THE DEVELOPMENT OF AN INTERDISCIPLINARY PROJECT

JAN BÄRMARK AND GÖRAN WALLÉN
University of Gothenburg

Problem and Perspective

Our objective is to draw attention to the development process within a research project, more particularly a case study of a forest project. The growth of knowledge in a research project can be seen as the result of interaction between cognitive and social factors. In order to study the interactions between intellectual, organisational sociological and psychological factors, we decided to study interdisciplinary research because we believe that it is easier to follow the interaction of cognitive and social factors directly in a project in which disciplines with conflicting paradigms, research roles and cognitive interests have to confront each other. There are differences between researchers' roles and the competence of theoreticians, empiricists, specialists and generalists which are as important as the differences between scientists from different disciplines.

In this paper we discuss, for example, the different motives of the researchers for entering the project, the basic differences in outlook and personality between empirical and theoretical scientists, and the effects of the existing academic career structure which creates a lack of solidarity within the group. Consideration of the development process within a project is also crucial to an understanding of the project. Research management approaches often imply that a project can be planned and organised once and for all in the beginning, and according to a simple goal structure. On the contrary, the organisation must be continually adapted to the development of knowledge, for example by using problem oriented groups instead of discipline oriented groups as in matrix organisations.

Correspondingly, knowledge cannot be integrated once and for all at the

Karin D. Knorr, Roger Krohn and Richard Whitley (eds.), The Social Process of Scientific Investigation. Sociology of the Sciences, Volume IV, 1980. 221–235.

end of the project simply by compilation of reports from subprojects. We have found that integration of knowledge is dependent on integration at different stages in the research process. Integration of knowledge in an interdisciplinary research group also implies that social integration must occur (1). Additionally, in our investigations of a number of projects we have found that it is necessary to have access to categories, perspectives and theories from the philosophy of science, sociology, the psychology of science and organisational theory. In this sense, studies of interdisciplinary science ought themselves to be interdisciplinary.

Our Methods

Our methods consisted of participant observation, close readings of reports, and interviews with scientists.

We can differentiate between three separate phases:

First, we formulated questions about problems concerning world-picture assumptions, ideals of science and research roles. The scientists, however, did not like to answer specific questions about metascientific problems but preferred to talk in general terms about their personal experiences with the project. They did not want to separate their way of handling metascientific problems from their own personal backgrounds, relations between their early interests in school, their ways of thinking and their specific interests in the forest project. They drew our attention to the importance of good interpersonal relationships in the research group as a prerequisite for interdisciplinary cooperation and to the importance of the organisational settings for the growth of knowledge.

Second, we asked more open questions about their experiences from the project. Why did they begin on the project? What background did they have? What factors had created their way of working with science? What roles had they played in the project? etc. From these interviews we obtained a great deal of information about cognitive style, personal experiences, different perspectives on the project and about the interaction of cognitive and social factors in the project. At the same time, our understanding of the project had deepened so much that we could ask specific questions about the different scientists' contributions to the project and this led us to the third phase of the interviews.

Third, we asked specific questions about the cognitive parts of the projects, the specific investigations, different aspects of model-making, problems in the process of integrating knowledge, etc. As we got different information from different researchers we gained insight into the biological problems, model-making, etc., which enabled us to reconstruct the history of the project. Cognitive elements, for example integration of knowledge, were dependent on social factors just as were ways of cooperation during the research process.

The Forest Project

The forest project is the oldest project we studied. It started about ten years ago, and it is also the largest, including 100 researchers and technical assistants.

There were different kinds of problems in the different phases of the project, so we have found it better to present our material in chronological order with our comments in each part of the presentation (2). There were five distinct phases in the life cycle of the project:

Phase One: The Initiation Phase (August 1970–June 1972)

One of the reasons for initiating the project was a desire on the part of some researchers to develop their experiences from the International Biological Programme (IBP) in which they had taken part. Another source of inspiration was a large ecological project on the Baltic Sea on the ecosystem level, i.e. studies of the flows of energy and matter in the ecosystem, rather than of individuals and populations. Thus a ecosystem tradition began to emerge. This view had a world picture component: the ecosystem perspective, and a methodological component: ecological knowledge has to be formulated in mathematical language. The link between the world-picture and the mathematical method was computer simulation. The ecosystem perspective was intended to unify the different disciplines.

Also some researchers thought that biological in Sweden was in need of renewal because the discipline of ecology was rather descriptive, especially compared to the North American parts of IBP. They were interested in introducing more exact methods and mathematical models. There were also many young biologists who had no university position and so were available for the project. One of our informants who came into the project as a young scientist and soon achieved a leading position told us:

> For me and many of my colleagues this was our last resort. We had to choose between this project or giving up an academic career. (3)

Subprojects were also initiated for doctoral students.

The forests in Sweden are very important to the economy and there were a lot of practical problems to be solved, for instance acidification of the soil, and fertilisation. The project was launched on the political level to solve all these problems by way of a large 'forestry management information system'. Ideas were borrowed from management information systems and the planning of the Apollo project. All these various motives are mirrored later on in many different goals of the project. The main part of this period was spent resolving conflicts in interests between different research bodies (4). Finally the project received special financial support from the Swedish parliament. The whole budget was about 27 million Swedish crowns (roughly 5 million U.S. dollars) for seven years.

Phase Two: Establishing the Project (July 1972–December 1973)

The project was, in this phase, mainly a cooperative project among researchers from different biology departments, some meteorologists and some data specialists. There were subgroups such as zoologists and microbiologists. The main problem was the formulation of operative goals and problems. The aim was to produce a mathematical simulation model for the coniferous forest which would show how the growth of trees varied with different inputs of temperature, water, carbon, nitrogen, etc. The model was also intended to coordinate the research work and make it into an interdisciplinary project.

At first, there were many seminars to teach the biologists systems analysis and mathematical models but many biologists found the mathematical methods difficult to learn. A single all-encompassing model was too complex to handle and they could not find any relation between the global model and their own fields. There was also some criticism that the model was a kind of black box model giving no explanations of causal processes. This was partly due to the aim of the model, which was to predict changes in the ecosystem without providing detailed explanations.

As no theoretical model that could guide the empirical investigations had been developed, the biologists went out into the field making empirical investigations on their own. At the same time, there was pressure from the research councils to start the empirical investigations at once. Both of these groups shared the conviction that 'real research' is synonymous with empirical investigation and not with tedious theoretical work making models. But there were also long delays in getting measurement equipment due to a shortage of money. Consequently, many of the earlier measurements could not be used: some had a wrong resolution of time measurement and other methods were incompatible. The whole project fell into a period of crisis during which a leadership crisis also arose.

As the first modelling approach failed, critics began to have an influence on the project and the project board was widened to a larger group including more of the empirical scientists. This large group, however, was inefficient in coordinating the project at the field level; many members of the boards felt more responsibility to their subgroups than for the project as a whole.

In retrospect, many of the scientists regretted rushing into this phase of establishing the project. It would have been better had a small group carried out theoretical investigations for a few years to prepare the whole project and to structure the empirical investigations. Such a planning period would also have made it easier to select researchers for the next part of the project. Many of the later difficulties are traceable to conflicts in this preparatory phase.

A Comment on the Psychology of Science

The reactions from the empirically-minded biologists towards systems theory are a reminder of the strong interrelations between intellectual and psychological factors. In accordance with Mitroff we found psychological differences between theoreticians and empirical scientists.

Mitroff states:

> Whereas the sensation type (the empirical scientist) may be too data-bound (he tends to go on collecting data forever because he is afraid to risk a generalisation that goes beyond the available data), the intuition type (the theoretician) may be too data-free; he may spin out a hypothetical conclusion in a minute that is based on no data at all. (5)

This is exactly what we found in our project; the first modelling approaches had no empirical significance and the empirical scientists collected long series of data in order to feel assured that the amount of data was enough to cover the complex empirical reality.

Some of the scientists had previous experience from interdisciplinary science and were not wholly unfamiliar with systems theory. They could see the idea behind system thinking but preferred to work without the models. The only heuristic value in the models, according to them, was that the models helped in structuring one's own thoughts. Although the very goal of the project was to produce models of the ecosystem, there was a lot of scepticism among many of the researchers.

The research leaders had not taken into account that learning systems theory is more complicated than just learning a new method or technique. The use of systems theory implies the adoption of new kinds of intellectual resources with new world-picture assumptions, preconceptions about science (criteria of knowledge, epistemology, etc.) and new research roles. Many of the scientists were not used to abstract reasoning in mathematical models. Their interest in attending seminars in order to learn a new 'paradigm' soon faded. They were interested in empirical investigations, measurements and the process of handling a lot of empirical data. So, instead of adopting new perspectives, they continued their research in the traditional manner.

At the beginning of our studies we unconsciously adopted the project leaders' view on the project. The project leaders had the most clearly articulated conception of the whole project and it was much easier for us to talk in terms of methodological problems and about research management problems to them than about what happened on the research field levels, for instance plant physiology, to scientists. Thus, in the beginning we thought that the more empirical scientists' refusal to learn systems analysis methodology and to take into account the global perspectives of the project was irrational. But later on, we changed our perspectives to that of the participants and realised that the models were too far removed from field work, so the previous competence of the researchers could not be fully exploited. The cost of learning a new competence was too high and was worthless as a skill when they had to return to their original disciplines after the project. Some scientists came from disciplines in which mathematics and statistics were looked upon with suspicion.

Phase Three: Empirical Dominance (January 1974–December 1975)

Problems of cooperation coupled with the difficulties mentioned above led to a reorganisation of the project in 1973. Various preconceptions about science were represented in the project. The first approach to making a global model of the ecosystem implied the construction of a mathematical model in theoretical biology which could be used for forestry management.

This preliminary model of the ecosystem was used to define certain problem areas (such as gas exchange, litter fall, consumption). However, the model was not developed to the point where it could be used to put forward specific questions. But these problem areas did form a basis for a change from co-operation between disciplines to problem-oriented groups and it was useful for planning purposes and for empirical work. For some time it also stopped the battle over resources between the disciplines.

At the beginning of this phase, the empirical researchers thought that this reorganisation would provide the working conditions which had been lacking and that they would have more opportunity to plan their own work. This change initiated the third phase of the project, with empirical dominance. The project was so large that it was impossible to steer the subgroups towards integrative work which led to a situation in which the actual field work was not particularly interdisciplinary. At the same time, the data measurement equipment at Jädraås, experiment-station, had begun to work. The technical equipment was considered to be one of the most important parts of the project and it was hoped that the multitudes of data could shed some light on the problems with the models and that a data bank would be created from an 'undisturbed forest' to be compared later with data from forests outside the experiment area. A lot of automatic measurement equipment was used and a special computer was used to steer this equipment and record the data. Integration was done at the planning level, on the theoretical side for the whole project and for the empirical work on an intermediate level in the problem-oriented groups.

Naturally, there were also advantages for the relative freedom of the empirical researchers, for instance the unexpected discovery of the fact that the production of bio-mass in the roots of the trees was twice as large as had been anticipated. The main steering effort in this phase then, consisted of a reorganisation into research-problem-oriented groups. A secondary effect of this was an emerging gap between empirical and theoretical work. These organisational changes also reflected integration over the borders of disciplines and disintegration of empirical and theoretical knowledge. In a few cases, however, a spontaneous integration of field work did occur. The experimental station at Jädraås offered a creative atmosphere for informal discussions and planning of further research projects.

Phase Four: Modelling (1976–1978)

In this phase, most of the empirical work went on as before but the modelling work was more successful than it had previously been and modellers began to demand empirical data. The first modelling attempts were aimed at making a global model of the pine-ecosystem. At the beginning of 1976 the idea of a global model was abandoned and a family of partial and more detailed models were produced. These latter models were closer to field work and the more traditional knowledge about growth in plants. These models were at first based on empirical data from literature because the data from the project did not have the same resolution in time measurement or did not fit in other ways, and also due to delays in delivery.

Views still vary on the relationship between partial and global models:

> In the beginning I thought it was easier and more attractive to have a model which could be stretched to the big perspectives and also could be used for the small perspectives . . . . I have realised that this is very difficult for practical reasons and, not least for reasons of data technology, that this global-modelling work is very demanding and laborious.
>
> Today there is a very negative attitude towards working with complex models because it is very difficult to follow their biological relevance. (3)

New kinds of models were built to answer specific questions and some applications were investigated, for instance the effects of acid-rain from burning oil on a forest ecosystem. From this we might hypothesize that in some cases successful solutions to practical problems could be found by using problem-oriented models without a purely scientific background. The term 'applications' would thus be misleading. As we have mentioned, there were a number of efforts to make a global model of the ecosystem. At the end of the project the researchers approached the model building in a new way: they began to use models as 'instruments' and not as 'maps' of reality. This led to an acceptance of radical simplifications of the models, for instance a reduction of the numbers of variables. As a consequence, a plant stand model for the growth of biomass in a 150 years perspective could function as the desired global model.

On the organisational side a special group and subprogramme were established for modelling and data-systems work. Thus, the previous gap between empirical and theoretical work now had an organisational manifestation. This group was later replaced when the project board was reorganised into a smaller group, from twelve researchers representing the problem groups to five who worked closer together.

Different modelling approaches, from a global model to submodels, from partial models to a global model and finally, expansion of one submodel to a global model, but not encompassing all the partial models, are in one sense an iterative kind of integration (6). In retrospect, different modelling approaches are not mutually exclusive and it seems that the first modelling approach was a necessary prerequisite to the second, etc. The transition of different modelling approaches can therefore be seen as quite rational. But the researchers themselves felt that it was due to failures in the earlier modelling approaches.

The integrational factors in this phase were thus: partially overlapping models, iteration in the sequence of different modelling approaches and, on the organisational side, a special working group for the development of knowledge. Among the disintegrational factors was the increasing complexity when different partial models are connected together. The conflict between theoretical and empirical work also grew deeper.

Some Consequences of the Division of Work

As the scientists were not interested in adopting the systems model, a division of work between modellers and empirical scientists took place. For some scientists this implied that their research roles were diminished, from their point of view, to mere data-delivering for the project. Furthermore, the data were used in such a manner that their producers could not recognise them when they appeared in the wider context of the model. This led to problems of a social nature. At the end of the project this matter was considered and the researchers responsible for data-collection were also allowed to take part in later work.

The process of delivering data was looked upon in different ways by modellers and empirical scientists. The modellers needed data quickly for their work. The empirical scientists, on the other hand, were eager to get longer series of measurements in order to get data which could fulfil their criteria of knowledge. The criteria of the empirical scientists were not taken from the modeller's paradigms but came from their own disciplines with their data-criteria.

You have to have large series of data, which take time to handle statistically in order to confirm reliability. This side has been underrated. (3)

For some scientists, the task of being a data deliverer was thus problematical. Time and again they had to choose between the demands for fast results for the project, and their own demands, which were to continue a productive line of research by continuing to do experiments and take measurements.

We didn't like the methods that were supposed to be used for the first data needed in the project. We wanted to work out our own methods and had to fight very hard to do so because it would then take time to get any data at all. (3)

For other scientists, the research role as data deliverer was not a negative experience. These scientists, on the other hand, had very vague ideas of the general goals of the project. There was a lot of scepticism and uncertainty between the two main group of scientists, the modellers and the empirical scientists.

Many of the empirical scientists look upon us as some kind of flagship of figurehead sitting at the prow, making the wave glimmer in the sunshine, but think there is no concrete need for us in the practical scientific work. This view is rather common among many of the empirical scientists.

There has been some difficulty for the project leaders to understand why certain empirical data are needed and to get enough money for this. It is not a matter of only saying that we want to have the data by a certain day and we except you to deliver it. Methods of obtaining the data must also be found. For some of the physiologists it took perhaps one year to get the equipment in order.

It often happened that we in the process of making models had a shortage of certain information .... The measuring programmes were perhaps often too heavy. We have several examples that show that they actually should have been cut off. It is a question of encroachment on the individual scientist .... This must be said to be a weakness of the project, there should have been better interaction between model and reality.

I have made up my mind at this time that I will only work with things I find meaningful.
And if that does not suit the project?
That is of secondly importance to me. (3)

These kinds of problems exemplify a more general problem in directing a large scale project. It is necessary to direct and coordinate the different investigations and at the same time, the more the project leaders direct, the smaller the opportunity for innovation by the participants.

Phase Five: Synthesis, Generalisations and Applications (1979–1980)

In the last phase, knowledge was integrated on the intermediate level in producing synthesising reports. We think that there are certain dangers in synthesising models afterwards, but then of course, this part of the project is not finished yet. In putting together pieces of knowledge there is a risk of missing system effects, for example new qualities could appear in the whole system which could not be found in the parts.

In the modelling work, the partial models could be connected. Output from one model could be used as input in the next one. But some factors are treated as constant and the latter models are not connected to the former. The modellers admit that some system effects could appear but they think that if these were anticipated the models would be too complex. One important system effect has appeared and caused a lot of trouble and discussion. It was found that knowledge about the 'mechanism of photosynthesis' was not necessary on the system level of the growth of a whole tree. Of course, it is necessary that photosynthesis occur but the trees are 'good at photosynthesising' so this is not a limiting factor for the growth, on the contrary, external factors are dominant. Research on photosynthesis is overly ambitious in terms of the goals of this project.

The Role and Competence of the Generalist

In interdisciplinary groups problems arise in understanding one another's language and preconceptions about science. In order to master problems such as these and to bridge the gap between specialists, a new kind of researcher-role is required, the role of generalist. He is to function as a link between the scientists in the group, leading discussions about ideas, coordinating scientists and investigations, editing reports and sometimes functioning as a research therapist solving psychological problems in interpersonal relations within the group. In other words, the generalist must have an understanding of the cognitive as well as the social problems of research. In many projects, organisation and administration are so closely bound to methodological problems that the generalist must also be skilled in administration. The necessity for generalists, however, may simply be the effect of lack of integration in the research team. We have found in our interdisciplinary projects that different kinds of generalists can develop in a given group.

First we can speak of generalists in a restricted sense. About half of the scientists in the ecological project never worked in interdisciplinary groups but they had the opportunity to meet scientists from other fields of research and to learn new methods and perspectives. Here, the generalist is familiar with different approaches although he does not usually overstep his own. In one sense, the generalist is and must be a specialist. He must be well acquainted with some field of science. One way to create generalists of this kind is to let scientists oscillate between research within their own discipline and participation in interdisciplinary groups. The generalist must develop competence within his own discipline in order to have a solid basis to stand on.

Second, competence as a generalist may involve the ability to create models of data from different areas of research, for example mathematical models. In this case, the role of generalist is based mostly on an integrating methodology. This created some problems when the biologists felt that their field was being intruded upon:

> The data people have done a lot of work. They have done a lot of biology without discussing it with the biologists. They have studied biology literature, etc. It is one thing to study the literature in order to discuss a subject with us biologists, but quite another to work within biology without asking the biologists, and that atmosphere can become tense.
>
> It is one thing to learn the fundamental concepts, and another to try to evaluate the physiological work. I don't think that the data people have the competence to do this in the same way a biologist would. They don't have the feel for it. (3)

Third, in some projects, as in the forest project, there is no strong integrative process, but there are many coordinated subprojects. Most of the integration of knowledge must be done at the end of the project. The generalist's function is to provide a synthesis of information from different subareas. To be able to do this, the generalist must know the relationship between the different problem areas and have system theoretical competence.

> We speak of two kinds of generalists. First we have the people who only have the mathematical skills. But in order to handle ecosystem problems it is necessary that you have the interest in and the knowledge of biological problems at the right system level. (3)

The person who said this meant that many of the specialists could not understand that simplifications are inevitable when changing perspective from parts of the tree and partial processes to, for example the level of growth of the whole tree.

Fourth, the role of generalist could involve coordinating projects. Since the organisation of a research project must be adjusted to the development of knowledge, for instance work groups according to problem areas in the project, the project leader must have scientific competence in the field. But the generalist, as a research coordinator, must also stimulate the researchers to cooperate and take responsibility for the integration process. In this project many of the researchers felt no responsibility for the general goals of the project, so it was very important that the project management kept the project together cognitively and administratively. Being a generalist can be the same as having the competence to work as a consultant in different kinds of interdisciplinary groups and handle general methodological, psychological and sociological problems. Metascientific competence is important in order to introduce discussions about paradigmatic questions such as world pictures, assumptions about causal factors, ideals of science, the role of researcher, etc. This has been a part of our own tasks when studying the projects.

Conclusions

(1) Our objective here is to draw attention to the development process within a research project. In order to get a realistic concept of knowledge, to find out what the 'real problems' are and to find the right scientists for the project, a preparatory phase is essential. In the forest project the lack of theoretical preparation led to a lack of coordination between theoretical and empirical investigations. As the problems and knowledge change, the organisational forms have to be changed as well. Much of this dynamic planning can only take place through close co-operation on the research field level. More formal

meetings on an intermediate or project level were experienced as boring, and as disturbing to the researchers in their work.

(2) Knowledge cannot be integrated once and for all at the end of a project simply by compilation of reports from subprojects. We have found that integration of knowledge is dependent on integration at different stages in the research process. Integration of knowledge in an interdisciplinary research group also implies that social integration must occur.

(3) Many research administrators and scientists tend to have a simplified and overly rationalistic picture of the research process. In the forest project, the research board had underestimated the social and psychological factors such as interests, cognitive styles and academic career. This led to few cases of co-operation during the project. So the main part of the integration of knowledge had to take place at the end of the project. The consequences were social problems among the scientists and a low degree of integration.

(4) There are differences between different researcher roles and the competence of theoreticians, empirical scientists, specialists and generalists which are as important as the differences between scientists from different disciplines. These groups of scientists have different interests and cognitive styles and this must be taken into consideration in order to attain co-operation. This has implications for measuring standards, relationships between models and reality, etc.

Notes and References

1. Different possibilities of integration of knowledge are discussed in Jan Bärmark and Göran Wallén, *Knowledge Production in Interdisciplinary Groups* (Report no. 37 in series 2, 1978, Department of Theory of Science, Univ. of Gothenburg) and Jan Bärmark and Göran Wallén, *The Interaction of Cognitive and Social Factors in Steering a Large Scale Interdisciplinary Project* (paper presented at the International Conference of Interdisciplinary Research Groups, Reisenburg, April 22nd–28th, 1979). Report no. 41 1979, Department of Theory of Science, Gothenburg.
2. The presentation is based on interviews and internal reports from the project; see Folke Andersson, *Development, Coordination and Administration of an Integrated Ecosystem Project* (paper prepared for the workshop 'Initiation of and training for integrated projects', Wenner-Green Center, Stockholm. 770509-10, organised by the Colloboration Committee of Northwest European Research Councils (NOS) (mimeo)) and Per Anders Bergman, *Development of a Large-scale Research Project*, FEK-report 5, Stockholm, 1975.

3. Quotations from our interviews.
4. It was difficult to formulate precise goals and they were also in conflict with each other in parts, see Bärmark and Wallén, *op. cit.*, 1979.
5. See Ian Mitroff, *The Subjective Side of Science*, Amsterdam, Elsevier, 1979, p. 168.
6. This manner of integration has been brought to our attention by Fred Rossini *et al.* in *Frameworks and Factors Affecting Integration Within Technology Assessment*, Georgia Institute of Technology, Atlanta, 1978.