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DISCUSSION PAPER

Preliminary material and interim research results circulated to stimulate discussion and critical comment

I Farming Systems Research: hitting a moving target

II The Social Scientist in Farming Systems Research

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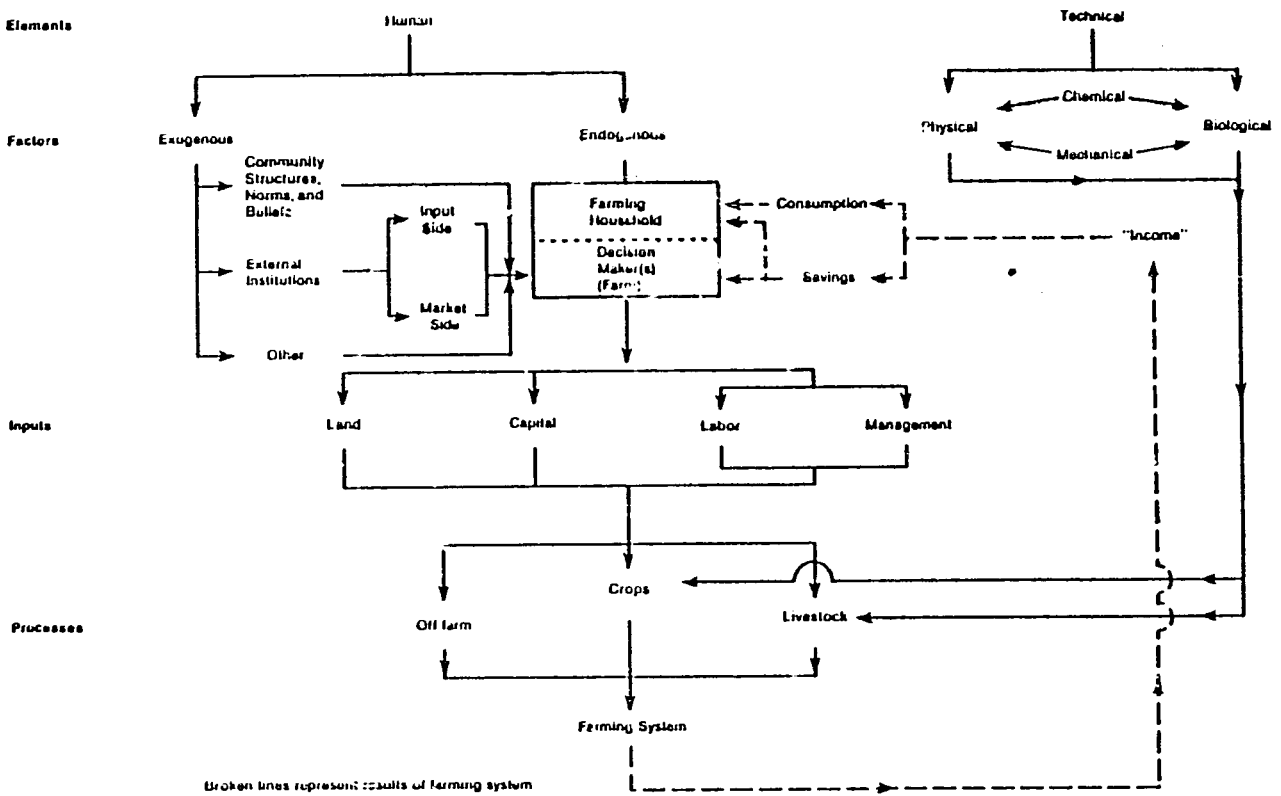

Figure 1 Schematic Representation of Some Determinants of the Farming System

Source: Norman 1980, p.4.

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Paper II

The Social Scientist in Farming Systems Research

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THE SOCIAL SCIENTIST IN FARMING SYSTEMS RESEARCH[1]

1. Introduction

In the last 10 years, social scientists concerned with agricultural technology have achieved a notable advance. They have made the transition from the role of external critic of technology and its effects to the role of partner, working side by side with natural scientists in the design and diffusion of new agricultural technology, especially for the poor.[2] Along the way there have been some important successes, particularly in the development of methods for holistic, multidisciplinary farming systems research (Perrin et al. 1976; Byerlee, Collinson et al. 1980; Shaner et al. 1981; IRRI 1984). However, there have been difficulties in implementing these new methods. Many social scientists have returned chastened and bloodied from their experiences in agricultural research stations, to write, with varying degrees of euphemism of 'institutional difficulties' (Biggs 1982), 'creative conflict' (Rhoades and Booth 1982), 'internal friction' (Galt et al 1982). These experiences seem to be universal: as Galt et al note, 'the integration of farming systems research into established commodity-oriented research programmes will never be free of friction' (1982:39-40).

The difficulties experienced by social scientists trying to integrate their work with that of natural scientists in agricultural research stations has become an object of study in its own right. Attention has focused on the communication difficulties experienced by scientists of different backgrounds (Horton 1984; Flynn and Denning 1982); on the professional incentives and rewards open to specialists in multidisciplinary programmes (Horton 1984; Biggs 1978); and on the decision-making processes in agricultural research institutions (Biggs 1982; Collinson 1982; Biggs and Clay 1983). Linking these issues together, Biggs and Clay conclude that the structure, organisation and control of agricultural research systems are now of 'central concern to agricultural research' (1983:4).[3]

The present paper is intended as a contribution to the debate about this central concern. It sets out to describe the ways in which social scientists run into difficulties on research stations; it proceeds to a discussion of why these difficulties arise; and it concludes with some suggestions for the various actors involved as to how these difficulties can be overcome. The main perspective taken is that of the social scientist, who operates in a highly constrained environment; and the main conclusion of the paper is that, despite a number of surface manifestations, the underlying conflict is one over the objectives and organisation of the research station. The issues raised by the introduction of social science and of a

multidisciplinary approach to farming systems research are fundamental and are unlikely to be resolved without substantial restructuring.

It is important to begin with an account of the kinds of conflict that can arise and we shall therefore introduce 'George', a composite agricultural economist, whose experience mirrors that of many social scientists engaged in agricultural research.[4] George was in his mid-twenties when he was appointed to join the foreign aid team attached to one of the regional agricultural research stations in a low-income country. He had a first degree in Geography, a postgraduate qualification in agricultural economics and previous field experience in project planning. This was his first experience in agricultural research and he had left home with some trepidation, but excited at the prospect of working with agricultural researchers and becoming involved at an early stage in the generation of new technology.

George's terms of reference had been prepared by the headquarters of the aid agency and cleared with the host government. As the first agricultural economist to be appointed to the research station, his job would involve setting up an economics unit and contributing in all relevant ways to the work of the station. Quite what these ways were was not spelt out in detail, but there had been talk of farming systems research, and George hoped that he would be able to work in a multidisciplinary context. In addition to his formal terms of reference, there was also what might be called a 'hidden agenda': before leaving the headquarters of the aid agency, George had been briefed about the importance of orienting research to the poorest farmers and told that, in the agency's view, there was considerable scope for improvement in this respect. They hoped he would contribute to some reordering of priorities.

When he arrived in the country, George found, as he had expected, an institution that was organised largely along disciplinary lines. Around 15 specialists worked under the director, dealing either with individual crop or livestock enterprises or with specialities such as soils or pest control. Foreign advisors were attached to some of these. The institution was poorly funded and relied on aid projects for over half its total budget; the director clearly spent a good deal of his time securing such finance and maintaining good relations with his constituency in the government and in the agricultural community.

George's colleagues on the aid team were older and more experienced, averaging 10 years or more in agricultural research. George found them all to be extremely friendly and hospitable, but somewhat sceptical about the value of his post and the contribution of an economist. Nearly all of them had done some economics at University and used some economic tools in their own busy programmes. Nevertheless, they were happy

to spend time with George and to explain to him the details of their own, very full research programmes. It quickly became apparent to George that he was dealing with skilled professionals. He also realised, however, that their work was of the type he had been trained to consider 'traditional': most of the investigation was being carried out on the research station or in a controlled environment on large farms and very little was being done off-station. Furthermore, there seemed to be little in the procedure of the research station that dealt holistically with the farmers in the area: there was one annual research meeting at which the different programmes submitted their research plans for the subsequent year and these were discussed in isolation from each other. It seemed to George that he had an agenda for change, if only he could manage to implement it.

Over the next year or so, George set about establishing his economics unit and developing his agenda. In some ways the work went extremely well. He had a bright, young counterpart with whom he worked very well, and together they carried out a good deal of field work on the farming systems of the area. Both George and his counterpart enjoyed the long talks they had with farmers and were pleased with the understanding of farming systems they felt they had developed. At another level, however, things did not go so well. As George began to study the farming systems and apply his economic training, he found himself increasingly dissatisfied with the content and results of the research programme. Much of the work being done did not seem to him to address the main problems of the poorer farmers in the region and, furthermore, the form of the output made it very difficult for him to reach general conclusions about the impact new technologies might have. For example, it was difficult to carry out economic analysis of trial results, because so little of the experimentation had been done on farmers' fields under farmer management conditions. George expressed these frustrations, of course, both to his team leader, his aid colleagues and his counterpart. He felt that the institution needed a shake-up and did not fight shy of saying so. Not surprisingly, this message was not always well received. As George's frustration grew, his relations with his colleagues also began to deteriorate. The easy sarcasm he had noted in his colleagues in the first week began to develop a bitter edge and the openness he had admired on his arrival became less evident. It seemed to him that there was more criticism of his work and less willingness to discuss ideas or share results before publication. When George or his counterpart did carry out economic analysis of trial results, it was often ignored or side-stepped. George and his counterpart began to feel increasingly alienated from their colleagues and regarded as mavericks by the rest of the team.

At this point, outsiders who visited the project from the aid agency headquarters noticed that George would talk very fast and in great detail about the difficulties of working on the

project and the old-fashioned and narrow-minded attitudes of his colleagues. They also heard that he was becoming impatient, aggressive and quick-tempered and that he was considered a somewhat disruptive influence on the team. Things might have deteriorated rapidly, had not an evaluation mission arrived at about that time. George decided to lay his cards on the table and he set down on paper his views on four separate topics which he felt needed attention by the research team: concentration on poorer farmers; the use of a farming systems perspective; greater reliance on on-farm trials; and the incorporation of economic analysis into the definition of research priorities and the preparation of recommendations.

On this occasion George was fortunate: his appeal fell on receptive ears and, after a good deal of deliberation, the evaluation mission decided that a farming systems perspective should indeed be given greater prominence in the work of the station. This news was not welcomed by all members of the team, but it was decided that there should be a pilot project in farming systems research and George threw himself enthusiastically into this. A national member of the research team was placed in charge and over the next months, limited finance was secured and a programme of on-farm trials was established. The difficulties were enormous, as everyone had predicted. The lines of communication between the head of the pilot project and the leaders of disciplinary teams were not always clear and there were several disagreements about control over research programmes in the project area. Furthermore, the difficulties of running on-farm trials were very great, requiring considerable time and money to resolve. The results were not always satisfactory. Nevertheless, George felt that some progress was being made.

It was not long, however, before a new round of problems began to appear. The pilot project was dependent on external finance and this became uncertain. Furthermore, it became apparent that the pilot project operated as an isolated unit within the institution and that the various procedures it had pioneered were not being adopted on a wide scale by other parts of the research programme. George could not see how the format of the pilot project could be extended and he began to feel that it had been a sop to keep him quiet while other parts of the programme continued much as before.

At this point, George's contract expired amd he left the programme. He felt a certain dissatisfaction with his role and with what had been achieved. He saw the problem very much in individual terms and felt that a combination of personalities and personality conflicts had impeded progress. Had he remained in the country, he might have seen more changes taking place in the institution. However, it was clear to him that his own departure would probably ease conflict within the institution and that the process of change, which had been slow enough during his stay in the country, would proceed faster if he left.

George's dissatisfaction is not unique. As noted above, he is a composite figure whose experience reflects that of many individuals engaged in agricultural research. Not everyone follows the same path: some, for example, fall at a point before any kind of pilot project is established; others may go on successfully to change the whole institution. But whatever the precise outturn, the pattern of frustration and conflict is common. It is necessary to try and provide alternative explanations, and to this we now turn.

2. Five Explanations

(a) Introduction

It is clear from the story of George that a number of different kinds of problem are encountered by social scientists in agricultural research institutions, some perhaps of their own making and some outside their control. In reflecting on the experience of George, five main, interrelated explanations can be suggested for the series of difficulties he encountered. These do not amount to models of how the institution operated, but each reflects a school of thought. The five explanations are as follows:

(i) personal inadequacy;
(ii) interdisciplinary communication barriers;
(iii) poor group dynamics;
(iv) inadequate structure;
(v) power struggle.

(b) Personal inadequacy

Personal inadequacy is the first and obvious explanation for why George failed to influence the research station. This is not an item discussed in the literature on social scientists in agricultural research, but it is an aspect that arises with great frequency in the field. It is pointed out that George is young and inexperienced; that he has no degree in agriculture and is therefore in some ways technically incompetent; that he is unnecessarily assertive or aggressive; and that he has a typical social scientific bias towards academic issues or social concerns. The hypothesis is advanced that a different social scientist without these various defects would have encountered less hostility and been more successful. Sometimes 'success' would mean achieving the various items on the agenda without conflict; at other times, it implies not trying to achieve them at all and fitting into the pre-existing organisation.

As the story shows, George is also inclined to levy accusations of personal inadequacy against his colleagues: they are traditionalist, technocratic, rigid. If only the agronomists on the team had been more flexible, then all sorts of advances would have been possible.

There is probably something in these accusations on both sides. No doubt internal friction would be less if the various individuals concerned were all optimally qualified and had optimal personalities for the task. Nevertheless, it is unlikely that they provide a complete explanation and it is more appropriate to treat them as residuals. There is an analogy with the discussion that often arises on research stations about the reasons why farmers do not adopt new technology and about the importance of 'good management': although different levels of skill may well be a factor in explaining the differences between farms, it is often sensible to explore other possible reasons first, including access to inputs, the profitability of the technology, its riskiness and so on. Individual idiosyncracy is there a residual; and so it should be in this case.

(c) Interdisciplinary communication barriers

The difficulty of integrating different disciplines is a factor much stressed in the literature. Horton, for example, refers to the difficulties resulting from the use of jargon by different disciplines (Horton 1984); and Flynn and Denning identify the integration of research from different disciplines as one of the main 'challenging issues' in agricultural research (1982:13). Particular attention has been paid to the problem of 'incentives and rewards': to the fact that traditional professional criteria for individual advancement and success may not be applicable in a work environment which stresses multidisciplinary, team activity (Biggs 1978).

Part of the problem stems from differences in training between social scientists and natural scientists. It is difficult to generalise about 'social scientists', a category which, in this context, may include agricultural economists, sociologists and social anthropologists. However, Chambers (1983) suggests that, as a general rule, social scientists are trained negatively, to 'evaluate and criticise'; whereas natural scientists are trained positively, to 'interpret and act'. This difference in training will lead to differences in behaviour: academic social scientists 'seek problems and criticise, practitioners seek opportunities and act' (p.33). Rewards are structured accordingly.

As regards economists, it is worth noting that this problem is not confined to agricultural research. Flynn and Denning quote a study by Rossini et al (1978) to the effect that 'in a number of cross-disciplinary studies, economists were regarded as the most difficult scientists with whom to communicate. They use jargon; are preoccupied with models and methodology which are not seen, in a practical sense, to contribute directly or obviously to problem solving; often make insatiable demands for data without output within project time

targets; and are unable, or unwilling, to consider non-economic goals in the arguments' (p 14).

Most agricultural economists in the rural development area would deny these criticisms, certainly as far as the use of jargon and the place of non-economic goals are concerned. However, they would surely agree that there are differences in style and approach between social scientists and natural scientists; and that these are thrown into sharp relief when the two work side by side in agricultural research.

These differences can be explored by comparing, in Figure 1, the way in which social scientists see themselves with the way in which they are often seen by natural science colleagues. For example, social scientists see themselves as being policy-oriented and concerned with the immediate costs and benefits of new technology; natural scientists often consider this obsession with finance to be ephemeral, when the really important relationships are physical input-output relationships. Similarly, social scientists consider that they use flexible research methods in order to obtain the greatest possible insight at the lowest cost; natural scientists, familiar with statistical methods in the design and analysis of trials, are inclined to consider that these methods lack rigour. There is also very often suspicion of the holistic view taken by social scientists, which is seen as tending to social work, and with the amount of time they spend in informal discussions with farmers and farm families, which is seen as a form of rural development tourism. These problems often appear more acute for sociologists and anthropologists than they do for agricultural economists. A parallel set of misconceptions or suspicions will exist on the other side of the fence.

If we look at George's experience, we can see that these misunderstandings hampered his relationship with the natural scientists at several points. The amount of time spent in accumulating information about farming systems, using informal methods in the tradition of 'rapid rural appraisal' (Chambers 1983), was not properly explained by George and not clearly understood by other members of the research team. The emphasis on poorer farmers with a low marketed surplus was regarded as a secondary interest which did not contribute to major increases in food output. The obsession with economic analysis was seen as misguided at a time when costs and prices were changing relatively fast. The farming systems perspective was seen as introduirrelevant to the main thrust of the research programme. When all these issues were raised, George's position was made more difficult because he was both inexperienced and the only social scientist on the project. At many stages it was difficult for George to retain faith in his own research procedures. Nevertheless, he tended to become defensive about those procedures and therefore lost the opportunity to benefit from some of the advice of more experienced colleagues.

Figure 1

Sources of poor communication between social scientists and natural scientists

Social scientists as seen by	
Social scientists	Natural scientists
Flexible research methods	Lacking in rigour
Policy-oriented	Ephemeral
Good communicators	Verbose
Holistic	Social workers
Field-oriented	Rural Development Tourists
Aware of political constraints	Insurrectionist

It is not easy to see how these communication barriers could be eliminated in the context of an operational research programme. Some suggestions are made below. However, most observers have noted that conflicts caused by poor communication between disciplines tend to disappear as scientists with different backgrounds work together in the field on concrete tasks. This is a clear case of learning by doing, once the initial barriers have been breached.

(d) Poor group dynamics

Poor group dynamics is another common explanation of friction in research programmes, even where personal inadequacies or interdisciplinary communication barriers have been eliminated. It is related to explanations based on analysis of conflict within the station and frequently ascribed to poor leadership. It is worth saying something briefly about each of these three: group dynamics, conflict and leadership.

As regards group dynamics, Handy (1981) has suggested, in a useful and readable summary, that a model for analysing the effectiveness of a group should begin by looking at the characteristics of the group itself, the environment in which it works and the task it has to undertake; from there it is possible to proceed to a study of the way the group is led and of the procedures it adopts, in order to arrive at an assessment of the productivity of the group and the satisfaction of those involved in it. He suggests (p.160) that groups go through the following sequence: forming, storming, norming and performing. In the first stage the group is beginning to be established, in the second it enters a phase of conflict, in the third it establishes new norms and practices and finally, it begins to act cohesively - 'only when the three previous stages have been successfully

completed will the group be at full maturity and be able to be fully and sensibly productive' (1981:161). The speed with which these stages are gone through and the success with which the final tasks are undertaken will depend in part on the characteristics of the group. Handy notes that a group size of greater than seven will diminish effectiveness and that 'people who are similar in their attitudes, values and beliefs tend to form stable enduring groups' (1981:156).

Arguments, competition or conflict are inherent in the working of groups. Conflict may arise for a number of reasons to do with substance or ideology and may involve different tactics, such as the control or distortion of information or the control of rewards. The literature on conflict and conflict resolution has focused on ways in which managers can control conflict, for example by setting rules or by arbitration. As seems to be implicit in the description of group work summarised above, an important place is given to confrontation, a kind of controlled explosion in which conflict is allowed out into the open and then channelled productively. This is analogous to the method used in psychotherapy of 'breaking open the client'.[5]

Whether in group work or in managing conflict, the role of the manager or leader is of great importance. Handy distinguishes between 'structuring' and 'supportive' styles of leadership, but notes that the appropriate leadership style will depend on the task to be carried out and on the environment. For example, problems with open-ended solutions, such as research, will best be tackled with a supportive leadership style. Supportive or participative styles also take longer, so that short time horizons may enforce a more structured leadership.

Can we apply these various ideas to the problems faced by George in his experience on the research station? Many of the disagreements or disputes with the natural scientists conform to the model of group interaction and conflict. An important point to make here is that George was a late arrival on the scene. Both the national research team and the foreign aid team were well established when he arrived and it can be assumed that as a group they had already gone through most of the tribulations associated with 'forming, storming and norming'. George's problem was then how to break into an established group that was already 'performing' according to its own criteria. It is not surprising that this was a difficult process and that there was a good deal of resistance.

When George began to introduce new ideas and try to establish his point of view, this was seen as going beyond the normal bound of argument and opening up new conflicts, especially about the objectives and ideologies of the work. On the whole, the conflict was not managed, other than by trying to suppress it, so that frustration built up on both sides. When the pilot project was set up at the time of the visit of the

evaluation mission, this did not result from confrontation but rather from the use of an external agent, discussed below. There was no discussion by the team of the fact that a situation of conflict existed and no steps were taken to describe and resolve the conflict.

For this omission, it may be possible to attribute some blame to the team leadership, both on the national and the aid side. The leadership style adopted was on the whole supportive, in the sense that the individual research programmes of specialist researchers were not determined in detail by the project management. However, there was also relatively little staff participation in the formulation of overall project goals, so that in one respect the situation corresponded more to one of structured leadership. It is worth pointing out that the team leader in George's case, and in most similar cases, will himself be a natural scientist with some research interests alongside his administrative duties. To the extent that conflict arises, therefore, the leader may find it difficult to stand above the fray and may side with the natural scientists.

We can see that a good part of George's problem can be explained by a failure of group dynamics, with the attendant weakness of conflict management and team leadership. However, George's agenda was a substantial one and it is likely that the changes he sought would go beyond the normal bounds of group dynamic. This is why it is necessary to consider problems of organisational structure.

(e) Inadequate structure

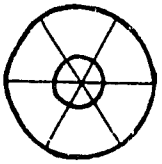
As in the case of previous items, there is both a general literature on the subject of organisational structure, and a branch of this literature which deals with the particular problems of agricultural research. In general terms, a distinction has been drawn between different kinds of cultures and associated institutional structures. The 'power culture' is often dominated by one individual and is pictured as a web, with the dominant individual in the centre and the web around him. The 'role culture' is bureaucratic in nature and is pictured as a Greek temple with a number of pillars topped by a pediment of senior management; the 'task culture' is job or project oriented and is best represented as a net with no focal point, but a number of working groups based around individual tasks. Finally, the 'person culture' is an anarchic system in which the individuals work independently of each other (Handy 1981:chapter 7). See Figure 2.

In agricultural research, it has been noted that traditional structures are of the 'role culture' type with strong individual subject matter departments; but that new approaches feature team problem-solving or forms of 'matrix management', which are closer to the task culture. Flynn and Denning

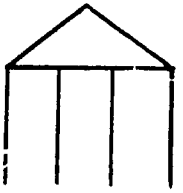
Figure 2

Four organisational cultures

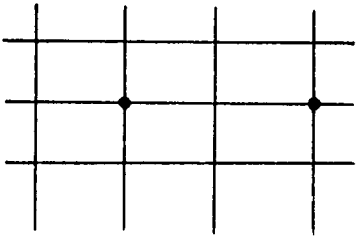
(a) Power culture



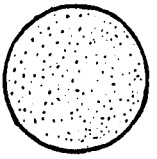
(b) Role culture



(c) Task culture



(d) Person culture



Source: Handy 1981:ch 7

(1982) describe these different models and their application in the international research system, particularly in IITA and IRRI. They note that in IRRI a task culture predominates: a total of nine research programme areas draw on the resources of 18 scientific departments in a series of multidisciplinary teams whose coordinators have 'limited administrative or scientific control over the assigned programme' (1982:7). They note some difficulties with the leadership of this kind of programme and draw attention to the IRRI cropping systems project as a more strongly controlled and therefore more successful programme.

If we return to George's problem we can see that part of the difficulty lay precisely in the structure of the organisation. The structure which George encountered when he arrived conformed to the role culture model in which the head of research managed a series of autonomous specialist departments. Some of the departments, as noted earlier, were

concerned with enterprises and some with topics, so that there was some coordination between them and a limited intrusion of task culture. However, the department heads were the key actors in the research process and resources were channelled directly to them.

George's agenda would have been difficult to implement within the prevailing role culture, especially for someone who was not in the management 'pediment'. The emphasis on poorer farmers and the introduction of on-farm trials were matters affecting the objectives and method of operation of the institution as a whole and could not be imposed by a relatively junior person occupying a position in one of the pillars of the temple. The introduction of economic analysis required an intervention from one pillar in the affairs of another pillar and was bound to cause conflict. Finally, the introduction of a farming systems perspective would require a shift from a role culture to a task culture and therefore a restructuring of the whole organisation.

What happened was that the structural issue remained unresolved until it was decided to set up a pilot project in farming systems research. When this was done, however, a further constraint was built into the system since the pilot project was to be carried out in the context of a role culture. Essentially, what was done was to add one extra pillar to the temple and place one of the disciplinary specialists at its head. This specialist had no control over the research carried out by the members of other pillars and therefore had very little control over the physical operations of the pilot project. Furthermore, the pilot project could be isolated from the rest of the organisation and, indeed, was so isolated because the person appointed was not the most senior or influential of the first line managers.

As Flynn and Denning suggest, a farming systems approach benefits greatly from the use of a task culture with some form of matrix management. The situation may be more complicated still because of the need to accommodate three kinds of specialists: those concerned with enterprises, those concerned with subjects and those concerned with areas or farming systems. We return to this problem below, but first turn to the final explanation of George's problem, which has to do with a power struggle.

(f) Power struggle

The notion of power and control is implicit in much of the earlier discussion about group dynamics and structural changes. Had George been team leader of the aid team or manager of the national research team he might have been able to carry out his agenda more successfully: he would have been able to manage the conflict or restructure the organisation by virtue of his position. In fact, he had no such position and, as noted, was one of the most junior and least experienced

members of the aid team. Nevertheless, the changes he was proposing required a fundamental restructuring of the organisation. In an obvious sense, therefore, George was engaged in a struggle for power. This is something not generally recognised in the literature on agricultural research, although there is discussion of power and influence in the general management literature.

Handy stresses that power is a resource which can be used to influence events in an institution. There are a number of sources of power, including physical power, resource power, position power, expert power and personal power. These can be used in order to exert influence. As Handy comments, 'anyone contemplating, or involved in, a process of influence needs to reflect upon his source of power, and thence the range of methods of influence that it suggests' (Handy 1981:128).

If we look at the sources of George's power, it was clearly not physical, nor was it positional power given his status in the organisation. His personal power, the power to persuade, was severely constrained; his expert power extended only to the field of agricultural economics and not to the management of the organisation; and his resource power was non-existent, given his membership of a centrally controlled team. It is not surprising, therefore, that George was unable to bring about the changes he thought necessary during the early part of his experience in the research station.

When the evaluation mission arrived, the situation changed dramatically. George was able to use personal power to influence the mission, which in turn set out to force changes in the institution that would otherwise have been impossible. The 'personal power' was simply the power of the argument for the changes proposed, which were anyway among the priorities of the headquarters of the aid agency. Once the mission had become convinced that changes were needed, it was able to write a report that required changes in the objectives and activities of the institution. This is a power not recognised by Handy, but one which must be important in many agricultural research undertakings: the power of the external agent or 'external power'. Associated with this external power was resource power, since there was agreement to fund a pilot project in farming systems research. Thus, George was able to achieve his short term objectives, though not his long term structural objectives, by mobilising political and financial support from outside the institution.

It has to be said that this is a strategem of last resort, since the use of external power does little to improve the dynamic of the research group. It is a method of influence based on force, not persuasion. However, once persuasion has failed, the agricultural economist in George's position has little with which to bargain and may be forced into this device. Of course, there may be problems in the long run, since the instruction of the aid agency headquarters or of the

evaluation mission will be less effective than the conversion of the researchers themselves. As Handy notes, attitude changes are much harder to bring about than behaviour changes (1981:136): the use of external force may well change behaviour without changing attitudes.

(g) Conclusion

It is an important conclusion that the structural changes required in the research system in order to implement holistic and multidisciplinary farming systems research cannot be made without power and that the agricultural economist working in such an institution should be aware of the need to work within the constraints of his own sources of influence. Others have pointed to the importance of strong, committed leadership in farming systems research programmes, but the practical problem faced by George and many others is that no such commitment exists. Collinson (1982) has suggested that what is needed in these cases is a bottom-up approach in which individuals work through research stations in order to bring about change, rather than attempting to impose change from above. It should be clear from the foregoing discussion that if individuals engage in this bottom-up approach, then they can expect to be involved in a series of conflicts over structural matters that will in the end result in a power conflict.

3. Practical Lessons

The preceding sections were largely descriptive: first, of what actually happens when social scientists are assigned to agricultural research stations; and secondly, of the reasons why conflict seems so often to appear. It is now necessary to be prescriptive, and to ask what practical lessons can be learnt. Given the primary concern of the paper with the role of the social scientist, we begin with ways in which the social scientist might act to reduce conflict or the damage caused by conflict. However, there are also important lessons for the colleagues of social scientists and for policy-makers and administrators, who are in some ways responsible for the occurrence of conflict in the first place.

(a) Lessons for the social scientist

It seems that a degree of conflict can be expected when a social scientist enters an agricultural research environment. Conflict may extend to the objectives and structure of the institution, to the method of working or to programme content. It is likely to be worse for sociologists and social anthropologists than for agricultural economists; and worse, too, if the social scientist is the first to be appointed to the station, if the structure of the institution conforms to a role culture, if the existing staff are already performing well as a group, and if leadership is structured rather than

participative. In the best of cases, there may be no conflict at all: but this may be the exception rather than the rule.

Personal inadequacy is unlikely to be the major cause of conflict. However, social scientists cannot expect to be successful members of a research team if they are inadequately trained, excessively arrogant or politically insensitive. It follows that the intensity of conflict will be reduced if social scientists are equipped not only with professional skills, but also with the skills required to operate effectively as members of a multidisciplinary team. Training in team work, including communication skills, would be a useful addition to social science training programmes.

On a related point, it is very rarely the case that social scientists are trained to deal with conflict; indeed, it is rarely the case that they are even told that conflict is likely to arise. The feeling of frustration, alienation and desperation that afflicts social scientists as they become enmeshed in conflict, believing that they are the first to experience this problem, should not be underestimated. There is a strong case for better training of social scientists in conflict resolution, both at the beginning of their career and as it progresses. There is also scope for stronger networks to provide mutual support between social scientists in agricultural research and for more exchange of information and experience, particularly with regard to institutional problems.

Social scientists can help to reduce communication barriers between disciplines. They can develop a greater understanding of natural science research methods and participate more actively in field experiments. They can take more pains to explain their own research methods, avoiding the use of jargon and emphasising the practical relevance of their results. A good library of farming systems research materials will be useful, as will good, practical examples of how farming systems research has helped to improve research programmes.[8] Contact with and visits from specialists in the field can help, particularly by people with a natural science background. Lectures, seminars or training courses may be appropriate. Collaborative fieldwork is often helpful, perhaps in the context of a diagnostic survey or a case-study programme (Biggs 1983; Maxwell 1984). Finally, some social scientists have established their own on-farm trials as a way to generate interest in multidisciplinary research.

Social scientists should be sensitive to the state of group dynamics on the team, particularly if they are late arrivals. If a group is already performing well, it may be possible for innovations to build on successes already achieved: on-farm trials, for example, may be seen as a logical next step in research procedures, rather than as a radical departure. Similarly, greater concentration on the poorest farmers may be achieved without discounting or belittling past research,

which may have had a different emphasis but which will still contain many valid conclusions. Incremental change will be easier to bring about and less disruptive than radical change: this implies that priorities should be set from the agenda and new issues taken up one at a time. Collinson (1982) makes the important point that the social scientist will often be able to cooperate well with one or two members of the natural science team: the social scientist will want to build up alliances and work slowly towards the eventual objective.

In the last analysis, however, these measures, though necessary, may be insufficient. As noted above, the introduction of a new farming systems approach may be difficult to bring about without a fundamental change in the objectives, culture and structure of the research institution. It may be impossible to avoid radical change and the associated conflict.

If the social scientist is to become involved in bringing about radical change, then it will be helpful if the job description or terms of reference are clear as to the objectives of the assignment and how the objectives are to be reached. 'Hidden agendas' of the sort provided to George place the social scientist in an invidious position vis-à-vis colleagues in the field and cause unnecessary difficulties for project management. Often these ambiguities can be faced before appointment; alternatively they can be addressed explicitly when a work programme is prepared. The structural implications of social science objectives should, if possible, be brought into the open for discussion between the social scientist, project management and agency headquarters.

This may not always be possible, and even if it is, residual conflict may remain. The social scientist should therefore plan for conflict resolution. Potential sources of difficulty should be identified at an early stage and strategies should be developed to overcome them. This requires the social scientist to have a clear idea of eventual objectives and the way these are likely to affect the working of the institution. That is to say, the items on George's agenda, which developed over a period of time, should be listed and thought about at an earlier stage. Priorities should be set and Handy's advice about reflecting on sources of influence should be followed.

As a last resort, it may be necessary to cause a row, to 'break the client open'. As Biggs (1984) notes, 'it is difficult to ensure that these conflicts come into the open and are reconciled'. Bringing conflict into the open may be easier than ensuring that conflict is eventually reconciled and this will be true particularly if the social scientist is not in a position to manage the conflict. The social scientist will want to think carefully about when to cause a row and about the range and implications of possible outcomes.

To conclude, it is difficult to prescribe for individual situations which vary greatly. The social scientist has to feel his or her way through a potential minefield of personal, institutional and power conflicts in order to achieve the objective set. Although this does require great sensitivity, the chances of success can be improved by a study of the likely difficulties and the ways to overcome them.

(b) Lessons for the colleagues of the social scientist

For those who are not social scientists, the analysis and the prescriptions produced so far must seem Machiavellian. By what right does any social scientist set out to subvert the objectives and mode of operation of an agricultural research system, fermenting conflict and undermining the established leadership in the process? Surely the correct thing to do if the social scientist wishes to introduce changes in the institution is for these changes to be suggested: if they are rejected, then the social scientist should either perform the task defined in the most efficient way possible or leave.

The problem is that structural change is often a precondition for the efficient performance of the 'task defined' for most social scientists: it may simply not be possible to introduce an effective farming systems methodology unless the organisation can be moved from a role culture to a task culture. The need for structural change is implicit in the appointment of a social scientist, who cannot perform effetively until the need is made explicit and acted upon. There is a contradiction between the job descriptions of different individuals in the institution, and this contradiction is unlikely to be resolved quickly or easily.

There is therefore likely to be conflict between social scientists and natural scientists and between social scientists and research managers. It will be helpful for natural scientists as well as for social scientists to have an understanding of the underlying causes of the conflict and to escape as far as possible from personality conflicts into a discussion of the structural issues involved. Discussion will be more productive if it can move away from the personal inadequacy model towards the group dynamic, structural and political models discussed above. It would be particularly useful if natural scientists were able to examine their group dynamic in order to incorporate late arrivals in the form of social scientists; and if the structural consequences of economists' work on trial results could be addressed in the context of research-extension linkages. As in the case of social scientists, there are clear training needs for natural scientists. These should cover the rationale and context of social science methods as well as the training in communication skills and institutional analysis that was recommended for social scientists. Ideally, natural and social scientists should explore these issues together.[7]

For the agricultural research manager or team leader, there is a particular problem associated with the recruitment of social scientists. This will be the case whether the social scientist was requested by the research manager or imposed from outside. It is important to realise that there will be inevitable communication problems between different disciplines, that the existing operation of groups will be disrupted and there may well be structural or political problems associated with the method of operation of the programme. As many observers have noted, all these problems can be reduced if the leadership at the centre is participative rather than coercive and if it is convinced of the desirability of a multidisciplinary and holistic approach. In these cases, training, group reorientation and some structural change may be necessary in order to incorporate and assimilate the work of the social scientist. In cases where the leadership is not entirely convinced of the value of the social scientist, it will still be possible to reduce disruption to the programme by focusing on the management of conflict and seeking to resolve conflict in the most peaceful way. Such accommodation may be particularly necessary if the social scientist is young and inexperienced.

(c) Lessons for higher level policy-makers

There is an important series of lessons for policy makers and aid administrators. Very often, it seems that the disruption and conflict caused by the introduction of social scientists have been the result of a failure to define objectives and methods of operation in a consistent way for research managers on the one hand and social scientists on the other. It has not been realised that a social science component and a multidisciplinary perspective require institutional change as well as programmatic change.

If this is the case, policy-makers need to plan simultaneously for the development of both programmes and institutions. They or their advisers will need to decide what kind of institutional structure best suits the programme they wish to see adopted; and they will need to take steps to see that this structure is implemented. At the very least a package of management consultancy and staff development is likely to be required. In extreme cases, it may be necessary to replace individuals and impose new management, committed to a new structure.

None of this will be easy, and change will often be resisted. To quote Biggs again, 'in scientific programmes, groups use the programme to meet their own special interests and establish procedures to protect those interests' (Biggs 1984). However, the greater the resistance, the more important it is to address the structural question at a policy level, rather than leaving it to be fought out between individuals within the institution.

Whatever the structural question, policy-makers can help to avoid conflict by helping to break down interdisciplinary communication barriers and strengthen the group dynamic. For example, it is particularly helpful on a new project for natural and social scientists to begin work simultaneously, so that the process of 'forming, norming and storming' is truly multidisciplinary. Similarly, 'external power' can be used to encourage the research group to produce its own solutions to the structural problems, rather than imposing solutions from outside: participative management at a higher level. Funds should be provided for special training seminars, study tours and the development of support groups. Policy-makers will also have a role in the management of conflict, by setting rules or offering arbitration; changes in the system of rewards and sanctions will help to encourage the resolution of conflict.

The point of all these suggestions is to reinforce the conclusion that conflict does not usually occur capriciously because of poor recruitment or personal failing. The difficulties faced by social scientists, their colleagues and their managers are real and substantive: there is a burden on policy-makers or aid agencies to understand and alleviate them.

4. Conclusion

This paper has been concerned with the introduction of social scientists to agricultural research programmes, particularly in the context of farming systems research. It began by noting that this process was often associated with conflicts of different sorts and set out to analyse the source of those conflicts as well as possible action that might be taken to avoid them. It concluded that structural and political conflicts were very likely to arise, in addition to the more usual difficulties of inter-disciplinary cooperation and group dynamics. These conflicts should be recognised as inherent to the process of introducing multidisciplinary research and should be accommodated in planning for such research.

However, a good deal more work is needed on this subject, as has been noted by others (Biggs and Clay 1983; Flynn and Denning 1982). As Flynn and Denning point out, most of the work dealing with this topic is based on anecdotal evidence, and the same criticism can be applied to this paper. It would be particularly useful if cross-country and cross-institutional research could be carried out in order to identify the conditions under which conflict is most likely to arise. For example, is the international system more prone to conflict than national systems? Or are commodity stations more prone to conflict than those which cover a number of enterprises and farm systems? Do economists run into more difficulties than sociologists or anthropologists? To what extent are the problems 'one-off', occurring only when a

social scientist is first appointed, and to what extent are they a continuing source of tension? If these questions could be answered then there would be potential for guidance to social scientists, natural scientists and research managers that would minimise or eliminate the costly effects of interdisciplinary conflict.

In the meantime, social scientists will continue to be deployed in agricultural research stations and will continue to find themselves engaged in the kinds of conflicts described above. Even without the results of more detailed research, there is a great deal that can be done to equip them to handle these situations more effectively than is at present the case. At the very least, social scientists and social science training programmes should be made aware of the likely problems inherent in multidisciplinary farming systems work. Those preparing social science projects or terms of reference for social scientists should be aware of the dangers of conflict and should either take steps to avoid it or brief the social scientists accordingly. Research managers should expect to have to deal with a certain amount of reorientation when social scientists are assigned to their projects.

Though some conflict will remain whatever action is taken to improve training or the administration of research, it is important to end by emphasising that the benefits of multidisciplinary farming systems research far outweigh the costs associated with the introduction of these new perspectives. Farming systems research is an extremely powerful tool in the development of appropriate technologies for poor farmers and the synergy that exists between social and natural scientists when farming systems research begins to work is very productive. A discussion of conflict should not obscure the fact that the end being sought is valuable.

Notes

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2. Many studies of the Green Revolution provide examples of the former: see, for example, Griffin (1974), Pearse (1980). For examples of the latter, see Norman (1978, 1980), Gilbert, Norman and Winch (1980).

3. Biggs and Clay provide a useful, analytical review of a number of models used to analyse the workings of R and D institutions, including those derived from political science, sociology and organisation and managment (1983:46ff.).

4. As in all good fiction, 'George' bears no resemblance to any social scientist, living or dead. But as in all good 'faction' he will be vaguely familiar to many. He is, in fact, based on participation in or observation of, agricultural research in at least four countries with widely differing agricultural systems and research institutions.

5. I am indebted to Des Hogan for this analogy.

6. Recent reviews of farming systems reearch include Gilbert, Norman and Winch (1980), Biggs (1982a), Casey and Barker (1982). See also the working papers of the CIMMYT Economics Programme and the IRRI Research Paper series.

7. An excellent course for those at an early stage in their career is the International Course on Development-Oriented Research in Agriculture (ICRA), held each year at the International Agricultural Centre in Wageningen, Holland. A shorter course for professionals in mid-career is the Rural Research and Rural Policy Course held at the IDS in Sussex. Similar courses are run elsewhere.

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