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Beyond the Pioneer Days! Where is Reception Research Going?[1]

Cross-Fertilization of Paradigms: A Synthesizing Approach to Qualitative Audience Research

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In the fall of 1997 a poster campaign was launched in all commuter train stations in the Greater Copenhagen area to inform people that it was possible to take a bicycle on the trains. The dominant element of the poster is a big photograph of a young man, tonsured and wearing a leather jacket, and with a pleased smile on his face. He is sitting in a train next to a bicycle and with a basket on his lap.

”Ole is faster than Bjarne” says the headline, and below the picture it says that you can take your bike with you on the commuter train free of charge on Saturdays and Sundays, and that this is a continuation of a trial period.

What did the sender want to say with this poster? – The manager of the Danish railway company DSB’s commuter train division says:

> The bicycle poster shows a picture of a nice and clean looking young man sitting in the commuter train next to his bike with a basket on his lap. In the basket we can see part of a book on wild mushrooms – in other words he is going out to gather mushrooms in the forest – taking his bike on the train.
> (*Ud og Se*, the DSB monthly magazine, March 1998)

To this can be added a few words about the headline: Most likely there is a play on the cultural knowledge of the Danish reader that Ole (who must be the one on the picture since he is both the main character of the picture and the primary focus of the sentence) is quicker than Bjarne – and who is Bjarne? In 1997 it was still pretty obvious that it had to be Bjarne Riis, the Danish Tour of France cycling hero. In other words an attempt to be funny, a small joke: When Ole takes his bike on the train he gets to where he is going faster than even Bjarne Riis.

But not everybody experienced the poster this way. Here is what a commuter train passenger with a bicycle had to say about it:

> The poster depicts a skinhead with a basket full of stolen goods, ready to confirm that Bjarne is a lot smarter than Brian. (…) The poster cautions us against offending troublemakers on the commuter trains.
> (*Ud og Se*, March 1998)

I have chosen to start with this poster, because here we are plunged headlong into a number of the issues at stake in reception research.

- firstly, the receivers often get something completely different out of a message than what the sender intended to communicate
- secondly, we have to consider what it means that a ’text’ has a message, what reception research since Hall (1973) has called a ’preferred meaning’. If a text has such a preferred reading, is that a meaning that is identical with the intention of the sender; a meaning inherent in the textual structure; or the meaning ‘preferred’ by most of the receivers?
- thirdly, how can we characterize the actual reading of the passenger here: It is ’divergent’ from

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the intention of the sender, but does that mean that we should see this actual reading as an 'oppositional' reading – a concept favored by early reception research, because the focus was so singularly on the role of the media as ideological oppressors in the class struggle. (Hall 1973, Morley 1980)?

- fourthly, how should we view actual readings that are in some sense 'wrong'? Or does it make sense at all to talk about 'wrong readings'? – Here it probably does, because we have to do with goal-directed communication, in which the sender 'wants something' with his message. But what if we had to do with a somewhat divergent reading of a character in a TV series: then perhaps there are only 'actual readings', rather than wrong readings?
- fifthly, where do the actual readings come from, how do they come about? One theory operates with what is called 'empty spaces' (Mikkelsen 1989, inspired by Eco and Iser), which so to speak *locate* the reader's opportunities for meaning creation to certain signs or absences in the text. Another theory operates with a sign concept where it is in principle *every* image or language sign in the text that the reader actualizes on the basis of his or her semiotic repertoire (Peirce 1985; Jensen 1995).
- sixthly, we cannot just consider the reader's maybe divergent interpretation of the elements of which the text in fact consists: sometimes the reader will *add* something *to* the text, write something into it that is not there, here 'Brian'. In Denmark the name 'Brian' is a working-class adoption from English which has often been given connotations like 'troublemaker' in popular jargon (schoolteachers may for instance describe an obnoxious pupil as 'a Brian'), so the reader's response must be viewed in this context.
- a seventh point is the question of *how* we can obtain knowledge about how the receiver reads the language and images of texts, i.e. *what methods* we can use. In the example here my knowledge about the reading is not from a major empirical investigation of the poster, rather it is due to the fact that a male reader wrote a letter to the DSB magazine *Ud og Se* in which he complained about this misleading poster. He had asked the staff on a station whether he could take his bike on the commuter train and had been asked to study the poster, which he *had* noticed earlier, but only took a closer look at now. In connection with a campaign this is rather interesting: The man was already motivated for reading the poster, but the way it was made prevented him from realizing its relevancy! As regards the campaign it is interesting how many people share his reading, and in this respect the sender is confident. The DSB manager says, 'We believe that the message is easy to interpret correctly – and that it has been interpreted correctly by the majority of our passengers' (*Ud og Se*, March 1998). The manager is probably right.

The main part of this lecture will address the last of these points: *methodological issues* in the history of reception research and its *empirical practice*, i.e. what concepts, tools and procedures can we use to create better insights into how people in today's society use and experience the mass media?

In this context it has been the function of the poster example to give readers a feeling for the universe of understanding that I move in when I discuss qualitative audience research. As a shorthand for this rather broad field of research I sometimes use the term 'reception research'', which I take to include research which – mainly through in-depth interviews – investigates people's experience of certain media products and/or their uses of media in their everyday lives. It does not include 'media ethnography' in the strict sense of the term, which has participant observation as its primary method of investigation (Schrøder 1994; Drotner 1994). But before I start the discussion of methodology, I want to say a little about the title of this article.

Metaphors – An Attempt to Control the Reception of a Title

I would like to control your expectations of this lecture by giving it a title that creates the right set of expectations – in other words a classic problem in planned communication: How can I in one phrase make you as an audience share my retrospective view on the development within reception research from app. 1980 to the present day, when in my opinion we are not facing a new shift of paradigms, but only a new step in a continuous development process?

A number of metaphors could be offered. One of these compares reception history with a life span. I could have called the lecture 'As reception research grows up'. But how would you interpret that? You all have your notions about what a life span con-

tains: birth, childhood ('the first steps'), puberty, youth, adulthood, old age, death.

I am not really happy about your interpretations of this metaphor: Some of you may still believe that an abortion might have been better back in the 1980s, because the child is a scientific monstrosity. Others may hold the opinion that reception research is quite far from growing up, and maybe never will.

I could also have chosen to call the lecture 'Reception research at the crossroads', a bit of an overused metaphor in many connections, and used also by Sonia Livingstone in an article from 1998 (Livingstone 1998b). However, we in reception research do not need to leave anything behind us. There is nothing in our earlier work we need to distance ourselves from. We had to follow the road we took in order to reach the point we are at to day, without any regrets. Moreover, if I claim that we are at a crossroads, it might be taken to mean that there is only one of the roads facing us that is the right one to travel, which happens to be the road that I am now going to guide you to. But this would be a misrepresentation of my view.

Then all of a sudden the idea of a colonization process came to me, i.e. the pioneer metaphor. Pioneers are people who set out from their safe homesteads to subjugate virginal land, which they gradually take into possession, cultivate, develop, and civilize. Pioneers do not have a lot of resources, they put all their money on one horse, they are surrounded by enemies, the reactions from those who stayed at home range from bitter resistance to the shaking of heads, and maybe some wanted to go but did not dare, or were beaten into place. Moreover, there is an ad hoc feeling about pioneer life, you have to use what you can find, you try out various possibilities.

I would like you to consider the early reception researchers of the 1980s as people who could not possibly have built a motorway or a cathedral, because the situtation demanded of them that they started building a log cabin or a footbridge. And then I would like you to consider the present day reception researchers as people who have started building more advanced scientific constructions, and who will get better at doing that over the years to come.

The Pioneer Days

Reception of Advertisements: Qualitative Research

As an example of reception research in the pioneer days I have chosen to use Claus Buhl's advertisement reception research from the mid 1980s (Buhl 1991). Of course I might just as well have given one of the usual examples – Morley (1980, 1986), Hobson (1982), Radway (1984) from international reception research, or from a Danish context, Niels Aage Nielsen (1982), Else Jensen and Birgitte Tufte (1984), Anne Hjorth (1985), Klaus Bruhn Jensen (1986) or Kim Schrøder (1988) – but I thought it would be refreshing to illustrate early reception research with a different example.

By choosing Buhl's study I may be pushing it towards the extreme, but this will enable me to make my main point clearer than I would have been able to, had I used one of the other studies. And Buhl's study is far from atypical of the early days.

Buhl wanted to find out whether the messages in advertising reach the receivers, as was the opinion of both the advertising industry and the critics of advertising, in their oppositional camps. Buhl thus defined his project in opposition both to the quantitative behaviorist marketing effect research and to the qualitative, ideological-critique kind of advertising research, maintaining (correctly) that both these approaches build on the so-called 'hypodermic needle' theory, i.e. the notion that the message of the advertisement is so to speak injected under the skin of the receiver.

Both marketing research and ideological criticism thus believe that advertisements work. They have a clear-cut message – be it a manifest message selling products or a latent message selling consumer ideology and seducing the defenseless masses in order to make them put up with the oppression of the capitalist system.

(Here I have to remember to mention that the perspective of ideological critique argued against by Claus Buhl is by and large identical with the view I myself put forward in the book *The Language of Advertising* (Vestergaard & Schrøder 1985) – which is a good example of how the dominant 'truth' of any given time is never an absolute truth).

Buhl investigates a number of advertisements, among these an American Express ad from the mid 1980s, in order to explore whether the intended message goes through to the receivers. It does not. Buhl's empirical design comprises both the senders and receivers of the ad, which enables him to relate

the communicative intention to the message realized by readers. He makes depth interviews with 6 respondents, who are interviewed in their homes about 9 different ads. The respondents were selected from different age and educational groups, and comprised 3 men and 3 women.

The American Express ad shows an evening picture of the Ducal Palace Square in Venice empty of people. On the back of one of the green folding chairs is a forgotten red handbag. The headline says, 'If you lose your American Express card, you'll get a new one – on the spot – free'.

Without going into details with the readings, one can say that the 6 respondents experience the ad very differently. One man quite rightly notices the forgotten red handbag on the chair on the Venetian square, but then immediately gets connotations about his family summer holidays and the forgetfulness of his wife – not about business travel as the sender had imagined.

Another man does not really notice the handbag, but he is struck by the green folding chairs, which, however, do not make much sense to him. A female reader gets in a decidedly bad mood about the situation with the forgotten handbag and never reaches the optimism that the ad wanted to promote.

Buhl concludes that the actualized message of the ad 'depends on subjective meaning processes and fantasies' and that 'one ad becomes many ads, i.e. the different realized meanings that reflect different people's different life situations and expectations' (Buhl 1991:119).

The meaning of an ad can thus best be characterized as 'complexity bordering on chaos'! (ibid.). Another study of advertisement reception talks about the readers' actualized meanings of advertisements as 'a hell of connotations' (Mick & Politi 1989). Buhl goes on to observe that, 'It is impossible to say the same to everybody at the same time, with the same effect and consequences' (ibid.). In other words there is no 'preferred reading', which is transmitted from the sender or the text to the receiver. There are in fact only actualized meanings.

In passing it is interesting to note that Claus Buhl is now the manager of a big advertising company that owes its existence to its ability to influence consumers through advertising messages…

Buhl's investigation broke new ground back in the pioneer days, and more than most he felt the antagonism of the established research environments of business school marketing research, where for a number of years he tried to cultivate his 'territory' for qualitative advertising research, with other pioneer allies like Christian Alsted (Alsted 1989) and Henrik Dahl (Dahl and Buhl 1993). They all of course ended up as outlaws and exiles.

How can Buhl's study be characterized today, and with it the rest of the qualitative research in the pioneer days, on the basis of traditional scientific criteria?

In short: Buhl's study is characterized by *high validity* in the sense that open in-depth interviews in the respondents' own homes guarantee that the data acquired by the researcher express people's own life-worlds and provide authentic meaning in people's own words, rather than something foisted upon them by the researcher with the rigid categories of a questionnaire.

But it is obviously also characterized by *low representativeness*, because there are only 6 respondents, whose experience of the American Express ad may not be typical for the target group.

And finally the study is characterized by *low reliability* because only Claus Buhl himself has seen the data, and it cannot be ruled out that he has interpreted them in a subjective manner. We as readers of his study have not got access to the interview transcriptions, and he does not show us many, nor very long, extracts from them. We cannot know how reliable the analysis is.

I could have raised the same objections to my own study of *Dynasty* in the mid 1980s. I did interview more than 6 viewers, but then no more than two dozen, and the readers had to trust my subjective interpretation of the data. This does not mean that I no longer wish to stand by this research. The analytical insights created are still valuable, and they were necessary in the struggle against the then dominant Frankfurt school-inspired idea that the viewers were passive, naive and stupid.

It is of course regrettable that our lack of resources and experience with qualitative field work meant that the research had certain shortcomings. But the aim of the investigations was not to find out *how widespread* the manifold readings were, but precisely to find out that they *were manifold*, that they existed!

Now perhaps we have come so far that we can do something about the weaknesses? But let us first take a look at another and at a first glance more 'scientific' way to carry out reception research. At the same time as Buhl's investigation other advertising researchers were also interested in whether the message of the ads reached the recipients, but adopted a different kind of method, e.g. Camargo's investigation of the Marlboro ads (1987).

Reception of Advertisements: Quantitative Research

Camargo's starting point was a skeptical attitude towards using the same standardized campaign for advertising Marlboro all over the world – he doubted whether the message got through, and whether the recipients took the same message in the USA, Norway, Japan, Thailand and Brazil.

His investigation was inspired by the semiotic wave within marketing and consumer research in the 1980s (Mick 1986), but coming from a quantitative tradition, in which the only scientific results that count are those that can be put into figures, he investigates the meaning of the Marlboro ads statistically.

He wants to establish people's connotations in connection with the Marlboro ads and consequently makes up a sentence that is supposed to get people to think of Marlboro ads. It goes like this: "Smoking a Marlboro cigarette". It has never been quite clear to me why he does not just show the respondents a Marlboro ad or two, but maybe that has to do with the fact that 'meaning' is something very simple for many researchers with a social science background.

First he translates his stimulus sentence into the 5 languages. Then his research assistants in each country give out a sheet of paper with the sentence repeated 10 times to 30 university students in each country. They are asked to write as many connotations as possible within one minute. All connotations are translated into English.

Having read all the connotations, Camargo sets up 20 THEMES, which he believes cover the connotations. Two interpreters then distribute the connotations among the 20 themes, cross-checking with each other so that they decode the material in the same way. The reliability is estimated at 96%.

Next Camargo establishes 7 GENERAL THEMES on the basis of the 20 themes, distributing the connotations among the 7. Then he makes a statistical calculation of the frequency of the themes and the general themes in the different countries.

In the 'theme' analysis he finds out that there are big differences in the perception of the stimulus sentence from country to country: Many more in the USA than in the other 4 countries have 'cowboy' as a connotation. More Americans than Norwegians have 'horse' as a connotation. More Brasilians than Norwegians, Thailanders and Japanese have 'pollution' as a connotation. Etc.

The statistical analysis of the 'general themes' shows that the respondents in the USA have more connotations to an 'advertising campaign' than those in the other 4 countries, and that the Brazilians are more negative than the Japanese and the Thailanders. All this is statistically significant.

Camargo's conclusion is accordingly that "the Marlboro man is not conceived of in the same way in different countries" (Camargo 1987:480), and that consequently it is not a good idea to launch standardized advertisement campaigns all over the world.

How can we then characterize Camargo's investigation according to the traditional scientific criteria?

The *reliability*, i.e. the way the data collection and data analysis take place, must be said to be relatively *high*. The data collection is very systematic and has been undertaken the same way in each country; and the way the two decoders interpret the data material is characterized by a high degree of intersubjectivity. Thus as readers we have to believe in the investigation as far as reliability is concerned.

As far as *representativeness* is concerned the investigation is a bit more dubious. On the one hand there are relatively speaking quite *a lot of* respondents in each country – 24 more than in Buhl's investigation – although obviously they do not make up a representative, arbitrarily chosen selection from the population of the 5 countries. And apparently it has not been considered whether the respondents were smokers or not, which ought to be quite important. What happens to data comparability if all the Norwegians are smokers and none of the Brazilians are?

Camargo's investigation is in this respect typical of social science research, always involving a lot of respondents, because questionnaire data can easily be analyzed extensively by the use of computers. Quite often, however, students are used as respondents – Frey et al. (1991) note that in 1985 65% of all social scientific communication research was based on student respondents. But still, let us – for the sake of the argument – say that in general quantitative studies have a *high* degree of *representativeness*.

As far as *validity* is concerned, however, it is equally clear that it is often quite *low*: In Camargo's case the respondents do not even get to see the Marlboro ads that the study wishes to investigate, and it is quite unclear what the real connection is between the original connotations of the respondents and the themes established by Camargo.

And if we look beyond Camargo's study to the many questionnaire-based surveys so prevalent in social-science communication research, the low validity – to be brief – is due to the fact that prefabricated questionnaires force the respondents to relate to media material through categories that may deviate partially or completely from their lifeworld-based experiences of the material.

'Triangulation' – A Methodological Panacea?

We have now reached the classical opposition between quantitative and qualitative approaches, as it is described in standard works about research methods:

> Quantitative observations provide a high level of measurement precision and statistical power, while qualitative observations provide greater depth of information about how people perceive events in the context of the actual situations in which they occur.
> (Frey et al. 1991:99).

The two approaches thus have their strong and weak sides: The qualitative method is distinguished by high validity, the quantitative method by high reliability and representativeness.

And what can be done about that? Well, it is of course possible to accept this as a fact of life, and more or less suppress the weak sides of one's own method – which is done extensively in both camps. The weak points become matters not discussed in the open (Roe 1996).

Alternatively one can call on the mantra named *'triangulation'*, and many do these days. In 'triangulating' one simply uses one method after the other. In this way they are said to compensate for each other's weak sides, and everybody gets the wiser.

For example, and remaining within a qualitative framework, it is possible to investigate people's use of the media both through interviews and observation: People may tell the researcher that they never eat a main meal while watching television; one may then have a video observation revealing that they do this anyway. Here one method functions as a corrective to the other: data from one method turn out to be not true, but obviously not without interest for that reason! One may also exploit the triadic essence of 'tri-angulation' to the full by using 3 qualitative methods: interviews, diaries and observation (Lindlof 1996:239).

But the term 'triangulation' is also used, and in my view as extensively, about the combination of qualitative interviews and quantitative questionnaires. This procedure is one which I myself have had very positive experiences with in a successful investigation, *Når danskere ser TV* (When Danes are watching TV) (Jensen *et al.* 1994).

We undertook a genuine, small-scale reception study of how the TV medium was part of the everyday lives of 9 Danish households. Among many interesting points we found that households can be divided into 3 types, which we referred to as moralists, hedonists and pragmatists.

Moralists are people who believe that TV is a waste of time or that it can even be harmful, and consequently they plan everything they want to see ahead. *Hedonists* believe that TV is a harmless element in normal everyday life, and consequently they do not plan anything ahead, television is just 'on', possibly controlled by the "inner program schedule" of the household members. *Pragmatists* are people who do not believe that it is harmful to watch TV, but nevertheless plan their viewing ahead, often on the basis of a printed TV schedule, in order to make room for other activities in their lives.

The primary purpose of the qualitative study was to be theory-generating, and for that purpose the qualitative approach was fine. But then we also had the ambition that we wanted to say something about the Danes' use of TV more generally. Therefore we made a questionnaire that became part of a representative Gallup survey, and this led to the following findings about the distribution of three types in the general population: Moralists: 24%, Hedonists: 12%, Pragmatists: 57%.

Another more recent investigation from Holland/Belgium uses the triangulation method to study the diffusion and use of new information technologies, and consumers' thoughts and attitudes in relation to the new technologies (Frissen & Punie 1998).

In their qualitative investigation 7 so-called "busy" households in Amsterdam are interviewed to illuminate how they talk about and relate to new information technologies, especially in a time perspective: The researchers want to investigate whether the respondents see mobile phones as a solution to time and coordination problems in a busy everyday life? The study shows that new information technology in fact is often a solution to such problems, but at the same time not all users *experience* it that way.

In their quantitative investigation questionnaires are used to find out whether busy households have more information technologies than non-busy ones, and if those that are busy think they have more communication problems than those who are not busy. I will not enter further into a discussion of the results, because the methodological issue if the main one here.

Frissen & Punie find that the two approaches supplement each other very well. The quantitative investigation says something representative about the diffusion of the technologies which the qualitative investigation of 7 households was not able to show, whereas the qualitative investigation helps explain some of the oddities of the statistical results.

Anne Jerslev, who has used a quantitative analysis before a qualitative one in her study of young people's video communities, puts it this way: "The *quantitative* investigation of media usage can point to *something being interesting*, whereas the *qualitative* investigation can try to capture *in what way it is interesting*" (Jerslev 1999:25, my translation).

In investigations of this kind, it is thus possible through triangulation to throw light on **different facets** of the communicative phenomena being investigated, and that is all very well. But in the methodological argumentation for triangulation more than this is sometimes claimed. First of all it is said that the combination of qualitative and quantitative methods makes it possible to throw light on **the same object** from different positions.

> Triangulation is a term that in surveying refers to locating a point in terms of two other fixed positions.
> (Frey et al. 1991:14)

But is this really true for triangulation in communication research? Is it not rather that a scientific method plays a decisive role in **constituting its object** through the way in which the method defines this object? Thus behind the communicative phenomenon or the communicative technology being investigated the real research object in the qualitative interview is the informants' discourses about he use of TV or IT – *their* words, expressions and metaphors – whereas the object of the questionnaire investigation is the *researcher's* words and categories. Maybe we should adopt a different metaphor and view the different methods as different optic lenses, where it is to be *expected* that we see something *different* through them?

Secondly it is sometimes claimed that the two approaches can be used as a kind of mutual answer book to each other:

> Both types of observations can be used together profitably to achieve triangulation, which enhances both the precision of the data gathered (with quantitative observations) and the contextual influences on those data (with qualitative observations). Using both types of observation also provides a way of assessing the accuracy of the findings from one operational procedure by comparing it with a different operational procedure. If the findings support each other, both procedures are corroborated. If the findings are different, however, this does not necessarily mean that the data are questionable. The difference could be a result of the types of data that are acquired through quantitative and qualitative data.
> (Frey et al. 1991:99)

I have never quite understood this kind of logic! If the two methods give the same result, then that is fine. If they do not, then never mind, because after all they *are* very different types of data! That is an eating-your-cake-and-having-it kind of logic (for a similar criticism see Silverman 1993:156-8).

I have asked myself the question, where does the triangulation metaphor really come from? According to Silverman (1993:156) it is an expression from navigation, where different bearings will give the precise position of an object. According to my (Danish) encyclopedia it is a geodetic expression:

> **Triangulation:** geodetic method for measuring points, based on the measurement of angles with a theodolite. A connected network of triangles is established. One of the side longitudes is measured (basis). Then all angles are measured, and all the remaining sides can be measured trigonometrically.
>
> **Theodolite:** Instrument for measuring angles. Consists of a telescopic sight placed on a horizontal and a vertical axis. After focusing the binoculars, horizontal and vertical angles can be read on scales.
> (*Gyldendal's 2 vol. Encyclopedia 1982*, my translation)

One can imagine the reception researcher in the field, like the surveyor setting up his instruments, aiming at the geodetic sticks and 'measuring' in the real sense of the word, leading to a precise picture of the fields of meaning created by the respondents!

I cannot deny that I get the feeling that sometimes, when in the humanities and social sciences we invoke 'triangulation', it is a result of our inferiority complex in relation to the exact natural sciences. In this respect this metaphor reminds me of the metaphor used in ideological critique of the media in the 1970s, when we used to say that the analysis of textual content could be likened to an ideological 'seismograph', carefully registering the slightest movements in people's collective unconscious (Larsen 1974; Schrøder 1984).

Maybe we ought to discard the triangulation metaphor altogether, and instead let it suffice to say that it is an advantage to use several methods to form a *series of images* about how people use and experience the media.

Maybe we also need to give up the ambition of ever being able to say something final and general about the precise relation between the images of reality we create through the different methods. Instead we should merely say what it is reasonable to say in each specific investigation on the basis of the practical coordination of methods.

And maybe, instead of using smart metaphors about our often opaque methods of analysis, we ought to try to formalize and demonstrate how we actually work, e.g. when interpreting and finding patterns in our interview transcripts (Bergman 1998).

Beyond the Logic of Doing Fractions?

We often, rightly, say to each other that reception research is in itself a meeting ground for humanistic and social scientific media research, and that people in both camps can gather around reception research, provided that they are willing to sacrifice a few of their own sacred cows (Schrøder 1987; Jensen 1991). For reception research is humanistic in its theory and social scientific in its method: We do research in culture and meaning, through qualitative empirical field work. That is quite true.

However, we seem to have quietly accepted that qualitative and quantitative research processes have their strong and weak sides – and we then compensate for the weak ones through 'triangulation'! Yet in my opinion that is a curious way of doing research methodological fractions, with the respective strong and weak sides nullifying each other (Figure 1).

Metaphors are often useful for pinpointing something, and in this case we could also talk about a fishing rod logic, where each swears by his own fishing method and has practiced it to perfection – I use fly bait, you use spin bait – but once in a while I would not mind borrowing your fishing rod while you borrow mine. The point is that so long as it happens this way, there is no real cross-fertilization in the relationship between the two methods.

I therefore believe that the real challenge is to *synthesize the strong and weak sides* within one research design so as to create a reception analytical approach with no weak sides!

When, as Frissen and Punie (1998) state, we have one method that can tell us a little bit about a lot of people, and another that can tell us a lot about a few people, I want to continue their line of reasoning and say that we need to create a method that can tell us a lot about a lot of people!

This methodological synthesis within reception research must have high validity, high reliability and high representativeness, so that our research appears convincing when it is put forward in the political or public debate. Consequently it should meet 4 criteria:

1. In order to have validity it should be qualitative.
2. In order to have reliability it should be systematic and have analytical intersubjectivity built into its research design.
3. In order to be representative it should use many respondents.
4. In order to be compelling it should be quantitatively oriented.

At this point I would like to point out that I am not pleading for a kind of methodological and epistemological imperialism. I am not trying to establish

Figure 1.

$$\frac{\text{QUALITATIVE}}{\dfrac{\text{Validity}}{\text{Reliability Representativeness}}} \quad \text{combined with} \quad \frac{\text{QUANTITATIVE}}{\dfrac{\text{Reliability Representativeness}}{\text{Validity}}} = \text{TRUTH}$$

a prescriptive solution for all reception research. I am only pleading for a pragmatic pluralism which makes possible precisely the approaches to research that are suited for different knowledge interests:

- Large quantitative questionnaire studies when the task requires it.
- Small qualitative interview studies when the task requires it.
- Triangulated studies when the task requires it.
- Large qualitative interview studies when the task requires it.

Having moved beyond the pioneer days, it is clear that we have now reached a point where we can make large-scale, qualitative reception studies.

Methodological Synthesis in Reception Research

Why does the study of audience meanings have to be qualitative? The answer is 'VALIDITY': When the research object is cultural meanings, a qualitative approach is required if our findings are to be a valid expression of our informants' life-worlds.

This is what *I* believe, and this is also what has been suggested by prominent researchers within the quantitative tradition (Gantz (1996, Roe 1996). Having carried out a major critical evaluation of the quantitatively oriented 'uses and gratifications' research through the last 25 years, Gantz concludes that

> the research agenda is likely to require alternative, if not innovative, methods of data collection. Interviews will need to be interactive, a mix of open- and close-ended questions. Probes will be critical; interviewers will need to ask respondents what they (the respondents) mean when they say, for example, that they turn to television to be entertained or they watch out of habit. (...) gratifications scholars will need to be creative and, as needed, supplement survey research (...) with depth interviews, where respondents are given ample opportunity to reflect and describe the nature of their relationship with media content.
> (Gantz 1996:26-27)

What Gantz is in effect saying here is that research that explores people's media gratifications must become more qualitative if it is to have any validity.

Secondly in the research synthesis that I propose here it will be necessary to take RELIABILITY more seriously than reception researchers have done so far, in order to eliminate arbitrariness of data collection and interpretation (Silverman 1993:145). We especially have to become better at describing and demonstrating our analytical process. The Dutch media researcher Simone Bergman criticizes reception researchers for having a magician's attitude to our audience: We spend a lot of time preparing the trick, showing props, etc., but our way of showing our results corresponds to pulling a rabbit out of our hats – nobody is told how we have come to the result, except that we have used 'thematic analysis', 'discourse analysis' or 'pragmatic analysis', or whatever we choose to call it.

To take the issue of reliability seriously we must continue to show ample extracts from the interview transcripts, so that the readers can see how we reach our specific interpretations. But most importantly we need to double-check our interpretation of the entire body of data, i.e. we need to systematically use research assistants or colleagues to reanalyze the interview material, so as to discover too imaginative analyses and to clarify obscure points. A few reception researchers have used this procedure, e.g. Mick and Buhl (1992), who had a colleague attached as analytical auditor in a project on advertisement reception.

Thirdly, REPRESENTATIVENESS must be taken seriously if the purpose is to say something about a big part of the population, e.g. the whole of Denmark or young people in Denmark. But I am pragmatic enough and sufficiently skeptical towards the statistical representativeness concept to not really want to promote representativeness in its well-known form, but rather to argue for the benefit of having *lots of relevant respondents*, even if they do not exactly match the composition of the population. Precisely how many are needed must be decided with a view to the task in question. But always: As many as possible.

In the history of reception research there are only a few examples of investigations with a lot of respondents, e.g. Lewis' studies of news reception and TV comedy reception (Lewis 1991), each of which has interviews with about 50 groups of 4 or 5 respondents. Radway (1984) had about 40 respondents. However, there are also examples of studies with very few respondents. But as stated earlier even a handful of respondents – as e.g. Buhl's study in the example above – had a clear purpose in the history of reception research.

And thus I have reached the fourth point of my recipe: that the investigations have to be QUANTITATIVELY ORIENTED. Before anyone else thinks or says this, I might as well say myself that this point is due to the fact that deep down I am a secret positivist…

My considerations concerning the desired quantitative orientation of reception research have to do with the presentation of research findings to a wider audience beyond the research community. Many years ago, in the heyday of ideological critique, Preben Sepstrup, in connection with his quantitative investigation of sex roles in advertising, argued along similar lines for the necessity of quantifying the insights of the critical textual analysis, if one was to get through with one's critical points to a wider public (Sepstrup 1979).

Such a public presentation is for instance required when a research project has been commissioned and funded by some organization or institution, as in the case with my own investigation for the Government's Commission on the Media (Schrøder 1995) or Jørgen Poulsen's large reader study for the Danish Press Association (Poulsen 1998).

The presentation of research results to a wider public can also be due to the fact that the findings of the investigation are quite simply in the public interest, which is always the case with reception studies of children, young people and the media (Drotner (1999), Jerslev (1999), Sørensen (*Medievold* 1995), Tufte (1999), Povlsen (1999).

In the pioneer days many of us wanted to tell others if many of our respondents experienced a TV series or the news in a certain, perhaps unexpected way. But we thought it sounded a little stupid to say that 5 out of 8 women in all among 16 respondents experienced the series in this or that way, so instead we used pseudo quantitative (Rosengren 1996) specifications such as "many female viewers thought that, etc." or that examples of this or that were "often" seen in the data (Schrøder 1989:15).

When I myself have read phrases like that in other people's research I have always speculated as to just *how many* and just *how often* – but that is probably this point again about my secret positivism!

Today there is quite an influential branch of reception research, called radical media ethnography, often oriented towards feminism, which dissociate themselves from not only quantification, but from any kind of *generalization* of data, because generalization makes a mockery of the complex reality people live in (Radway 1988; Ang & Hermes 1991; Silverstone 1994). These researchers believe it is the researcher's job to let the informants make themselves heard as directly as possible, and that generalizations are the researcher's untimely interference with and reduction of the reality of the informants.

It is clearly a theoretically legitimate point of view that one should not generalize, but in my opinion it is quite unreasonable if the aim is to enter into a dialogue with e.g. decision makers or for that matter ordinary people about how the media function today and will function in the future. In that situation one has to search for patterns, types and tendencies in one's qualitative data, and not only say e.g. that the 16 informants' experiences of the ads shown to them were very different indeed.

One example of how one can generalize in a quantitatively oriented way is provided by my qualitative, depth-interview based study of 16 English informants' experiences of 'ethical corporate advertisement' (Schrøder 1997). Their experiences of the individual advertisements could be placed in 3 categories: Sympathetic, Agnostic (skeptical) and Cynical.

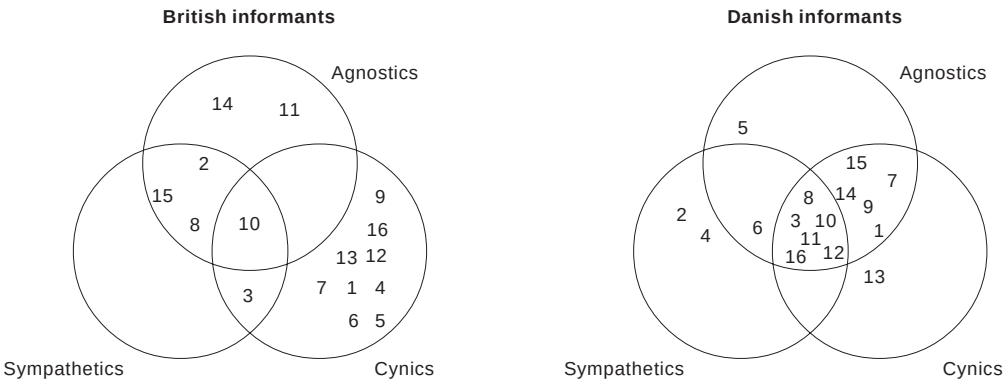
However, in the material it is quite clear that the individual informant could be sympathetic towards some ads, skeptical towards others, and maybe cynical towards others still. For that reason I felt that it would be unreasonable to categorize the informants in these three groups – that *would* be making a mockery of the complex reality!

But by considering the 3 categories of experience as ideal types and drawing them as overlapping circles (a so-called Venn diagram), I could place the 16 informants in such a way that I both drew up a pattern and still maintained some of the complexity in their experiences of the ads. The circular diagram allows the informants to appear either in the 3 'clean' categories or in 3 double categories, or even in the triple category.

Of course many analytical details are lost in a diagram like this, and these have to be made up for in the accompanying text. If this is done successfully, it might perhaps be said that one has succeeded in making *a qualitatively based generalization* about people's experience of the advertisements in question.

In this way the distance between qualitative and quantitative approaches has been narrowed considerably. In fact the reader can count how many informants there are in each category, while at the same time, through the accompanying text (includ-

Figure 2.



ing excerpts from the interviews), drawing a line back to the qualitative complexity behind each category.

Thus I agree with Alasuutari when he says that ”there is a lot of good in the quantitative approach as long as it s not used in the standard fashion” (Alasuutari 1995:3). Maybe one can say that by reporting one’s findings in this way one gets close to preserving the qualitative dimension in qualitative data that have been through a quantification process (Schrøder 1987: 27)?

Some of you may be thinking that when I draw up such geometrical diagrams, I am no better than those who speak about triangulation – that I am also subject to humanistic feelings of inferiority! To this I can only answer that I am willing to run that risk if it can get us further in the discussion about the *mutual* fertilization between quantitative and qualitative approaches.

It may then be asked whether there is any chance that the methodological manifesto I have put forward here has any chance of becoming implemented? The answer is that this is already happening, even though to my knowledge there are as yet no reception analyses that contain all the elements one hundred per cent.

For instance I can point to my own study *Danskerne og medierne. Dagligdag og demokrati* (The Danes and the media. Everyday life and democracy) (Schrøder 1995; a short English summary is included in Schrøder 1999), which lives up to the requirements of validity, reliability and quantitative orientation – but only takes a step in the direction of fulfilling the representativeness demand (’only’ 30 household take part in the investigation).

Or I can point to Jørgen Poulsen’s large scale investigation of the Danish newspapers’ so-called ’maybe readers’ (Poulsen 1998), which both triangulates by applying together no less than 5 different qualitative and quantitative approaches, and which approaches a high standard of validity, reliability, and number of respondents (N=82) in its principal (qualitative) study, and which is generally exemplary in explicitly addressing methodological problem areas.

Or I can mention Gitte Stald’s large scale investigation of Danish children’s use of the computer medium (Stald 1998), which I think holds the Danish record for respondents in a qualitative reception study with over 100 respondents (N=103). She uses several qualitative research methods (observation, group interviews, individual interviews, drawings) and establishes 3 user types: super users, users, and non-users. The weakness about her study is especially the low reliability of the interview analysis, including some not very transparent definitions of the 3 user types. In light of this shortcoming it is maybe just as well that she does not inform the reader about how many respondents there are in each category. But for any quantitatively oriented person this information would be interesting to have, and necessary if in five or ten years one wanted to carry out a similar analysis and compare the results.

Where is Reception Research Going?

Today reception research is facing many exciting challenges. What does the growth of the new media mean- are the existing methods of reception re-

search sufficient, or do the more interactive and to a certain extent dialogical media demand that the tool box be expanded with new tools (Mayer 1998; Roscoe 1999)?

And what about reception research oriented towards cognitive psychology, whose practitioners believe they can add a new dimension to reception research (Höijer 1998; Höijer & Werner 1998)?

And what about the globalization of the media – what does that mean for reception research? Do we need more transnational, comparative studies to look into differences and similarities in the dissemination and use of media in the age of globalization (Jensen 1998; Livingstone 1998)?

And some researchers have begun to practice experimental reception research – e.g. Bruno Ingemann's study of how Danish newspaper readers experience more or less manipulated press photos (Ingemann 1998; Gjedde & Ingemann 1999).

When I started planning this lecture I imagined that I would try to throw a critical glance at all the exciting things that are currently happening in reception research. I had also imagined that I would touch upon my own recent work to set up a multidimensional analysis of reception data (Schrøder 2000), and upon the growing consciousness about how the insights provided by the research in interpersonal communication can be made useful for analyzing the linguistic interaction taking place both in individual and group interview settings (Lunt & Livingstone 1996; Myers 1998).

I quickly realized, however, that I would have far too little time for that. For that reason I have concentrated on going into depth with the methodological challenges that interest me the most at the moment (for a more detailed argument, see Schrøder 1999).

Returning to a couple of the metaphors used above about the historical development of reception research, one can perhaps say that the crossroads which reception research is facing is in no way a small country crossroads, rather it is reminiscent of the many roads one can take from the *Place d'Etoile* in Paris. And the interest of reception research is best served if the individual reception researcher takes exactly the road that he or she thinks looks most exciting.

I therefore conclude that for reception research the pioneer days are over. But a great deal of *pioneer spirit* is still required if we are to develop reception research as a plural, enlightening, result-oriented and critical science.

Note

1. The article is a slightly revised English version of Kim Schrøder's inaugural lecture as professor of communication, Department of Communication, Journalism and Computer Science, Roskilde University, Denmark, delivered 25 October 1999.

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