

Gummesson, E. (1991) **Qualitative Methods in Management Research** (Revised Edition)
London: Sage.

Reviewed by *Frank Dybdal Jensen* and *Claus Rerup*, Research Fellows at Department of
Organization and Management, The Aarhus School of Business, Haslegaardsvej 10, 8210 Aarhus
V.

This amounts to nothing and should be rejected

A rising number of academics in the field of management research are in the course of their career involved in management consulting activities on a part-time basis. They may from time to time get struck by the feeling that the excellent working conditions as a consultant - firsthand access to vast amount of fresh data and the clients' friendly, almost humble, cooperation throughout the project - could really add something of great potency to their own empirical academic research. The role as a change-oriented, dynamic management consultant compared with the role of being an anaemic academic carrying out a strained research project is almost as different experiences as hot and cold showers.

Gummesson's book is about hot and cold showers, and the possibilities of mixing the water in order to get more welltempered and healthy bathing conditions for both the management researcher and the consultant. The project undertaken in the book is ambitious, strongly personal and value-loaded, from time to time provocative, in addressing a broad range of topics concerning the relationship between the roles and activities performed by the management consultant and the academic researcher, respectively. It is not a "how-to-do" book on qualitative research, on the contrary, most of the problems raised by Gummesson are open ended, and urge the reader to make his own stands. It is an excellent written and extremely reflective book on current subjects of high importance for the development of more adequate research methods. Furthermore, it is a thoughtful examination of the intricate task of research evaluation. Therefore, Gummesson contributes significantly to the ongoing, but apparently in his opinion too narrow and vague, discourse within the field of qualitative research by taking position on many controversial standpoints. The author's main point is that it is possible to practice better research if the researcher is capable of combining the roles of a consultant and of a researcher.

In essence, the author concentrates on the following three areas of inquiry as the most important challenges of mutual interest for the researcher and the consultant:

Access to reality through different roles
Preunderstanding and understanding
Quality assessment

These challenges are presented and commented below.

Access to reality through different roles

"Access to reality" is the first challenge that Gummesson focuses on.

The reality - or the tiny segment of the so-called reality - that the author seeks access to is the realms of decision-making, implementation, and change of processes in a business administration environment.

By gaining "access" is meant the task of getting close to the object of study, in order to be able to find out what is really going on in the field of inquiry. The notion of "access" is further subdivided into three types of access: money to finance the project, access to the system, and access to individuals. To succeed in the "access game" researchers must engage in different roles. Some of these roles are defined by the object of study, and others by the researchers themselves. Gummesson's point is that researchers are often treated as visitors when entering an organization and therefore faces insuperable difficulties because they cannot get access to the object of study on a neutral or objective basis. The fallacy of the illusion of the researcher as a neutral collector of "facts" is in a brilliant way illustrated by the following conversation from p. 22 in the book:

(E=executive, A=author)

E: Some type of professor is over here again from the USA wanting to interview us about strategy. He had a long questionnaire with him containing about 50 factors. He wants to find out which factors are important when we decide to enter a new market.

A: What are these factors?

E: Just the usual sort of thing, market potential, competition, political stability, etc.

A: What did you reply?

E: Well you know, you go through the list and tick off a few factors, show him some marketing plans and then send him off to meet a few other people. I have no idea what he gets out of it all.

A: You don't seem too enthusiastic about his research.

E: No, it doesn't really work like that in practice, does it? Let me tell you what happened when we decided to enter a country in Latin America. Four of us got together over dinner in New York: a divisional director, the vice president of R&D, a department head, and myself. We sat and chatted around the problem but just couldn't agree. In the end we had to take a vote: two in favor, one against, and one undecided. Well that was it, in we went. Two men flew down on the following Thursday to check the lie of the land.

A: Did you tell the professor about all this?

E: Of course not! He might have thought that we're not serious.

Misunderstandings in role expectations and lack of trust from informants, or mental access, seem to threaten to lock off the researcher from valuable insights. A gap of perception divides research and management practice, and the result is that empirical research performs poorly in reflecting and conceptualizing business practice. This gap of perception could also in some instances merge into the very nonsatisfactory situation that the researcher loses physical, continued access to the object of study. To minimize the risk of occurrence of this situation, Gummesson claims that different roles should be involved, e.g. as researcher and consultant in order to approach the object of study from an action research paradigm perspective (see below). In this way, it is possible to develop a wider spectrum of possibilities to stabilize the relations -often controlled by gatekeepers - that make access possible on a workable basis. Researchers should act as *change agents*, since the traditional conception of the researcher as a low-involved bystander will not provide proper access to the knowledge of the object studied. The researcher should leave behind his preferences for the detached position and overcome his fear of not being "objective" when utilizing his own personal

experience - his preunderstanding. Gummesson's combined - or rather complementary - approach to management research the - *action science paradigm* - is worked out as a synthesis derived from the consultant and the scientific paradigm as they are introduced and developed by the author, respectively.

This "synthetic" paradigm is neither a positivistic or hermeneutic one; it is rather an attempt to combine the requirements of research and consultancy into the praxis of *action science* within the framework of a single project - a case study - or a program - a series of case studies. Another lesson that the researcher, at least in Gummesson's opinion, could make from the praxis of consultancy is the practice of *methodological triangulation* and *paradigmatic reflectivity* in order to increase the chances that "facts" can be obtained from the object of study. According to the author, the use of two or more methods to analyze the same research problem would increase the reliability of the results. Moreover, triangulation is claimed to help overcoming the problem of methodological redundancy since the traditional academic researcher "characteristically uses in his next step, the very methods that he and others have used before on the same problem" (p. 122). So even though the researcher is trained in assessing theoretical and methodological problems, he must become aware of his private idiosyncrasies or institutional determined narrowmindedness. He must be able to assess his preunderstanding to understand why he perceives and acts the way he does.

Understanding and preunderstanding

According to Gummesson, the consultant pecks at theory and contributes to practice while the researcher pecks at practice and contributes to theory. This is only one difference between the researcher and the consultant. They have a lot in common but are also separated by lots of differences. The author is focused on the possibilities and limitations of the use of qualitative methods for data collection and analysis. Furthermore, and perhaps more interesting, is the insightful discussion of the impact that knowledge, based on personal experience, has on the way that the object of study is approached.

The author argues that substantial quality improvements could be made in the field of management *research* if there was a higher degree of awareness directed towards the opportunities of conducting research via the role and techniques utilized in management consultancy. Correspondingly, management *consultants* without any research background are in general unaware of methodological and paradigmatic problems. They are not conscious of the implicit assumptions that guide their work and are therefore also ignorant of the possible implications that this has on the quality of the consultancy services provided. In the author's terms, these assumptions have become part of the consultants *preunderstanding* and are therefore not part of his formal and explicitly stated analytical considerations. But Gummesson is also concerned with the *researcher's* personal and scientific values - his paradigm platform. He states that this platform always is - and of course unavoidably must be - subjectively chosen. Once this platform is chosen, research can be carried out with varying degrees of objectivity, or perhaps more properly stated: methodological rigorism. The author claims that: "Mainstream scientists who just apply approved methods without being aware of the subjective foundation of their activities are not scientists: They are technicians" (p. 14).

To become reflective, scrupulous researchers, the author prescribes the practice of *double loop learning*. But first of all we think that in order to become a scientist and not a technician the researcher needs to identify his own self, and thereby develop selfawareness by starting" to act

towards himself and guide himself in his actions toward others on the basis of the kind of object he is to himself" (Blumer, 1969, p. 12). In order to become an object to ourselves we need to see ourselves from the outside. The fact that the researcher has a self enables him to interact with himself. Selfinteraction exists fundamentally as a proces of making indication to oneself. To interact with oneself, the researcher must account and audit his personal values, at least to himself.

In practice, unfortunately, the paradigm of values that governs individual action and cognition often resists identification. It is located in a tacit cognitive map. Attempting to identify our paradigm we must be aware of the two concepts: Theory of *action* and theory in *use* (Agryis and Schön, 1978). The first is about how we *claim* that we think and act while the latter is addressing the way we *actually* think and act.

To perform better, that is in this context, more consciously, in both academia and consultancy, Gummesson urges us in the words of Habermas to free ourselves - and in the spirit of critical theory also emancipate the individuals involved in the objects of study - from conditions and dependencies that we may so far have considered as given or fixed. In other words: We should constantly question our conception of reality. Furthermore, we must develop a competence enabling us to reframe our way of preceiving social reality in order to understand that the subject of investigation keeps changing and expanding. Further reading on reflective research can be found in Alvesson & Sköldbberg (1994). Theory building and research results should - in accordance with the tradition of Karl Popper - not be seen as arrival at any universal truth but as a point of departure for further inquiry and attempts to falsify what is found out. No theory can claim to state something true, only that it provides the best possible explanation of a phenomena for the time being.

This last statement has a close affinity to the task of assessing the quality of research and consultant work, since the researchers or the consultants proclaiming that their methods possess qualities as superiority or universality show, according to the author, symptoms of intellectual rigor mortis and opportunistic attempts to dictate fashion. Therefore dependable measures determining what should be considered good research must be developed.

Quality assessment

The author fearlessly faces the dangers on his Oddysé: Where other writers stand back from looking too deep in, Gummesson without hesitancy continues his exploration. He goes to see the many-headed "Quality monster" and bravely fights the multifarious, and in some aspects tabooed, "Evaluation Criteria creature" in his attempt to determine what should be considered "good" research and consultancy performance.

The author is very much aware of the ambigious notion of "Quality" and the fact that judging the quality of research and consultancy work, respectively, should be governed by different values since the activities seek to serve different purposes and needs. But soon it becomes painfully clear that the delicate task of identifying and choosing the relevant criteria for drawing up an adequate quality assessment procedure is only solving part of the problem. The real severe difficulties emerge when those criteria chosen should be ranked, given weights, and added up, to produce a final quality assessment statement. Any attempt to compile a universal and rationally defensible method for quality assessment is doomed to fail. Accordingly, Gummesson quite frankly and very provocatively ends up with only one commonly accepted and prevalent criterion used for identifying "good research": "A scientific report should be boring and difficult to read" (p. 160).

Though, some general applicable quality criteria are presented as being workable devices for the evaluation of case study research. But still, any ranking, weighting or addition scheme resists occurrence. Gummesson ends up listing the following set of quality criteria, which are not ranked, not weighted and not given rules for addition:

1. A research project should be conducted in a manner that allows the reader to draw his own conclusions
 - this means that a detailed description of the research process, methods and coding procedures, and also a well-documented and rich case-description should be available to the reader
2. Researchers should present their paradigm
3. The research project should possess credibility
 - of importance here is: *internal consistency*, that is, the conclusions should be in accordance with one another, and *external consistency*, the actors (informants) should be able to recognize the reality presented in the report
4. The researcher should have adequate access to the object studied
5. A statement should be made regarding the validity of the research project
 - of interest here is to whom the results apply, and whether other studies or theory and models in the literature confirm the findings
6. The research should make a contribution
 - that is, the research should take up problems of relevance to the scientific and public community and provide increased knowledge
7. The research process should be dynamic
 - this means that the researcher must be in a continuous state of learning by communicating impressions, hypotheses, and so forth to people involved in the project and to the research community
8. The researcher should have commitment and integrity
 - that is, at the same time the researcher must be deeply involved in the project and also be able to retain a certain distance
9. As an individual, the researcher should satisfy certain requirements
 - the researcher must possess an adequate amount of preunderstanding, candour, and honesty

This listing does not seem to provide any new insights into the ongoing discussion of how evaluation of case studies should be made. Compared with the research evaluation paradigm from the natural sciences, only the *validity* criterion in a somewhat moderated version is retained, namely in the fifth quality criterion listed above. The remaining two elements in the holy Trinity of positivism, namely *reliability* and *generalizability* are not explicitly treated in Gummesson's listing. The *reliability* criterion is difficult to handle in qualitative studies within the field of social sciences, but Gummesson's idea of external consistency could to some extent help to guarantee some permanence and trustworthiness in the material used for data analysis. Concerning *generalizability*, this criterion is to some extent covered by the author's sixth commandment "the research project should provide increased knowledge", since the applicability of knowledge to a wider environment than the studied case is conditional on the generalizability of research results.

Concludingly, the holy Trinity of the positivistic paradigm from the natural sciences seems to have survived Gummesson's inquiry, and he surely has something of considerable importance to add in his presented evaluation scheme. But still, the ongoing discussion in the field of evaluation of qualitative research has by no means reached any conclusions, and probably never will.

Concluding remarks

The reader might be puzzled about the title attached to this review of Gummesson's book. The title is, in our opinion, closely related to the commonly held attitude against something new and provocative: Our first reaction is simply to reject it as worthless. The title is inspired by an anecdote referred by the author about an article that Bertil Ohlin submitted to a prestigious British journal which used John Maynard Keynes as a reviewer. The article by Ohlin spelled out ideas that later was rewarded by the Nobel Prize in economic sciences, but Keynes proposed that the article "amounts to nothing and should be rejected." Gummesson's book introduces lots of new concepts and perspectives on qualitative research and the conception of the researcher's roles. Some of the ideas and views might seem somewhat idealistic and naïve, but they are without any doubt a very important contribution to the ongoing discourse in the field of qualitative research. Whether Gummesson's "synthetic" action science paradigm is adopted by researchers and will become part of the future ideas of "good research methodology" cannot be predicted, but to judge that his project "amounts to nothing and should be neglected" might well show out to be as erroneous as Keynes' above mentioned evaluation of Ohlin's article.

References

- Alvesson, M. & Skoldberg, K. (1994). *Tolkning och Reflection. Vetenskapsfilosofi och kvalitativ metod*. Studentlitteratur, Lund, Sweden.
- Argyris, C. & Schön, D.A. (1978). *Organizational Learning: A Theory of Action Perspective*. Addison-Wesley Publishing Company.
- Blumer, H. (1969). *Symbolic Interactionism. Perspective and Method*. Prentice-Hall Inc., USA.