FISHERIES CO-MANAGEMENT RESEARCH AND
THE CASE- STUDY METHOD

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ABSTRACT

Until now, fisheries co-management researchers have preferred the case-study method to other methods. In this paper I discuss the strength and weakness of this particular research method, what it is and what may be expected from it. I argue that case studies are well-suited for theory development, for the discovery of hypotheses, especially if case studies are part of a comparative research approach. I draw on my own experience as a sociologist who have been involved in both qualitative and quantitative social research on fisheries organizations, fisheries management and community development, mostly in Norway.

Introduction

In this paper I reflect on the usefulness of the case study as a method of research in fisheries co-management as I have experienced it in my own work. I will discuss what this particular method is, for what purpose it is used, what its strength and weaknesses are, and what is reasonable to expect from it. While case studies are common in social science, they are more so in certain disciplines than in others. They are for instance more used in social anthropology than in sociology and economics. However, many of the classic studies in social science are in fact case studies. Until now, most of the research on fisheries co-management has used this method.

My work on fisheries co-management has for the most part also been case study-based. Indeed, I can say that the idea of co-management was first revealed to me through a case study. My first case study on fisheries co-management was on the Lofoten fishery in Norway. In the early 1980s I traveled with a group of students to the Lofoten area, where Norway’s most important cod-fishery takes place between January and April. My students did research on various aspects of this fishery for their term paper, and one group focused on the management system. Prior to the excursion I was only vaguely familiar with the Lofoten management system, and it was intriguing to find that fishermen in this area had successfully managed important aspects of their fishery for almost 100 years. The Lofoten fishermen seemed able to cooperate in sorting out their different interests contrary to the assumptions of Garrett Hardin in his famous paper on “The tragedy of the commons” and the Prisoners’ Dilemma in game theory (Hardin 1968). Theirs was, I realized, a model contrary to the general fisheries management system in Norway. According to my knowledge at that time, it also seemed quite unique compared to management systems in other countries. Before I visited the area, fisheries management per se had not been a focus of mine, but I was already tuned into the theoretical issue of coordination of interdependent fisheries activities by means of organized cooperation, which was the theme of my doctoral thesis. The trip to Lofoten then made me interested in yet another aspect of this general subject. A couple of years later I went back to the area and did a more thorough case study, which resulted in a published article in Jentoft and Kristoffersen (1989). The general issue of co-management and this particular case study has been with me ever since, and I still make reference to it in papers that I write.

The Logic of Discovery

I will return to this particular experience because it seems to me to be a good illustration of what case studies may bring us. But let me stress here at the outset that there is nothing unusual about my experience. Many social researchers can tell a similar story. We stumble into something, often by pure accident, which triggers our
curiosity because it is different from what we had expected. And for some preconceived theoretical position the discovery seems to make a difference, it changes our way of thinking, and sometimes even our careers. Such incidents are, of course, not unique to social scientists. Many of the great breakthroughs in natural science have come about in similar ways. Remember Newton’s discovery under the apple tree or the revelation of Archimedes in the bathtub. Sometimes the researcher sets out to find out or to prove a hypothesis, but in the process he or she discovers something else, just like Columbus did. In some cases, no experiment was going on. Something struck the researcher – like a lightening – in a moment of clairvoyance, for instance while busily doing something else. I once got the title for a book when I was preparing dinner. I still have a vivid memory of that moment when it happened. Psychologists now say that walking is good for the creative mind, provided that you leave the walk-man at home. It is equally stimulating to travel. When visiting another country, we always learn something new, not only about the place we arrive at but also the place we left. You start asking yourself: “Why is it not like this at home?” Consequently, we have a research question or a hypothesis in our suitcase when we get home.

Twenty years ago I went to Canada on a sabbatical leave from my university. On the first day I saw fishermen on the picket line. Not only did I ask what their complaints were, but I also asked myself: “Why do we never see fishermen on the picket line in Norway?” My answer later appeared in a published article. My professor from college Otter Brox once told the story of how he discovered the Raw Fish Act the first time he came to Newfoundland. Notably, Newfoundland has no Raw Fish Act that regulates the exchange between fishermen and fish producers, but Norway has. This, I believe, is part of the answer why Norwegian fishermen never go on strike. The problem with institutions is that once you have gotten used to them, they become invisible. You take them for granted and stop noticing them. And just as the only way to observe that the shape of the earth is round is to go into space, you need to go away to get a different perspective on fisheries institutions. They are best viewed from a distance. Visiting Canada gave Brox the distance he needed to see the Norwegian institution from a new angle.

Observations like these are similar to those that led the sociologist Robert Nisbet to argue that “the logic of demonstration” and “the logic of discovery” follow totally different paths. The former is described in detail in methods textbooks: it has strict rules and procedures. The researcher has only to follow a straight, well-marked road. The logic of discovery is more impressionistic, creative, and visionary. The road is bumpy, filled with potholes, far from straight, and the destination is less certain. Nisbet argues that it is a great mistake to assume that one can obtain the latter (discovery) by following the rules of the former (the logic of demonstration) (Nisbet 1976). Again, if we think of it, most of the great classic studies in social science did not result from large-scale surveys and rigorous testing of hypotheses. They are more often case studies, which provide a new and challenging perspective on society. But I should add that although Nisbet’s point is well put, I am not sure he is entirely correct. I have had revealing experiences of discovery in front of the computer screen based on analysis of quantitative data as I have had in the field doing qualitative case studies. It should also be stressed that case studies do not have to be qualitative.

Many fisheries’ case studies combine qualitative and quantitative methods. They mix participant observation, semi-structured interviews, archival search, and survey techniques. A good example is Raymond Firth’s seminal study of Malay fishermen (Firth 1966). For qualitative case-study researchers, discovery and testing go hand-in-hand. Neither do I share Nisbet’s belief that it necessarily takes a particularly visionary or artistic mind to make discoveries even though it is clear that the people he is referring to, Marx, Weber, Durkheim, Simmels, Tocqueville, were especially gifted people in this respect. However, I do share Nisbet’s argument that before we can test a hypothesis, we must discover it, and that the way of discovery is not as straightforward as the path of testing and verification. In addition, I do believe that case studies are particularly suited for the purpose of discovery, but that there are ways of making the process of discovery by means of case-study method less coincidental. One way is to systematically expose oneself to new empirical situations. My experience is that hypotheses personally generated through empirical research in the form of a case study, stirs more enthusiasm and excitement in me than those that I receive from reading theory. The Lofoten study really got me going on the issue of fisheries co-management. Surveys can be fun, but I find case studies much more stimulating. This is, of course, a matter of personal taste. The good thing with case studies is that they bring you out of the office and into field where you meet your respondents face-to-face and can get a feeling for the particular situation they are in and how they see it. There can be no doubt that the enthusiasm one gets from doing case studies is an important component of the logic of discovery. Distance from real-life situations is occasionally necessary as part of the research process, but we cannot be distant all the time. Clearly, “armchair” sociology has its limitations.
Before I proceed I must say something about the nature of the case-study method, what case studies are and what they are not. There is widespread skepticism about the case-study method. Researchers of the positivist inclination tend to regard them as unscientific, and are questioning their generalizability. I will in what remains of this paper talk about not only the limitations but also about the potentials of case studies in co-management research. Finally I will discuss what in my view characterizes a good case study.

The Case-study Method

A case study is, according to Yin (1989), an empirical inquiry that a) investigates a contemporary phenomenon within its real-life context; when b) the boundaries between the phenomenon and context are not clearly evident, and c) in which multiple sources of evidence are used (Yin 1989). Let’s say co-management, or a certain context is not the “real-life context.” The context is not the focus but the locus of the study (Arensberg 1961). The country, community or fishery is locus where we situate ourselves. The contemporary phenomenon – co-management and legitimacy – is our focus of research. We are allowed to make use of any method that is available, useful and ethical for our focus in that particular locus – be it participant observation, semi-structured interviews, questionnaires, archival studies, action research and real-life experimentation.

Case studies should be explanatory: they should answer both why and how questions. They should also be explorative: They should attempt at generating new research questions and hypotheses. For this, the researcher must be openminded and keep an eye for the unexpected and the obscure. Furthermore, case studies should be descriptive: They should tell a story, be a good read, and present the actors’ point of view. Case studies should preferably focus on the social interaction that takes place in vivo. Typically, co-management case studies focus on communication, cooperation, conflict resolution, and learning among parties involved with respect to regulatory decision making. They should also depict institutional mechanisms that guide and shape the involved actors’ behavior and worldviews. Case studies should emphasize the structures that co-management systems are embedded in. This is Elinor Ostrom’s idea of “nested institutions” (Ostrom 1990). They should look for the subtle cultural conditions underpinning those institutions and the mechanisms that the involved parties tend to take for granted because they are part of moral fabric of the whole society and only implicitly part of the co-management design as such.

Case studies occur at various levels, from micro to macro contexts. They can be investigations of one organization within one community, or several organizations within one or several communities or a particular fishery. Raymond Firth’s case study was of one region, Kelantan, which is the northeastern-most province of the Malay Peninsula. Case studies can also embrace a whole industry or several industries. For example, in 1998 I published a comparative co-management case study of fisheries and reindeer pastoralism in my country (Jenotf 1998). Case studies may also involve a country or several countries. Some years ago I was involved in a case study that compared the Norwegian and the Canadian fisheries management system (Apostle et al. 1998). Recently, I became part of a co-management study that will compare countries within the European Union.

Case studies do not have to involve a great number of units to be of scientific value. The best book on unemployment I ever read was a study of one single individual in a Newfoundland “outport” (Wadel 1972). Again, here the theoretical focus was general even though the data was drawn from one small fishing community and one individual’s experience of being unemployed. It is precisely this combination of a general focus and a particular locus that makes case studies so valuable. We don’t have to be inhabitants of Lofoten, Lake Kariba, Nampula Province, or Western Cape to find that these case studies of fisheries co-management speak to us. We learn about these sites, and that is, of course, important in itself. But we also learn because they address a general issue of mutual concern, which is co-management.

This is a crucial point that I must dwell on, because this is where we meet prejudices against the case-study method. How often have we not heard that case studies have low scientific merit because they lack the rigor necessary to be called a scientific method, and because their findings cannot be generalized? It is argued that case studies, because of their predominant qualitative approach, are not more than “advanced journalism.” First of all, there is nothing inherently sloppy in the case-study method, even though it is true that in many instances case-study research may deserve such a label. There exist handbooks that are of great help to the case-study researcher. Also qualitative research has defined procedures. Besides, an experiment or a survey could also be carried out in a sloppy fashion. As to the second criticism regarding generalization, it is true that a particular case is not representative in a statistical sense, but the unit of analysis of case studies may well be typical for a larger population. And it is often precisely because a case is unique that it is interesting from a research point of
view. Furthermore, as Yin (1989:21) points out, case studies are "generalizable to theoretical positions." They can be used to develop theories, for instance pertaining to co-management.

Case studies can be repeated, just as experiments can be repeated many times. Research designs can also contain a number of case studies. In fact, multiple case studies designed to be comparative, is as close to the laboratory experiment as one can get in social science. Society cannot easily be turned into a laboratory, but, in principle, by careful stratified sampling one can at least do the same thing as in a laboratory: By selecting cases for comparison, one can keep some variables constant while studying the effect of varying others. For instance, one can select fisheries that are similar with respect to resources and technology, and then compare the effects of management institutions that are different. Admittedly, this can be a difficult research design to employ, particularly on a large scale, but it is possible.

Case studies are more useful for theory development than for theory verification. For that, even a single case study can be important. Discoveries that one does in one case study can be pursued qualitatively or quantitatively, in a new case study or in a survey. My Lofoten case study stirred my interest in similar studies, which brought me to other countries and led me to become part of research networks. Case-study findings may thus be added, compared and synthesized into a fairly comprehensive general theory of fisheries co-management. For this, however, the case studies would have to be read with some analytical categories and theoretical perspectives in mind. For instance, we would look for what the case studies have to say about key issues such as legitimacy, trust, compliance, interactive learning, conflict resolution, power-sharing and community. Isolated, a case study may have limited interest, but over time, as more and more case studies are published, we get a better and better grip on these issues. And if we cannot be totally confident in what we say is true, we should always seek comfort in the saying: “It is better to be approximately right than precisely wrong.”

A good case study speaks to one or several general issues such as those mentioned above. A case study is not pure description or story telling without focus and message. It is an obligation of the researcher to be both empirically thorough and theoretically relevant. The researcher should attempt to make a point, a general argument, and to draw a lesson from his findings relative to what others have done. Only when the study, deliberately or not, addresses a general analytic theme can it be interesting from a comparative perspective. The ideal is that case studies should be theoretically informed and theoretically informative.

The reason why many of us find co-management theoretically interesting is because it touches issues with a deep history within our disciplines, such as democracy and legitimacy of power. Personally, I have found inspiration in political theory and bringing it into the co-management debate. My most recent work is on the issue of representation in fisheries co-management and what roles that may be assigned to involved users who represent, speak for, a larger constituency. Representation and democracy have been an interest of social theorists for a long time. Rousseau, Hobbes, Burke, Madison, and Schumpeter all had relevant things to say on this subject. They were concerned with the question of what constitutes a representative government: When can we say that a government is truly representative of its people? For our purpose, we can easily replace government with co-management and ask: What does it take to make co-management systems genuinely representative of affected stakeholders and user groups? The question is no doubt an important one. I believe that it is useful to know how theorists have struggled with the answer. I also think that we, inspired by these theorists, can address the issue empirically by means of case studies of actual co-management systems.

I have concluded that even though my own country Norway has long traditions of co-management, it can hardly be characterized as truly representative of user-groups. I got this insight before I read Edmund Burke, James Madison, and other theorists on representation. My colleague (Knut H. Mikalsen) and I simply attended management council meetings and listened to the debate. We concluded that because constituencies have complex interests, it matters not just who the representatives are but also who they represent and in what capacity - or role - they meet. In the Norwegian co-management system fishermen are represented only as members of certain gear groups and as union members, not as members of communities and districts (Jentoft and Mikalsen, 1994). This impacts on how they argue and how they vote in the decision-making process. For stubborn skeptics of the case-study method and this finding in particular, the best advice I can give is to check it out themselves by means of the same method but on other cases as well. It is far from unlikely that they will discover that our conclusion does not hold true in their case. There may well be things we did not see, or that our claim does not hold true in their case of scrutiny. Then, they would have brought the theory of co-management a step further, and for that they should be congratulated.
**Grounded Theory**

I have claimed that case studies are useful for generating hypotheses, which can be tested on a larger sample through other, more rigorous methods. This is also Barney Glaser and Anselm Strauss’ argument in the book titled "The Discovery of Grounded Theory," which I use in my methods class (Glaser and Strauss 1967). They hold that generating theory should involve a strategic, comparative process of research. It is the emerging gaps in the theory that should decide the next case study. "The emerging theory points to the next step." They call this "theoretical sampling," in contrast to statistical sampling. The cases are not randomly chosen, but out of theoretical relevance. They are selected for the deliberate purpose of developing categories and depicting their properties and relationships. The position of Glaser and Strauss is that necessarily there is no clash between the purposes and capacities of qualitative and quantitative research, as we are often led to believe. The two approaches can fruitfully support each other, and they can both be used for verification and generation of theory. The authors argue that quantitative studies have a potential for theory generation that has not been fully exhausted. To give an example from my own research: Together with a Canadian colleague (Anthony Davis) I did a case study of one fishing cooperative in Nova Scotia. When analyzing the data by means of SPSS, our tabulations suggested that the more members were involved in the daily decisionmaking of the cooperative, the more willingly they accepted a price below the going market rate and the more they would volunteer in non-paid work for the cooperative. In other words, participation makes members more inclined to support their common cause. We concluded that involvement and participation generates what Cyert and March call “organizational slack,” which for the cooperative would be an asset in hard times (Cyert and March 1963). Our case study was thereby “generalizable to a theoretical position.” We also discovered a hypothesis that could be fruitfully employed in co-management research (Jentoft and Davis 1993).

Here, one may object that this finding is just based on a small case study, that the data analysis is not all that sophisticated, and that the conclusions drawn cannot be generalized for a larger population of fishing cooperatives. This is, of course, true. But we could, if we decided to do so, find out. We have at least been equipped with a very interesting research question, also with respect to co-management regimes. Even if it is preliminary and suggestive, the finding supports the general thesis that co-management promotes legitimacy and compliance, because compliance requires that fishermen sacrifice what may be in their short term, private interest, for instance over-fishing their quota. Similarly the Lofoten paper contains some reflections on the relative strength on co-management with respect to participation and compliance. My co-author (Trond I. Kristoffersen) and I argued that co-management is particularly important in committing those that lose the vote in the collective decision made. Again, this should be considered as an interesting hypothesis rather than a solid finding.

Glaser and Strauss distinguish between substantive and formal theory. The latter is at a higher, more abstract, level than the former. For example the legitimacy of power is theoretically at a higher, more formal, level than legitimacy of fisheries co-management. Theories of governance are at a higher analytic level than theories of fisheries management. Theories of knowledge and learning are also at a higher level than theories of fishermen’s local ecological knowledge. Substantive theory is developed for, and about a particular empirical area, such as fisheries co-management, while formal theory pertains to a more general, conceptual area, such as participatory democracy, powersharing, and institutions. Notably, while some cases are not theoretically comparable at one level, they can make useful comparisons at another level. Cooperatives, universities, business corporations, and co-management systems are different empirical entities. Nevertheless, they are comparable from the perspective of participation, legitimacy of decisionmaking, and implementation of rules and regulations. In all these organizational settings, members are involved in decisionmaking for basically the same reasons. In all instances, I have found the theory of organizations and theory of democracy to provide useful analytical tools.

Formal theory may fruitfully guide our research questions at the substantive level. But a good case study should also move from the substantive to the formal level. There are important lessons to be learned from the empirical studies of fisheries that are relevant for larger issues. We should have the ambition of contributing to the general debate on issues such as sustainable development, democracy, communities, organizations, power and equity, to name a few, even though our empirical work is on fisheries co-management. If not, fisheries social science will continue to be considered as an esoteric area of research within our disciplines, not particularly interesting for other than those who have a special interest in fisheries.

Comparative research is, according to Glaser and Strauss, particularly productive for developing grounded theory, which is theory drawn from empirical data. New hypotheses that arise in the process should be pursued in new case studies, but they also invite us to return to our previous case studies. This is why I still have not
finished my Lofoten research. Thus, the systematic approach prescribed by Glaser and Strauss should reduce our reliance on the specially gifted, visionary mind, as Nisbet referred to. Hard work and stubborn perseverance should help a long way.

References


