The Function and Handling of Research Problems in the Social Sciences

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Research problems are usually assumed to play an important role in the research process and different authors have given testimony to the importance of such problems. For example, Wertheimer (1945:123) stated "Often, in great discovery the most important thing is that a certain question is found." Another example is Kantorovich (1993: 29) who concurred: "A discovery of a new problem may lead to a significant progress of science, sometimes more so than a discovery of a solution of a known problem; such a new problem may open up a whole new field of investigation." Merton (1973: 453) reported from interviews with Nobel laureates (carried out by Harriet Zuckerman) that "They uniformly express the strong conviction that what matters most in their work is a developing sense of taste, of judgment, in seizing upon problems that are of fundamental importance."

These remarks, not all of them based on any reported systematic empirical observations, primarily concern great and maybe rare events in science. However, research problems are of interest also in connection with more mundane every-day science. In addition, Ziman (1995) noted that what is important is not just to formulate an original question, but to have enough faith in it *to take it on* in one's research. What role do research problems play in the production of scientific knowledge?

To what extent do researchers look for fundamental problems and to what extent are they driven by other goals in their search for research problems to focus their research on? Where do research problems come from and how are they handled in the research process? In this article the nature and function of research problems in a few disciplines within the social sciences are explored through interviews with experienced researchers. The disciplines in question are psychology, social anthropology and ethnology.

The concept of a research problem is often taken for granted, but it is appropriate to provide at least an initial definition. A minimal definition of a research problem is that the researcher has a goal for his or her understanding but does not know immediately how to reach this goal. A problem involves meaning and is represented in terms of a conceptual framework or frame of reference available to the researcher and accepted by him/her to be suitable for representing the problem. Most or all of the conceptual framework is usually taken from the discipline or specialism that the researcher is acquainted with. Even if the early formulations of the problem are done in every-day common-sense terminology, it will later be reformulated in order to fit the conceptual framework of the research community that the researcher aims to communicate with. The problem is expressed from a perspective, a "view point", which to some extent will determine which associations are easy for the researcher to make. Expectations for how the problem can be solved usually also come as part and parcel with the conceptual framework. The theoretical concepts in the conceptual framework used are usually defined partly through use of a certain methodology (Fleck, 1935/1997). It may be seen as a natural *default* expectation that solutions to a new problem expressed in an established conceptual framework will be solved by use of similar or compatible methods.

Research problems belong to those parts of the research process which are the least investigated. This is not simply because the initial stages of the research process are less investigated; research problems may be present at any stage of the research process. Another reason may be that research problems are an ephemeral kind of entity. Their identity is constantly negotiable as pointed out by Fuller (1993: 108) "Indeed, such basic identity questions as 'Which problem was solved' are settled only when the results are being written up for publication" Problems come in many forms, from vague and unarticulated to well specified and structured. Furthermore, they vary from very broad and general to very specific and local. Given that research problems have a genesis, and a developmental process, their presence may be particularly difficult to establish in their early stages.

During the hey-days of logical positivism research problems (hereafter simply "problems"), if treated at all, were often treated in a mixed descriptive-normative perspective. A common stance among writers in the logical empiricist tradition has been to reify problems and to claim that they are present from the beginning in the research process (Allwood & Bärmark, 1996a). Thus, for example, Bunge (1967: 165) stated: "It is not just that research begins with problems: research consists in dealing with problems all the way long" Likewise, Popper (e.g. 1972) in his famous model of the growth of knowledge (Problem -> Tentative theory -> Error elimination) gives the problem an initial position in the research process. Exactly how problems appear in this position is not treated, nor considered to be of importance by Popper, except that he notes that new problems occur as a result of the error elimination phase. However, as Popper clearly would have been aware, this is not the only way that problems can occur. The same standpoint, that problems occur first in the research process, was formulated by for example

Grinnell (1992) when describing research in biology.

A number of other writers have made statements about the origin, selection and handling of problems. In a classic study, the psychologist Roe (1953) through use of various psychological tests attempted to provide evidence for a linkage between early childhood events, personality structure and choice of research themes. More recently, Campbell (1994) provided speculations along the same lines. However, Roe only provided empirical evidence in the form of correlations and trends. Although these linkages may have some intuitive credibility on a general level, on the more detailed level they can only remain speculations. It is, however, of interest that her results showed a clear relation between eminence as a researcher and having personality properties such as curiosity, and a need for independence. Roe (1953: 49) also stated that "It is of crucial importance that these men set their own problems and investigate what interests them. No one tells them what to think about, or when, or how. Here they have almost perfect freedom." It seems that Roe here totally misses out on the social collective dimension of science.

Science is a social enterprise and the specific social context in which research takes place sets conditions for researcher's problem formation and problem handling. Mulkay (1972) is an example of a writer who, in contrast to Roe (1953), emphasizes the social dimension of science. He notes, following Hagstrom, that scientists strive for recognition by the scientific community and that they want other researchers to take an interest in their work (see also Campbell, 1974). For these reasons they need to consider which problems are legitimate within the research community addressed (Mulkay 1972: 15). Within the science community there also exists cognitive and technical norms for problem selection which researchers violate at the risk of being ignored by the other researchers. Thus, legitimacy is put forth as a norm for choice of a problem to work with. A similar view is expressed by Fuller (1992: 450), who characterizes the "constructivist" point of view in the following way: "... most of the real cognitive work occurs at the planning stages of scientific research, during which the team director determines the best way to mobilize all of her or his resources to make the biggest impact on the scientific community." However, this remark just as those by Mulkay, above, appears to relate less to the formation of problems and more to the handling (including the selection) of problems once they have been identified.

The training of graduate students is seen by Mulkay, just as by Kuhn (1970) and Hill (1995), as providing them with an ability to discriminate which part of the conceptual framework of the specialism is seen by the community as insecure and may be problematized and which parts may not. Experienced researchers, in the role of supervisors, are assumed to provide the necessary guidance in this connection. Ziman (1995) pointed out that the natural sciences differ from many of the humanistic and social sciences in that Ph.D. students in the latter two types of sciences are often given more freedom to pick their own problems. In a never ending search for recognition researchers will opportunistically look for "topics available" in a specific research domain and move to a new domain as "the current paradigm is filled in" (Mulkay, 1972: 34; see also Kantorovich, 1993). Each territory has its "recognition-relevant" properties. On the one hand the territory must not be too crowded with researchers because this indicates hard competition. On the other hand it must not be too empty because then there is "the risk that fellow scientists would pay no attention." (Campbell, 1988: 521).

Due to the assumed cognitive rigidity, "strong mental sets", of experienced researchers and the career strivings of the middle status level researchers (assumed to be most conservative) Mulkay (1972: 50) speculated that young researchers are the ones most prone to take the risk to be very innovative. Although not covered by the empirical material presented in this article, for various reasons this does not seem to be fully credible, except maybe for the most brilliant researchers who can be expected to show their brilliance early in their research career. Most of the young researchers will more likely be busy establishing their reputation in a way that heeds the established norms in the community. In this picture, the important basis for the formation of problems is the conceptual structure of the research discipline. Implicit in what Mulkay writes is that it is the gaps and other possibilities of this framework which provide exploitation opportunities for the researcher. Features of the individual researcher do not contribute much.

Other writers on the theme of problems have also to a large extent focused on the conceptual structure of the discipline or the specialism as the important resource for problem formation (e.g. Nickles 1980a, b). Ziman (1995) wrote about (natural) scientists' rationale when choosing between different research plans, mostly paying attention to the cognitive, material (apparatus) and social aspects (the research community). Ziman (1987) presented results from his research on middle-career researchers in natural science and technology R&D institutions. The results related mainly to the researchers' experiences and thoughts about the intellectual costs and benefits of change in their careers and research areas.

In the present study experienced researchers from parts of psychology (mainly cognitive psychology) and from social anthropology/ethnology are interviewed about their problem formation and handling of problems. The disciplines of psychology and social anthropology differ with respect to content and are also, at least somewhat, different with respect to the researchers' attitude to method. In most of psychology (clearly in those areas the researchers interviewed in the present study came from), when making claims about effects and differences between groups, the demands are often stricter with respect to reliability, research design and use of statistical methods. In social anthropology and ethnology the methodological demands may be said to be stricter in other senses, for example, the researcher is expected to have had more in-depth contact over longer time periods with his/her research field before drawing conclusions. Given these differences between the two subjects, it is of interest to investigate whether the researchers from the two subjects will also show evidence of differences in their attitudes to and experiences of problems in their own research.

Method

Informants

Fifteen experienced researchers in psychology (most of them from cognitive psychology, but two of them came from developmental psychology and one from health psychology) and 15 experienced researchers in social anthropology and ethnology (8 from social anthropology and 7 from ethnology) were interviewed between December 1995 and February 1997. The informants came from all five of the "old" universities in Sweden (Uppsala, Lund, Stockholm, Göteborg and Umeå) and in addition from two other research institutes in the Stockholm area. All, except one researcher in psychology and three researchers in social anthropology/ethnology, were full professors. The remaining four researchers were also senior researchers, well above the docent level in their research careers. This means that nearly all full professors in Swedish psychology with a cognitive orientation and more or less all full professors in social anthropology and ethnology in Sweden were interviewed. None of the interviewed psychologists were females and five of the informants from social anthropology/ethnology were females.[1]

In order to simplify the text, social anthropologists and ethnologists will, below, be called anthropologists. The main difference between social anthropology and ethnology in Sweden is that researchers in ethnology mainly conduct their fieldwork in Sweden and in social anthropology mainly outside of Sweden. The theoretical literature overlaps to a large extent between the two disciplines.

Interview questions

The interview questions were constructed with the purpose of covering three areas in connection with problems. First, questions were asked about the development, over the researcher's career, of his/her problems. The second area concerned the researcher's stance with respect to various aspects of problems in his/her current research activities. Finally, some more general aspects of problems were covered. Only the answers in connection with the second area and one question in the third area will be reported here due to space limitations. The questions are listed in Table 1. Earlier versions of the interview questions were tried out in pilot interviews with ten researchers (from psychology, social anthropology, theory of science, history of ideas and one informant from a research-funding agency). Allwood and Bärmark (1996b) have published edited versions of some of these interviews.

Results

The results from psychologists and anthropologists will only be presented separately when they differ substantially, or in the case of results from individual researchers.

Research without Problems?

The researchers' answers to the question if research can be conducted without problems varied. Two researchers pointed out that the answer to the question depends on the definition of problems. Ten other researchers noted that problems might be more or less articu-

Table 1. Reported interview questions, listed in the order they were asked.

1. Can you imagine that research can be conducted without research problems? If so, when, under what circumstances?
2. When do you consider a research problem to be promising?
3. When do you consider a research problem to be researchable?
4. Can you give an example of a research problem which you have viewed as exciting but have put aside because you have found it to be unrealistic? In which aspects was it unrealistic?
5. Do you use any special strategies in order to generate and formulate research problems? If so, describe these strategies. When are the strategies used?
6. Do you at the present time have a research problem that you could consider researching but concerning which you have not yet made up your mind? What are the arguments for? What are the arguments against?

(7. Which changes in the academic environment do you think would improve the effects of this environment in connection with the formation of research problems?)*

8. In your opinion which are the functions for research problems in the research process?

* The answers to this question are not reported due to space limitations.

lated (also including answers to question 8, in Table 1). Sometimes problems are nothing but a "theme", or "topic", sometimes more implicitly and sometimes more explicitly defined. One psychologist rejected that getting to know a phenomenon could be seen as a problem. On a more general level, another psychologist noted that it is presumably possible to formulate a problem in relation to anything.

Research without problems is impossible. Combined with the researchers' answers to the later question about the functions of problems (question 8, in Table 1), 25 researchers thought that problems are necessary in the research process. However, as will be described below, three psychologists in this group recognized that explorative research could sometimes be worthwhile at least in early stages of research.

Most of these researchers were very emphatic in their "no" to this question. One of the psychologists said "you always have a research problem even if you have not clarified it to yourself" and an anthropologist, argued that "you are not just openly searching for material, you never observe in a completely unstructured way". One further psychologist stated that research without problems is difficult "even if the problem is very unarticulated and vague, for example that I would like to have better insight into this phenomenon."

Some of these informants gave arguments for their opinion by referring to a *definition of science.* Among the psychologists, one informant said that a general search, "just feeling around", can not, nor should, be considered research. Another stated that empirical science is characterized by the fact that the researcher tries to control and knowing what to control demands a problem. Yet another informant said: "No. It is always the problem the research concerns." One of the anthropologists who thought problems were necessary noted, however, that the problems do not necessarily need to get answered, the important thing is that an interesting line of reasoning exists.

Among the anthropologists, one informant stated that the task of research is to "problematize". "The problem is the first and the last thing an ethnologist works with ... you have to 'polish' the problem up to the very end." Two further voices said: "Research without research problems is not research" and "That would be documentation activity and I don't see that as research." Two other informants concurred, both by contrasting research and shallow registration, one of them also depreciating "only telling stories from where you have traveled."

Some further arguments given for the necessity of problems were the following: Three psychologists argued that research has to be problem-driven in order to promote research which is "qualified", "of lasting value", and "successful", respectively. An anthropologist said "There exists a wonder which directs you towards the whole thing". The same person also said "Problems and hypotheses are there, otherwise I would not have started the research." Another anthropologist meant that problems are just as necessary for research as the empirical part is. Two anthropologists argued that in a minimal case, problems picked up from the literature or from previous research will direct the researcher's attention.

Research without problems is possible. Five researchers said that research without problems is possible, one way or another. A psychologist said that he preferred to work with a very broad explorative attitude. The two reasons he mentioned for this was that the important factors often are not those that one had expected to be important and that it is best to learn to know the phenomenon first and then to ask questions. Another psychologist simply stated another criterion which research activity should satisfy; it should have "a direction". An anthropologist said that research without problems was only possible during fieldwork when "most" anthropologists work in a very open way. According to another anthropologist, simply wanting to find out about something is sufficient and a third anthropologist in response to the question noted "No. Is that too short?, But it happens anyway. ... but competent research can be carried out without a clear problem, within a paradigm." In addition, one psychologist stated a seemingly contradictory view. He, on the one hand, suggested that research has to fulfill a minimum level with respect to problems in that it has to attempt to discover patterns in the data. On the other hand, he stated that he often would "go out" and attempt to make an inventory in an assumption-free way, "it is very much a discovery oriented research we are conducting."

Furthermore, four researchers who had previously stated that research without problems was impossible, agreed, after some interaction from the interviewer, that such research is possible and can be of value. Three of these researchers gave examples from their own research. Of these, an anthropologist stated "but then I had to work towards a problematization, the problems had to be created from the material." Another anthropologist stated that attempts at emic descriptive research could be used as contributions to a meta-theoretical debate.

Research without problems is possible as an indication of an early developmental stage. Three further informants in psychology, along the same line, after interaction from the interviewer later in the interview, noted that research without problems can be possible in early stages of research or in a research area, in order to get to know the phenomena. One of these also mentioned that it could be possible in another discipline, such as anthropology. Three anthropologists saw research without problems as indicative of an earlier developmental stage in anthropology. A further anthropologist noted that research without problems is not possible in anthropology but could be possible in history.

Other remarks. Three anthropologists noted that initial problems will change in the research process and that this is how it ought to be. Two researchers noted that although problems do not always drive the research process, they themselves teach their students that problems should have this function. Finally, two anthropologists recommended openness with respect to what is given by the empirical world and warned against being too attached to one's problems. However, one of them also noted that "research problems can help you."

Functions of Problems

The researchers were also asked to state what they considered to be the functions of problems in the research process. If the interviewees reiterated what they had already said in response to the question about the necessity of problems, their answers are not repeated here. The functions allotted to problems were many and varied.

Emotional or motivational function. A few researchers stressed the emotional or motivational function of problems. Two researchers said that the problem provides motivation, or "energy". An anthropologist said that they provide excitement and interest, and another characterized problems as a form of security.

Problems structure, organize, or drive the research process. Seventeen researchers said that problems structure, organize, drive (or some similar expression) the research process. This happens, for example, because problems provide a conceptual frame for the research or a focus for activities. An anthropologist argued that problems are what "make it a process." Two informants used more creative metaphors: "problems are the spine of the research process", and "... are the motor" in the research process." Four informants used the expression "orient" or described the same concept for example by using the metaphor "... are the rudder." Other words and expressions used were, for example, "steer", and "define" the research process.

In this context, two researchers saw a danger in "too well specified problems" because "you risk not being open to the unexpected." One anthropologist explicitly *denied* that problems steer the research process, "at least not in my case."

This informant also argued that theories provide understanding of something you find interesting in a field, no area is interesting only because it gives better understanding of some theory.

Other functions. Other descriptions of the function of problems were more specific. Two researchers stated that the function of problems was to *start* research, to get the research process going. A psychologist noted that problems create an opportunity for the researcher to *articulate his or her own values* with respect to what is worthwhile to research. Two anthropologists suggested that problems decide the *practical measures* taken, such as which social contacts you make and where you apply for research money. One of these informants suggested that this was the only function of problems. Two anthropologists said that one function is to make possible the *generation of sub-problems or sub-goals.*

A psychologist suggested that problems have an important function when it comes to *formulating ideas and hypotheses* that later can be tried empirically. Likewise, two researchers described the function of problems as making it possible to *relate concepts and theories to the empirical level.* Similarly, for one anthropologist the function was to *provide understanding of empirical phenomena.* Another three anthropologists suggested that the function of problems is to *support conceptual and theoretical development,* to make the research something more than just "documentation". Finally, two anthropologists stated the function of problems in connection with writing research texts, "as *themes for writing*" and "texts have to have some organization", as they expressed it.

Emphasis on variation. Other answers emphasized some aspects of the fact that the functions of problems may vary. Two psychologists noted that the importance of problems can vary; sometimes problems are very important and at other times they are more of an ad-hoc nature. As one of them said: "one sort of throws in the jest after the dough and makes a virtue out of a situation you have got your self into". Three psychologists and seven anthropologists also noted that problems can change character as the research cycle proceeds. "You start with some problems, but it is not these problems which you answer" and "You make initial problem formulations which you then continuously alter." One psychologist noted that researchers differ with respect to the function which problems play in their research.

Other remarks. Five researchers noted that their Ph.D. students are tought that problems are very important in the research process, but some of them also noted that this was not always so in their own case. Three anthropologists saw it as very important that problems are formulated in such a way that they fit the area investigated, that they are exciting to work with, that they should work as an "anchor" in the research process, and that they should be thoroughly fixated when the research process is completed (at this point they should be "carved out of stone"). One psychologist found that problems sometimes were allowed to play too small a role in the research process.

Use of Strategies to Generate Problems

The researchers were also asked if they

use any special strategies in order to generate and formulate problems. If they did, they were asked to describe these strategies and when they were used. The researchers differed with respect to the extent to which they used strategies to generate problems and also with respect to the extent to which they appeared to pay attention to this aspect of the research process.

A large range of different contexts and strategies were reported. In practice it is presumably not possible to make a clear distinction between when problems are generated as a result of the researcher using a particular strategy and when they occur spontaneously. For example, a specific situation where problems may be identified, such as reading research reports, may for the same researcher sometimes be used intentionally as a means to generate problems and on other occasions be the context in which problems occur spontaneously. Furthermore, it is unclear whether specific attitudes or stances which might facilitate the identification of problems, such as being interested or open towards events in everyday life, should be seen as intentional or spontaneous. Clearly, they can on different occasions be both. Thus, in what follows no attempts will be made to distinguish between intentional strategies and spontaneous identifications. Furthermore, the researchers' statements varied in that they sometimes concerned early stages in the research process, the formation of research themes, and sometimes concerned somewhat later stages, the generation of new perspectives on an already localized theme or the development of a subproblem or maybe even a new analysis within an ongoing research work. Finally, many of the contexts or strategies described below can occur together, such as drawing on your own research when writing grant proposals.

Problems originating in one's own research. Five psychologists mentioned that problem formation often occurred in relation to research that they had already conducted. For example, earlier experiments gave rise to new questions. Differences between your own results and your hypotheses or other researchers' results were mentioned. To go back and think through your own research, for example by trying to see patterns in your results, was mentioned by these researchers as a more thought-through version of this type of strategy.

Problems originating in writing research applications. The writing of research applications was stated by seven psychologists and one anthropologist as a natural context for generating problems. Some of the informants expressed it like this: "I'm always alert, especially before grant proposals", "grant proposals affect problem generation since I am then forced to make my ideas more concrete" and "... collect what you have done every third year, at the prospect of grant proposals, to see an understandable pattern". One psychologist said "if it had not been for the time pressure writing grant proposals would actually be counted among the finest moments in the research process". It is likely that more of the researchers would have agreed if the interviewer had asked them explicitly about this particular context. It seems reasonable to be especially alert, prepared to pick up, or to identify problems when an important research-funding agency's deadline for proposals is approaching.

Problem formation relating to the disciplinary content. Eleven researchers talked about different ways of using the disciplinary content as means of generating or identifying problems. Examples of such approaches, mentioned by the psychologists, are to keep the original problem in mind, to find a theoretical basis for applied problems, to attempt to find factors which will explain a large part of the variation in a phenomenon, to search for gaps in the research that has been done, to apply a special perspective, to generate questions by applying a particular perspective on different groups, to take 'a bird's eye' view and try get a larger perspective on the problem, to try to find new methods in a field or to find a new approach, and to try to develop one's own theory by taking aspects of other researchers' theories into consideration. Somewhat similarly, the anthropologists mentioned: to apply a special anthropological-ethnological approach, to try to get at a phenomenon from the side - the 'kitchen door-way approach', to consider what is researchable and what is interesting, to see a large phenomenon through the perspective of small phenomena ("such as understanding the early bourgeoisie through its societies for animal protection"), to study early phases in the development of a phenomenon, to merge yourself in the material, and to work though "concrete data".

Reading the research literature. Nine researchers stated that they find problems when reading the research literature. Some researchers reported this to be a more planned, intentional approach to problem generation and other researchers reported reading the research literature as a common occasion for spontaneously identifying problems.

Communication with other research-

Table 2. Individually oriented steps and measures taken in order to generate problems.

- Be prepared to identify problems or research themes (5 informants)
- Attempt to think on one's own (3 informants)
- Start to write (2 informants)
- Be observant in everyday life (2 informants)
- Try to relax (2 informants)
- Place oneself in different environments (2 informants)
- Brainstorm on a blank paper
- Think of what it is one really wants to do
- Start from one's existential situation
- Choose areas that feel important
- Deadlines for delivery of written products
- Occupy oneself with new, challenging, and fun things
- Have as many things going on at the same time as possible

ers or interested persons. Six researchers mentioned talking to other colleagues as occasions for problem generation. A further anthropologist mentioned participation in seminars as a means of generating problems. Three researchers mentioned building on others' initiatives as a strategy. These initiatives could come from students seeking advice in connection with essay or thesis writing or from other researchers or practitioners seeking collaboration.

Individually oriented steps and measures. Eleven researchers reported different kinds of individually oriented steps and measures taken. These are listed in Table 2.

In addition to the strategies listed in Table 2, seven researchers mentioned *saving ideas in notebooks* or in other ways as a strategy. An anthropologist described an elaborate system with folders and computer files (hereafter: folders) where he had stored 20-30 ideas and suggestions for research. He continuously fed these folders with new material, such as texts or pictures, when he encountered it. He described the folders as "waiting for hatching". This occurred when he felt that "now I would like to take on this folder/topic."

Other strategies. Three researchers mentioned trying to find out what areas it would be *possible to get research money* for and then formulating problems in these areas as a strategy. One psychologist said that he got many problems from *practical problems.*

No special strategies. Fourteen researchers said they did not use any particular strategy to generate problems. Two informants volunteered that they had never experienced a lack of problems. In contrast, one other informant said that he had not, in his research career, experienced a richness of problems.

Promising Problems

The informants were asked to state when they see a research problem as *promising* and when they see it as *researchable.* First the answers with respect to when a problem is promising are described. In general, the answers were quite differentiated. On a general level, four researchers volunteered that promising problems are detected by some kind of intuition and an anthropologist noted that what makes a problem promising depends on what you expect from research. The answers to this question ranged from answers stressing theory to those emphasizing the connection with empirical data. Below, the categories will be presented in this order. Finally, answers stressing motivational features are presented.

Emphasis on theory. Nine researchers stressed the importance of the problem having theoretical connections and implications. For example, one psychologist stated the importance of the criterion of "what one would gain understanding of if the problem was answered, theoretical depth" and another that the problem "appears to be able to explain something in a good way" and an anthropologist concurred "addition beyond the empirical contribution."

Gives associations. Some further answers also focused on the conceptual side of research, but in a somewhat more unconstrained fashion. Eight informants stressed the potential of the problem to give associations. Examples of answers are: "loaded with possibilities", "rich, not a thin problem, rich problem formulation, many sub-problems" (psychologists) and "feeds me with many ideas", "when something starts to give many associations, feels intuitive, you get new metaphors, new connections", "when it wakes up an energy ... a problem which is loaded in various ways" and "a problem which gives occasion for scientific reflection" (anthropologists). One of the anthropologists stressed it as important seeing the problem as possible to develop.

Possible to relate the theoretical and the empirical level. Five psychologists and one anthropologist gave answers which emphasized the possibility of connecting the theoretical and the empirical level as a feature of promising problems. For example, "when you have brought together theory and method", "when you have an idea of how you can study it, otherwise it is not promising even if it is interesting", "it can support or confirm or could falsify", "I don't think there exists any problem which is disengaged from at least some idea of how it should be solved", "answers ... which will lead my thoughts further" (psychologists) and "... when I can get surprised" (anthropologist).

News-value. Seven researchers focused on the news value, on the innovative, when describing what they meant by a promising problem. The potential for giving *new answers* was most commonly mentioned but also giving *new means of accessing the phenomena.* One psychologist stated that repeating previous research in the sense of doing "variations on a theme" was *not* promising. Some further examples are "when the research can give new answers" (psychologist), "it is the innovative which is the indicator being promising", "it gives knowledge about something I didn't know about before" and "when one can get at a phenomenon from another angle, to find an alternative entry to it" (social anthropologists).

Phenomenological criteria. Another group of answers stressed the phenomenological aspects of promising problems, i.e. the type of sensations or feelings such problems create in the researcher. Two psychologists and seven anthropologists gave answers of this kind. Here expressions such as "interesting", "exciting", "creates curiosity" and "creates the right feeling" were used. For example, "interesting, will give something" (psychologist), "nearly as: if I find this fun then it is good", "the problem has almost a form of a paradox, so that one thinks: 'this must be solved'" and "a feeling of this being interesting." Only one of the nine researchers who stressed the importance of theory in connection with promising problems talked about sensory features of promising problems.

Promising implies researchable. Five psychologists and two anthropologists argued that a problem has to be researchable in order to be promising. One of these psychologists noted, however, that a problem can be researchable without being promising. In contrast to these researchers, two informants explicitly denied that promising problems have to be researchable.

Other answers. In addition, a number of more diverse answers were given. One psychologist noted that a promising problem gives rise to *hopes that it will have a great importance.* One anthropologist noted that "promising" meant that it was *possible to get research money* to study the problem but the feature of being promising had no relevance for this researcher, the important thing was whether the researcher found the problem interesting. Another anthropologist argued that "promising" should not only mean a demand for positive results, *negative results should also be acceptable*, and yet another anthropologist connected "promising" with a *particular problem content*, -problems dealing with processes of change in today's world.

Researchable Problems

In similarity to their answers to the question about promising problems, the informants' answers to when they consider a problem as researchable also varied with respect to whether conceptual (theoretical) aspects or empirical aspects were stressed.

Conceptual demands. One psychologist and four anthropologists mentioned theoretical or conceptual demands when characterizing a researchable problem. For example, one anthropologist demanded that the theorizing in the area should be sufficiently ripe and another that the formulation of the problem should be such that it could give new perspectives. A third anthropologist considered the research question to be part of the discovery; "a theme to unwind" and the fourth added that researchability was "a problematic category".

Relating the theoretical level to the empirical level. Other answers focused on the relation between the theoretical and empirical level. A psychologist mentioned that the presence of a reference from the problem to the empirical level was necessary for a problem to be researchable. Five researchers talked, on a general level, about a correspondence between the theoretical and the empirical level. One of them, in this context, talked about the necessity of being able to operationalize your concepts. An anthropologist more specifically demanded that the empirical units should be demarcable in emic terms, i.e., in social units "that people can recognize". One psychologist had difficulties expressing himself on this issue but seemed to associate researchable with being able to conduct 'peer-acceptable' empirical investigations.

Availability of methods. In the same vein five researchers requested that methods should be available which could be used to handle the problem. One of the psychologists regarded it to be absolutely necessary that some form of systematic data collection was possible to perform. Furthermore, one should be able to exert some kind of control over "the conditions". Another psychologist argued that the problem should be possible to handle within the limits of the knowledge and techniques one have available.

Availability of appropriate data. Six researchers stressed the necessity to *be able to collect appropriate data* to answer the problem. Four researchers stressed the importance of being able to *solve the practical problems in connection with getting access to data.* Two further anthropologists stressed the necessity of being able to carry out research on the problem on the practical level.

Other aspects. An anthropologist suggested that demands on researchability should *allow unsure results* and another argued that "researchable" meant that people should be able to *recognize the results of the research* from their own experience. Two further anthropologists talked about *ethical demands* and one of these also somewhat jokingly talked about the desire to survive when the results had been reported. One psychologist thought that most "things" are researchable.

When Exciting Problems Are Perceived as Unrealistic

The researchers were asked to give an example of a problem that they had regarded as exciting but which they had put to the side because they had seen it as unrealistic. They were also asked to state in which respects they had seen the problem as unrealistic. Six researchers could not give any examples. Of these, an anthropologist said that he didn't think there are any unrealistic problems. A further anthropologist said that there had been very few problems that he had put to the side as being unrealistic, instead his approach was to put them in a "waiting position." Many of the other informants gave more than one example of the kind of problem asked for. The various difficulties seen with the identified problem ranged from having to do with demands of a disciplinary kind to aspects having to do with practicalities such as the researcher not having sufficient time for the research.

Motivational deficits. Two anthropologists brought up motivational problems, one of them could not get the right "feel" for a project which might have been financed and the other thought that the phenomenon that was to be the object of the study was too dreary to work with for four years (street alcoholics).

Ethical problems. Five researchers mentioned ethical problems as a hindrance. However, two of the psychologists in this group only mentioned this possibility on a general level. One of the anthropologists found ethical problems in studying how gypsies can be assimilated to the larger society. The other was stopped by ethical concerns from studying what happens when, due to a starvation situation, men in a specific cultural group could not fulfill their social obligation to act as hosts.

No access to data. Five researchers reported that they had put a problem aside due to the fact that they, within the range of acceptable efforts, could not get access to data concerning the phenomenon they wanted to study. One of the psychologists wanted to study brains from dead dyslectics but could not get access to brains and another wanted to study parliamentary members' perception of risk but felt that they would not agree to provide data. A further psychologist could not find methods to record meal situations in a sufficiently unobtrusive way. One of the anthropologists felt that the way he had presented himself to his informants did not allow him to collect the data needed since to collect data of this kind would militate against his presented identity. The other anthropologist had found that her identity (or lack of clear identity) was not acceptable to the group she wanted to work with. Two further psychologists had experienced the data access problem by not being able to construct the technical apparatus considered necessary to study the problem. For example, one of these psychologists reported that at the time it had not been possible to construct an EEG-measuring device stable enough to be carried around in everyday life.

Difficulties in operationalizing variables. Similarly, four informants reported insurmountable difficulties when attempting to operationalize independent or dependent variables in their studies. For example, one psychologist reported difficulties constructing recognition memory tests for TV-material. Another psychologist said that he had not been able to construct natural-language conversation of the kind needed to test peoples' ability to separate things you have said from things you have not said. An anthropologist had not been able to find ways how to get access to "the mental landscapes" of tourists. A further anthropologist had put a problem aside when he realized that data he had collected in another context was not appropriate for the analyses he now wanted to conduct.

Too high complexity. Seven researchers reported having given up a problem due to its complexity. One psychologist had wanted to study physiological reactions of persons with heart afflictions when they interacted with family members. A second psychologist had put a problem to the side because he got stuck in the mathematical calculations. The third had found the task to find the genetic substrate for the inherited component in dyslexia too complex. The last of the psychologists had given up studies of the relation between motivation and memory because he had not been able to satisfactorily solve the task of studying motivation experimentally. One of the social anthropologists had attempted to write a world history but had found it too complex, and the second had failed to quantify "thermodynamic flows". Furthermore, one of the three anthropologists had also given up the study of how societies and cultures behave when they are in deep crises, because the whole thing simply became too difficult to handle. Finally, one of the anthropologists had interviewed researchers about why they became researchers in their chosen discipline but had found it difficult to report the results as a scientific text, i.e. as "generalizable patterns."

Time and money. Two anthropologists reported having put a problem away due to lack of time to pursue it and three psychologists and one anthropologist reported abstaining from studying a problem because the necessary costs for the research were judged to be so large that they had felt it would have been unrealistic to get funding.

Other difficulties. One psychologist had put a problem to the side because he experienced lack of competence in the problem area. Another psychologist had been interested in studying the effect of climate on choice of means of transportation but had left the problem since he could see no practical implications of solving it since "one can't control the weather." A third psychologist reported that he on two occasions had put a problem aside when he had found out that another researcher had already published a study on the same problem.

Possible Problems

The researchers were also asked to describe a problem which they at present could consider researching but with respect to which they had not yet made up their mind. In this connection they were asked to state the arguments for and against researching the problem. Three researchers could not mention any problem of the type asked for.

Many of the researchers described more than one possible problem. Due to space limitations, no description will be given of the actual problems, only of the arguments given for and against taking them on. The purpose of asking for the description of the problems as such was to activate the specific reasons for or against these specific problems and to avoid that the researchers talked on a general abstract level. The researchers' arguments *in favor* of pursuing the problem will be presented first.

Exciting or important. Fifteen researchers mentioned that their problem was exciting or important. One additional psychologist saw that the problem would give him a chance to conduct cross-disciplinary research that he found exciting and one further anthropologist felt that his problem was a good example of a relevant topic.

Availability of possibilities and resources. A number of the researchers' reasons centered on the presence of possibilities and availability of resources for the research. Two psychologists noted that *very little research had previously been conducted* in the area they were considering. One of these psychologists also noted that he had an interested and suitable *graduate student available* and furthermore that he had *access to patients* to study. Furthermore, he "had some *ideas* that one could conduct research on in relation to this problem". Three anthropologists in connection with their problem also noted the availability of ideas. Another psychologist said that he had *access to good techniques* for studying the problem. Two anthropologists saw the problem as "*fully researchable*" and one noted that research on the problem was *compatible with his other work tasks.* One psychologist noted that he already had *data available* that could be used when researching the problem. Another type of resource argument was given by a psychologist who stated that he had *already spent (mental) resources* on the problem, "I have already familiarized myself with the problem and thought a lot about it." Finally, two researchers in psychology mentioned in favor of the problem that they thought that it would be *possible to get research money* to finance research on the problem.

In line with academic social needs. Four psychologists and one anthropologist mentioned as an asset that the research connected with the problem was in line with their academic social needs. For example, one psychologist said that his grants were running out and that he was thinking about new projects. Another psychologist stated as a pro argument that research on the problem was a part of the research niche that he officially represented. A further psychologist said as an argument in favor that his present career level allowed him to conduct "high risk projects". Yet another psychologist saw it as positive that he had found that other researchers were interested in the problem. Finally, the anthropologist mentioned as an asset that it did not matter if his research colleagues were interested or not since he was close to retirement.

Possibility to work with certain methods. Finally, with respect to the arguments in favor of taking on the problem, three informants mentioned in favor of the problem that researching it would allow them to work with research methods which they enjoyed or would like to work with. One of the anthropologists saw a chance to conduct fieldwork and the other to "work theoretically". Next, the arguments presented by the researchers as speaking *against* taking on a problem they were attracted by are presented.

Lack of motivation. Two psychologists and six anthropologists mentioned various types of lacking motivation as deterring them from starting research on the problem. Two informants simply reported a general negative, heavy feeling in connection with the problem. Another anthropologist said that he felt tired of the whole research area, "I'm fed up with it". Three researchers felt they lacked interest for some specific part of the research work. For one of them negotiating with the County Council was not appealing and one of others did not feel fully motivated to start "to administrate a large project with many collaborators, long trips and grant applications." An anthropologist mentioned that he was not sure he felt motivated enough to conduct the interviewing which researching the problem would involve. Furthermore, he was unsure about his motivation in general to conduct empirical research. Another anthropologist mentioned as a de-motivating factor that he was not sure he would like the people he would encounter in his fieldwork. Finally, one anthropologist said that the problem he was considering was quite theoretical and that he preferred research that allowed him to write in a non-theoretical way.

Disciplinary difficulties. Seven psychologists mentioned disciplinary difficulties as an argument against starting research on the problem. One of them experienced difficulties even formulating more specific problems which were new and which would lead to new results in the area. Three of these researchers experienced conceptual difficulties in connection with the problem. One found difficulties conceptualizing the phenomenon and was unsure how he could conduct effective research on it. The second stated that the phenomenon in which he was interested was very complex and that there was no good theory for it. He had experience of a similar situation in the past that had given rise to much labor and meager results. As the present problem involved much risk he didn't know if he would dare to take it on. The third researcher feared that there would be some statistical trap in connection with his problem that he had not detected. Another of the seven psychologists did not know how he should measure his variables. Finally, for one the difficulties were on the technical side; he did not know of any technical possibilities to measure the phenomena he was interested in on the time scale necessary.

Lack of knowledge and practical possibilities. Three researchers reported *lacking competence* in the area as withholding them from pursuing the problem. One psychologist and four anthropologists mentioned *lack of time* as a deterring factor. Two of the researchers in this connection also said that they wanted to complete the research activities they already had going. Two of the anthropologists wanted to have time for non-scientific activities such as writing and paining. Three researchers mentioned practical problems with establishing *contact with co-workers* or getting *access to subjects.* One further psychologist was not sure that it would be realistic to carry out the research in practice since he was *not sure if the subjects would cooperate* even though he had access to them. Four psychologists feared that they would have *difficulties getting research money* for studying the problem.

Not academically opportunistic. Two psychologists saw the problem as not being opportunistic within the academic setting. One thought that the phenomenon he considered studying was not sufficiently international to create interest among researchers outside of Sweden. The other argued that the problem would lead to very expensive research with results which would only have small practical consequences. Furthermore, this researcher noted that a lot of research had already been conducted in the area, thereby maybe implying that there might not be so much more to find out. Yet another psychologist feared that the research he was considering had already been carried out but that he had missed reading the appropriate articles.

Social, political or ethical consequences. Three anthropologists mentioned that they feared negative consequences of a political and social kind as a possible result of conducting the research. Finally, two researchers saw ethical problems as a hindrance.

Discussion

The function and place of problems in the research process of course depends on how one defines science. However, if a minimal definition of science is accepted which says that research is a social, collective and communicative enterprise, based on systematic, critical thinking, certain conclusions are possible. Although some researchers may work in isolation, their results have to be communicated to the research community where they are critically evaluated. Each scientist contributes grist to the mill, i.e. the concerned community of researchers which critically evaluates and possibly integrates the results into further research.

Problems appear to have many functions in science, not all of which are located on the level of the individual researcher. From the results of this study it appears that there is no *necessary* role for problems in the research process, *except* as a *means of communication.* That is, it is not strictly necessary for a researcher to have a problem in order to conduct research. Some of the researchers presented accounts of when they themselves had been in a situation where they were performing research without researching a problem that they were aware of. The most common case is presumably anthropological fieldwork where the task for the researcher, initially at least, often is to get acquainted with the phenomenon, i.e. to get to know the culture he or she is to study, but other contexts were also reported. At the same time, somewhere in the research process, thoughts have to be focused on a theme or topic, if nowhere else in the process, then at least in connection with reporting the research, most commonly in an oral and written mode.

The communicative function of problems explains why a problem, if not identified or formulated before, as a minimal move, will always be tucked in at the end of the research; the results have to be communicated to an audience, i.e. the scientific community. Thus, one could say that it is the social nature of research which at the end *necessitates* the presence of problems in the research process. Along the same line, one can argue that problems, by acting as social communicative glue, help to keep the research community together. The social communicative function of problems also includes allowing the researchers to formulate grant proposals in order to secure resources for their research. Problems also make it possible for researchers to communicate about their research during the research process.

However, at the same time, naturally, there are of course other *possible* (and important) functions for problems in research. These functions occur both on the level of the individual researcher and on the level of the research community. Examples of functions on the individual level, are *focusing* and *structuring* the researchers' thinking and activities, and *motivating* the researcher to conduct the research activity. Individual researchers differ with respect to the degree to which they utilize these functions and with respect to at what stage of the research process they utilize them. This appears to be a matter of the demands from chosen research methodology and disciplinary tradition and also a matter of the individual researcher's research style.

A function of problems at the level of the research community is that they appear to be an important *means of socialization* for undergraduate and graduate students into the disciplinary tradition. Some of the researchers stressed that they always taught their students the importance of formulating a clear problem before starting the actual research, although they did not always do so themselves.

This study does not primarily focus on the formation of problems but more on how problems are dealt with once they are identified. At the same time problem formation and handling overlap since the handling of identified problems also influences their further formation. Furthermore, there are individual variations in the style of researchers in connection with their problem formation. Some researchers reported an abundance of problems; others that problems were a more scarce resource. It is also clear that the generation of problems often is planned and does not always occur totally spontaneously. Some researchers trust spontaneous formation processes to deliver problems, for example out of their ongoing research, but others take a more active approach when it comes to aiding the formation process. This is done, for example, by purposefully changing environment in order to get new ideas, by communicating with other researchers or by reading the research literature. It is also clear that researchers are aware that on certain occasions (such as at the time for grant applications) they have to have a fresh set of research ideas available. Some prepare for this by collecting problem possibilities in "folders" or by taking notes in a notebook or use strategies such as reviewing their own or their colleagues' research to see what is the natural thing to do next.

The interviews give support to the importance of the profit-calculating way of thinking among researchers, pointed out by writers in the sociology of knowledge (e.g., Mulkay, 1972; Fuller, 1992). The researchers in the present study report taking into consideration whether the problem will create interest in the research community, if it has been researched before, if it will promote the scientist's career interests (for example by bringing in new research money when the available ones are running out or by allowing the researcher to fill out the research niche which he or she feels obligated to occupy), etc. Thus, it is quite clear that simply attending to researchers' "psychological properties", for example their early experiences, is completely insufficient if the agenda is to understand why certain problems are selected and not others.

However, there are some components of how researchers handle problems that are not well captured by the sociology of knowledge. The researchers' motives for taking on a problem are richer than simply evaluating the possible profit in the research community in terms of recognition or mobilization of resources in one's own direction. Problems are experienced as interesting, or not, and they are evaluated with respect to how much fun the researcher thinks he or she would have working with them. They are also evaluated with respect to how much work the problem will imply and the amount of work predicted in connection with a specific problem is compared not just with other possible problems to tackle, as might be assumed from a purely social competitive perspective. It is also compared with the researcher's experienced level of interest in the problem, his or her other interests and obligations in life and what other fringe benefits (such as getting to work with enjoyable research methods, or meeting nice people) the problem has. Thus, the fact that researchers also have other interests besides gaining recognition and research resources clearly influences the way researchers take a stand with respect to whether to take on a certain problem or not.

The demand for pleasure may be an important factor in connection with decisions of whether or not to take on a problem. This may be particularly important for experienced and established researchers but may also to varying degrees be important for beginning researchers. It could be that the need for pleasure should be counted among the factors which bring creativity and novelty into science since such motives may make the researcher pay less attention to which problems can be quickly and effectively sold to the research community, including its grant agencies.

In brief, in order to better understand how researchers handle possible problems, more perspectives are needed than those merely focusing on the cognitive content of the discipline, the dynamic psychological processes of the researchers and on the sociology of knowledge. For example, we also need to add perspectives focused on the particular researcher's local, concrete, life situation and on the researchers' subjective reactions to the problem they are considering.

The study of problems may also be able to provide information about similarities and differences between disciplines. In the present study senior researchers from cognitive psychology (and neighboring areas) and social anthropology (including ethnology) were interviewed. First of all, it is clear that the researchers studied here have more freedom in their choice of problems than the middle career researchers in natural science and technical R&D institutions studied by Ziman (1987). The researchers studied by Ziman were part of larger groups and used expensive equipment. The researchers reported often being told by management which problems to tackle next. In contrast, the researchers in the present usually worked with graduate students or worked alone and, to a higher extent than Ziman's informants, themselves took initiatives and made their own choices, i.e. had more of the freedom traditionally associated with university research. The type of discipline, type of research institutions and the career level involved may all have contributed to the difference found in freedom of problem choice in the two studies.

In the present study, few differences between the disciplines of psychology and social anthropology were found with respect to the attitudes to problems evident in the researchers' answers. For example, the researchers in both disciplines to a relatively equal extent stressed the importance of problems being well integrated with theory. Furthermore, no clear differences were found with respect to how necessary problems as such were seen to be in the research process. These results indicate that the studied disciplines do not differ substantially on the normative level with respect to the status given to problems.

With respect to the reported research-practice some differences were found between psychology and social anthropology. More psychologists than anthropologists mentioned conceptual and methodological difficulties as arguments for not taking on a problem they were tempted to take on. Furthermore, more psychologists mentioned that problems were generated out of their previous research and more anthropologists stressed that their problems changed over time in the research process.

These findings are compatible with the notion that the disciplinary content of (cognitive and related parts of) psychology is more tightly structured with respect to theory and method and thus exerts a higher pressure on the researcher compared with the disciplinary content of anthropology. The further finding that a minority of about an equal size in both disciplines reported that they had conducted research without problems does not necessarily argue against this conclusion since these researchers in psychology compared with anthropology to a higher extent were oriented towards applied research.

In this study researchers have been interviewed about certain aspects of their own work. The question may be asked if such interviewing is of any use at all. Maybe, as has been suggested by some writers, scientists will only relate such details of the whole picture, and combine fragments of their stories in a way that will lead to an increase, or at least not a decrease, in the recognition they are seeking. For example, Mulkay (1976: 214) suggested "A scientist's typical account of why he took up a particular line of research at a particular time will stress technical considerations, e.g. 'the problems were scientifically interesting', 'suitable techniques were available'."

However, and in contrast to Mulkay's suggestion, in this study the researchers were more outspoken. They freely provided information about their calculations about what would pay off in the scientific community, about their motivational lapses and about parts of their activities, which were not altogether flattering for themselves. Furthermore, at least some of the researchers were anxious that their anonymity would not be protected. This indicates that they themselves may have felt that they had revealed more than it would be socially recommendable for them to do.

One reason for why the researchers were less inhibited and less calculating in their answers than might be expected, may be that the zeitgeist in general has become more allowing. This may have influenced the researchers' own self-understanding and their understanding of what can be said about back-stage events in science. Twenty years have passed since Mulkay wrote his chapter. A further reason may be that the scientists were all social scientists. Thus, they may be more aware of the kind of processes they were asked about than researchers in physics or biology who, most often are the ones studied in the sociology of science. Many social scientists are, for example, presumably aware of the debates occurring in the cultural sciences concerning the contextual conditions for development of scientific knowledge.

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Notes

1 Only one intended informant (an anthropologist) declined being interviewed. The interviews were semi-structured. The questions were sent in advance to about half of the informants, to the rest a copy of the questions was given at the start of the interview. Six of the interviews were conducted by two interviewers (the author and Jan Bärmark) and the rest by one of these two persons. All interviews were tape recorded with consent from the informant, usually with the promise that their anonymity would be respected. The average duration of an interview was about two hours (range from one hour and 15 minutes to two hours and 30 minutes). All interviews were transcribed in full. For a few of the interviews, due to idiosyncratic reasons, one or a few questions were not asked. The answers to about one third of the questions in the interviews with two psychologists were lost due to a faulty tape recorder.

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