and management over large areas (an agro-climatic region, a State, sometimes even a whole country) and which, in addition, are strongly resistant to management changes of the kind most needed. This means a much greater concern than in conventional action research with pilot experiments and with the extension of lessons learnt on them to other areas.

Both these factors - the need to involve farmers and their representatives as much as possible in the action research process and the need to extend and replicate lessons from pilot areas - imply a long period of experimentation, with a series of action research cycles extending over several years. They also imply the need to reinforce the action research process through regular training sessions and workshops for irrigation officials and farmers, from both inside and outside the command area where the experiments are being made.

For readers unfamiliar with irrigation in developing countries, my repeated references to official resistance to management change may need some amplification. As already indicated at the beginning of this paper, my own research in Asia found deficient main system water distribution to be a principal cause of poor performance on large irrigation schemes; in two out of three cases design deficiencies were also a contributory factor. On one scheme, the worst management problems were associated with the operation of public tubewells and were reflected in frequent breakdowns, poor planning and implementation of operating schedules, lack of communication between tubewell operators and water users, and preferential access to tubewell water on the part of larger farmers. On the other two schemes, poor water distribution practices were manifested in the classic pattern of locational inequity found on many large canal systems. Watercourses at the head of the canal commands were allowed to draw much more water than they were entitled to, leaving those at the tail with inadequate and unpredictable water supplies, or in the worst cases no supplies at all. Evidence showed that there were two principal reasons for the failure of government staff to operate the main system satisfactorily: inadequate technical skills in water scheduling, and insufficient motivation to resist often powerful pressures to misallocate water, especially in times of greater scarcity. The problem, in other words, is as much social and political as purely technical. Wherever water is a scarce and valuable resource, the motivation of those in charge of rationing and allocating it - the main system managers - becomes a critical issue; and where (as in most cases) the financial and other rewards for firm and fair management are unattractive, the likelihood of mismanagement is great.

Many other researchers have been coming to similar conclusions (eg Wade 1978; Pant 1979; Palanisami 1981 in India; Moore 1980 in Sri Lanka; EWUP 1979 in Egypt; Wickham and Valera 1979, Early 1980, 1981 in the Philippines). The same view is being increasingly endorsed by senior irrigation administrators (eg Ali 1980; Jayaraman 1980; Murthy 1980; Sinha 1978). Yet most government and aid agencies have been unwilling to face the facts and their implications for policy. Typically, they have ignored the issue of main system management altogether and have chosen to limit their attention to problems of water management at the watercourse and farm levels only. Their investment has been concentrated on physical infrastructure alone at the main system level and on a combination of physical infrastructure and reorganisation at the watercourse level - the familiar package of 'on-farm development' and 'water users' associations'. It should be obvious that in those cases where main system management is seriously faulty, such an investment strategy must be sub-optimal: if water is not being delivered to the watercourse outlet adequately and predictably, investments below the outlet, whether in hardware or software, are bound to produce disappointing returns.

It should by now be clear why main system management has been a 'blind spot' for so many official agencies. The implied assumptions behind their policies are that management problems only arise in those parts of the irrigation system for which the farmers, not government, are responsible. How convenient! And how unpalatable to many is the suggestion that the principal causes of poor irrigation performance are faulty planning, design and main system management on the part of the 'experts' (Wade and Chambers 1980; Bottrall 1981 b). No wonder there is resistance to the researchers' and evaluators' findings. However, as their evidence accumulates, the traditional position is becoming increasingly difficult to sustain. Action research-cum-training is likely to be one of the most effective ways of breaking down the barriers of defensiveness and suspicion. Apart from its other virtues, action research on large irrigation schemes must necessarily be carried out on an experimental pilot project basis and this provides an opportunity for introducing improved management gradually and unobtrusively, in a way which need not be overtly threatening to the power and prestige of irrigation planners, managers and other important interest groups.

There has been limited experience so far of experimental, open-ended action research in the fields of agricultural and irrigation development, though no shortage of pilot projects with pre-planned 'blueprint' institutions.¹ One example is the attempted introduction of the Programming and Implementation Management (PIM) System into rural area development programmes in Kenya, where external initiators were used 'to appraise local conditions, to design appropriate procedures, to introduce them, and through continuous monitoring and evaluation in collaboration with those who are operating them, to modify them and introduce simplifications' (Chambers 1974: 53). In the irrigation field, action research has played an important part in the programme to promote farmers' participation in the planning and construction of small community-managed systems in the Philippines (Bagadion et al 1980). But the only example of sustained action research with a focus on the management of water distribution on large irrigation schemes has been the NIA/IRRI programme, also in the Philippines. Experiments with improved management procedures on one distributory produced an overall increase in dry season production of 39% over one year, including a 137% increase in the tail section; in a later experiment, production was affected by pest damage and typhoons but dry season water utilisation efficiency was increased from about 50% to 70% (Early 1981).

Both in terms of their method of implementation and of their results, the NIA/IRRI experiments lend encouragement to the view that action research-cum-training could be introduced effectively elsewhere to tackle deficiencies in main system management. On method, Alan Early, an engineer with IRRI, has recently written about the experiments from a perspective remarkably similar to that of social scientist proponents of action research. Echoing their dissatisfaction with the limitations of positivist organisational analysis, he rejects traditional forms of irrigation research as inappropriate to the solution of management problems:

'Research on irrigation system management problems cannot be carried to conclusion in laboratories or experiment stations. It requires a definite intervention in the procedures of managing irrigation systems' (Early 1981: 2 - 3).

This intervention implies 'a unique collaborative methodology between the (irrigation) agency and the research institution', involving the following steps: define problem, develop methodology, select site, train personnel, implement improved management, collect data, conduct analyses, evaluate experience

1. For a powerful criticism of the latter approach, beloved by governments and aid agencies, see D. Korten 1980.

and report results (ibid: 3). In the NIA/IPRI case, changes in management procedures have been accompanied by minor changes in technology, in the form of improved control and measurement structures (Early 1980: 87; 1981: 11).

Early's paper is also valuable in pointing to weaknesses in the action research methodology employed by the NIA and IPRI staff and it is to this theme that we turn next.

Dangers and pitfalls

All action research, whatever the context, is surrounded by pitfalls and those who engage in it need to be on regular guard against them. Things can go wrong at any stage of the cycle, and when they do there are likely to be two main sources of trouble. Firstly, there may be an imbalance in the relationship between the sponsoring agency and the researchers: instead of being genuinely collaborative, decision-making is excessively dominated by one party. Frequently it is the sponsors who dominate, in which case the researchers risk losing their professional identity and ceasing to be independent agents (Clark: 81); but it can sometimes be the researchers who overreach themselves. Secondly, it may often happen, even where the client-researcher relationship is satisfactory, that the researchers have problems in combining and reconciling their dual roles of co-planners on the one hand and objective analysts on the other. Evidence from the irrigation field suggests that non-social scientists without previous action research experience may be particularly prone to confusing the two roles; but even experienced operators with a clear understanding of action research principles are regularly faced with the question of how best to perform two tasks (the theoretical and the practical) for more than one task master, and the answer is rarely easy (ibid: 22, 126).

Particular vigilance is likely to be needed where the action research concerns irrigation management, for the following principal reasons:

- (a) The sponsor - usually a government or parastatal agency - will have had little or no previous experience of action research or of the client-researcher relationship it entails.
- (b) The irrigation research establishment - largely technologists - have been used to working in a very different intellectual tradition of experimental work. This may sometimes lead to work being done in the name of action research which offends against some of its most basic principles: for example, instead of a programme being planned and executed by two agencies together, a single agency (either within a government or a separate research body) may seek to do all the work on its own.
- (c) The need to adopt an experimental pilot approach exposes the exercise to serious danger of falling into the 'unreplicable pilot project' trap.

- (d) Where main system water distribution has been identified as a major problem, there may be strong resistance from certain quarters to allowing free and unfettered analysis and experiment.

These points can be illustrated by reference to particular problems which commonly arise at different stages of the action research cycle (see pages 5-6). The first stage of *diagnosis* or problem identification is of crucial importance. The greatest danger here is that the reasons for the current performance of an organisation will not be explored in sufficient depth and detail before conclusions are drawn about the nature of its problems and the remedial action required to solve them. The consequence will be that too narrow a range of alternative courses of action is examined during the subsequent action planning stage. This fault may sometimes be the result of client domination, often perhaps because the client is in a hurry, wants quick answers and allows insufficient time for preliminary investigations (eg Clark: 113). In the irrigation context, it may also commonly stem from narrow single-disciplinary vision, on the part of both client and researcher. For example, an engineer may automatically assume that poor system performance is entirely attributable to technical factors and start experimenting with different kinds of canal lining; someone else may assume that all problems can be solved by creating water users' associations; or a third person may assume that the only thing needed is to improve main system management (cf Lenton 1980: 5-7). None of them is likely to be right. The only reliable safeguard against premature problem definition is for the client to allow sufficient time for an independent, interdisciplinary 'whole system' analysis of current scheme performance and the reasons for it. (The framework for a detailed diagnosis of these issues should already have been established before the beginning of the action research process through an initial externally-commissioned identification study, of the kind discussed in my World Bank report.)

Entry into the *action planning* stage requires the research team to transform themselves from independent analysts into co-planners, but both they and the client agency may find the adjustment difficult. Sometimes instead of a genuine partnership emerging, one or other side will dominate. Care also needs to be taken at this stage against selecting and designing an experiment which is likely to be unreplicable. Early, in drawing lessons from the NIA/IPRI experiments, warns that action research on irrigation

management 'must be conducted on a realistic scale to avoid pilot project concentration of resources' (Early 1931: 3). The resources concerned may be financial (eg unreplicable subsidies to farmers) and/or administrative. In the latter case, the principal danger lies in increasing the operating agency's staffing levels within the research area to a point which will be unrepeatable on a larger scale.¹ The presence of extra research and planning expertise is also likely to have a distorting influence on the results of an action research programme, but that is to some extent inevitable, especially in its initial stages. Probably the best that can be done to minimise the effects of the distortion is to discount for it at the evaluation stage and to keep reducing the role of external personnel steadily throughout the course of the programme.

Action taking. Once the content of an action programme has been agreed, it must be executed by the client agency alone, with the research team reverting to a purely analytical role, monitoring programme performance. This principle is central to the whole purpose of action research, which is to develop and test the capabilities of the client agency under new conditions of organisation and management. Unfortunately, it has not been fully understood by some of the technical specialists who have been active in water management research. There have been cases where research teams, in designing an experimental pilot programme, have themselves taken on direct responsibility for executing it, with little or no involvement of the staff of the irrigation project concerned. Their intentions have usually been entirely honourable - to show staff 'how things should be done' - but experience shows that experiments of this kind, though not entirely valueless, tend to have little influence on subsequent staff performance. This is hardly surprising, since the staff have been excluded from all opportunity to learn by doing. Moreover they will have noted that the research team's results have been achieved in the absence of constraints under which they themselves normally have to operate; they will therefore be inclined to regard them as impossible to emulate and of largely academic interest (compare small farmers' attitudes to agricultural extension recommendations based on research station experiments). At worst, the experiment may

1. This does not mean that existing staffing norms must never be exceeded in action research areas. But it does imply that an increase over present norms should not be agreed to by those concerned unless they have reasonable grounds for expecting that the increased level will be sustainable on a large scale in future.

actually demoralise staff further if it appears to have no obvious purpose beyond publicly exposing their deficiencies.

Even where field staff have been given clear responsibility for executing an action programme, the research team may sometimes be tempted to intervene and lend a helping hand. By his references to 'external interventions' and 'the presence of outsiders ... creating extraordinary opportunity for success', Early appears to suggest that there may have been occasions during the NIA/IRRI experiments when IRRI researchers have strayed across the borderline and involved themselves in decisions which should have been left in the hands of NIA staff (Early 1981: 3, 17, 20). The temptation is easy to understand, but it should be strongly resisted because it can cast doubt on the validity of the whole experiment.

Monitoring, evaluation and drawing conclusions. For good monitoring and evaluation the research team must be in a position to analyse the client agency's performance independently and objectively. Here as elsewhere in the action research cycle there may be particular proneness to analytical error in the irrigation context because of the predominantly technical traditions of irrigation research. One major danger is that the research team will confine itself to the measurement of quantifiable performance indicators without documenting the processes by which performance has been achieved. It is only through systematic recording of the ways in which decisions have actually been taken that significant lessons about management reform can be confidently learnt and extended. Another danger is that the real financial and administrative costs of the experiment will be overlooked, so that the expected benefits from its extension on a larger scale will be exaggerated (Lenton: 7-8). The answer in both cases lies in the use of improved monitoring and evaluation procedures and of an interdisciplinary research team with the appropriate skills in organisational analysis to apply them.

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1. The organisers of the NIA/IRRI experiments can be criticised on this score. As Frances Korten has pointed out, they have never explained how NIA personnel managed to persuade upstream farmers to wait several weeks for their water to enable downstream farmers to receive supplies first. In fact, the experiment involved intensive communication with the farmers. Yet the details of that communication process - issues such as methods of contact and communication, farmer organisation, and staff motivation - were not reported on, though they consumed considerable day to day attention of the researchers. I think they were considered to be administrative "nuisance" issues, outside the scope of "scientific" research' (F. Korten, 1981).

A further very insidious danger, which has nothing to do with weaknesses in analytical technique, is that the client or the researchers, or both, may be tempted to 'bend' the research results. This is likely to be a particular hazard where action research on irrigation is concerned because so much - sometimes the whole shape and scale of an investment programme - can hang on its conclusions. The danger can be illustrated by a particularly deplorable pilot project I once encountered in the field. Without investigating alternatives, aid agency and government officials had in this case started from the assumption that high returns were likely to come from improved watercourse layout. A few pilot watercourse projects were then constructed at unreplicable cost and new water users' associations were created. A monitoring unit was set up, using staff from the irrigation agency concerned rather than independent researchers. Ignoring the cost aspect, the unit estimated the benefits of the pilot watercourses by comparing their crop production levels with those of nearby control watercourses. The pilots' production levels were shown to be much higher and on the strength of this they were proclaimed a great success. What was not mentioned, however, was that the pilot watercourses were being allocated much more water than neighbouring units and were also given preferential access to fertiliser and other inputs. The monitoring team's conclusions were therefore based on a conscious fraud. It is difficult to escape the conclusion that the sponsors were never seriously interested in objective research into alternatives but instead saw it as an opportunity to bolster arguments for an already favoured investment programme. A major capital-intensive programme of watercourse rehabilitation and construction has since been launched on a nationwide scale.

From the foregoing paragraphs it is clear that unless it is very carefully carried out, action research (or work done in its name) can often be a source of intellectual confusion and, in the worst cases, intellectual dishonesty. Fear of association with such work is probably one of the chief reasons why some academics tend to shy away from action research in general (though there may be other reasons too, such as failure to understand the principles of good action research, aversion to working with governments or businesses, or fear that action research work might not prove professionally rewarding).¹ However, it does not follow that because a job is difficult to do well it should not be attempted at all. The NIA/IRRI programme,

1. On the last point, see Vyas 1979: 22.

though open to criticism, has shown that serious action research on irrigation management is possible. Further progress can be made if those concerned are prepared to learn from past mistakes and failures. Intellectual confusion can be reduced by a better understanding of action research principles, while the most effective weapon against dishonesty is public exposure.

In summary, experience suggests that if an action research programme is to be successful, the following conditions must be met:

- (a) Two separate agencies must take part - the client organisation and an independent research-cum-planning support team.
- (b) The programme must be concerned to test alternative approaches to organisation and management and analyse them as objectively as possible.
- (c) The relationship between the two agencies concerned must be collaborative: 'one of joint effort, where there is mutual determination of goals, and in which each party has ... opportunity to influence the other' (Clark: 79).¹
- (d) The changing roles and responsibilities of the research team at different stages of the action research process must be clearly specified and understood by both parties.
- (e) To guard against the selection of too narrow a focus for the action research programme, it must be preceded by a wide-ranging diagnosis of weaknesses in current practice.
- (f) In monitoring performance, the research team must record in detail the decision-making processes through which a particular level of performance has been achieved, so that the right lessons can be fed into the next action research cycle and into new programmes elsewhere.

1. Clark's full text reads 'equal opportunity to influence the other'. Ideal, no doubt, but an entirely equal relationship is difficult to envisage in most irrigation contexts, except perhaps where the research agency has international support, as in the case of IRRI.

- (g) Before attempts are made to adapt the programme for extension to other areas, care must be taken to ensure that it is financially and administratively replicable.
- (h) In the irrigation context, the effective extension of lessons to other areas will require the action research programme to be integrated with regular training and workshops.
- (i) Where the programme is designed to influence policy over a large area, a coordinating committee with responsibilities for longer-term planning and supervision will be needed at provincial or national level (cf Bagadion et al, 1980: 5; Early 1981: 20).

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