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□ Schrøder, Kim, Kirsten Drotner, Stephen Kline & Catherine Murray:

Researching Audiences

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A Review by Martin Barker

This is an important new book. Schrøder et al. (I hope the other authors will forgive this abbreviation for the purposes of this review) offer us an enormously knowledgeable, highly detailed and well-exemplified study of the several traditions of research that have historically helped form audience studies. They pay in particular detailed attention to the *practices* of the research, to the methods of data-gathering or materials-gathering, and to the various procedures of analysis which have been developed. For this attention, it is as far as I know the first of its kind. It is also, I hope to show, in some important respects rather interestingly problematic.

Divided into four main sections, their book organises the audience research field into four main traditions. These are, first, an ethnographic tradition, where the emphasis is on the imbrication of media use into the whole social fabric – all our lives being permeated by systems and structures of meaning-making, research must necessarily examine the way our activities as audiences are interwoven with the rest of social and cultural life. Second, there is what they call a 'reception studies' tradition, in which the focus of attention is on the acts of interpretation or 'decoding' that audiences undertake when faced with particular texts or genres of texts. The third tradition they distinguish is a survey tradition, whose fundamental aim is to map the spread, frequency, and location of particular kinds of audience. And finally they tackle an 'experimental' tradition – which they associate strongly with the American 'effects' tradition. The first two, then, are essentially qualitative, the second two are quantitative. This is an interesting way to organise the field, and all ways are to some extent arbitrary and open to question. Here, it is worth noting that the choice of 'reception studies' introduces a small clash with the recent tendency to use that title for the European/American tradition which originated in reader-reception theory, and then transported itself into film studies via people like Janet Staiger and Barbara Klinger – where the emphasis has increasingly been on the study of publicly circulated framing discourses. (I wonder if some day *Participations* is going to have to sponsor a short conference to try to clarify our naming tendencies. It is a clear mark of the state of our field that there are such naming difficulties – we simply have not talked with each other enough.) The other clash resulting from these 'namings' is that there is no real place for the work of people such as Philip Schlesinger and David Morrison, who have recently been devising experimental conditions for exploring how people interpret and use the idea of 'violence'. It is a matter worth thought whether this is just an accidental by-product of an inevitable need to somehow organise the book, or whether the absence of the latter in particular may reveal something important about the book.

The book opens quite provocatively, with a reframing of the main traditions, and with a challenge. Drawing on James Carey and Brenda Dervin, they offer an overall framing of audience research as a running debate between two broad conceptualisations of audiences' relations to the media: 'transmission' (referencing a linear model of communication) and 'dialogue' (referencing a transactional model). But while Dervin in the end wants to negate the transmission model, Schrøder et al. 'preach methodological pluralism': 'This book is based on the belief that people who are going to work as professional communicators or communication researchers would not benefit from drawing this conclusion. We take the view that the communication process should be conceptualized through the optics of different models, and that within such an eclectic perspective there is a role to be played by both the transmission and the dialogic model.' (p. 15) I will argue later that while at some levels this openness is welcome, it contains unacknowledged difficulties which we would do well at least to bring into the open.

Perhaps a start can be made on seeing why it is an issue by noting that in the second chapter the authors move on to question a second – and as they see it related (p. 17) – opposition between the paradigms of quantitative and qualitative research. I must say that I found their discussion of this opposition very well framed and helpful. Their conclusion, both here and in their summation at the end of the book, is that the opposition between the two paradigms is unnecessary and unhelpful. I believe that they are right in this – but that this is more distinct from the transmission/dialogic distinction than they allow.

Schrøder et al. then distinguish their four main routes through which audience research has travelled. They use the work of Thomas Tufte (a nicely-chosen example) to illustrate ethnographic research. It thereby becomes clear that they are using the term 'ethnography' to mean almost what others might mean by 'participant observation' – although with an added emphasis on the use of multiple means of investigation and recording. I have some sympathy, I have to say, with this quiet ducking of some of the debates around the meaning of 'ethnography', which can convey an air of possessiveness (don't use the term unless you are prepared to live up to my criteria for it). But it does mean that they are not tempted to call the work of people like David Morley or even Janice Radway 'ethnographic'. This, instead, they class as 'reception research', by which they say they mean the investigation of the meanings of media to audiences through an analysis of their talk.

Their estimation of the strengths and limits of ethnographic research is a wise one. They note (p. 84) the way its very multidimensional approach presses researchers to move away from examining individual media, instead examining a whole ecology in which audiences are enveloped. But in the opposite direction the increasing tendency of audience communities to be physically dispersed (communicating on-line, for instance) makes traditional ethnographic participation very much more complicated, perhaps impossible.

One of the strengths of this book is its willingness to try, wherever possible, to step beyond the boundaries of academic research (although, as they themselves say at several points, one of the problems in doing so is how secretive most commercial researchers are). And they also note that reception research, despite its primarily academic – and supposedly very political – origins, is now finding a toe-hold in various kinds of commercial focus group enquiries. (They do not mention, but I will here, David Morrison's vast critique of the tradition of focus group research and, as he sees it, its uncritical adoption by cultural studies. I am not in any way implying that they have intentionally ignored it. But it is relevant to my broader argument in as much as Morrison argues that from its inception the focus group method was infected with a political agenda which has quietly been forgotten in cultural studies' subsequent enthusiasm for it[1]). This existence of reception research on the boundary between academic and commercial worlds is nicely captured in the research example with which they open their account of this tradition: Schrøder 's own study of the ways audiences responded to corporate responsibility advertising. I found extremely helpful the detail which they were able to include about how this and other exemplary pieces of research were designed, carried out, and the resultant materials analysed. Many research publications lose this sense of process, not least because of constraints of space – a problem, of course, that in principle an on-line Journal like *Participations* need not suffer from.

At the close of their discussion of the reception tradition, as they understand the expression, they identify three broad strengths accruing to its now-consolidated position. These are: its willingness to deal with the 'wholeness' of audience responses, seeing these in the context of the rest of people’s lives; the rise of comparative studies; and the increasing attention being given to interactive audiences (and given they open and close with a discussion of *Big Brother* all three of these are of course very germane).

After these two excursions into different traditions of qualitative research into audiences, Schrøder et al. turn to the quantitative traditions, here distinguishing a survey and an experimental tradition. Again I found their choice and exploration of a chosen example to introduce the survey field both interesting and helpful. They use Tony Bennett et al.’s study of taste cultures in Australia, and they take time to explore its relations with Bourdieu’s original study on which it is based, but from which it importantly varies. They also once again consider, interestingly, what policy-implications might flow from the research.

Here too, there is a kind of 'politics' in the way traditions are grouped, for here, in the first, survey tradition they include the Uses and Gratifications approach. They argue that its merits have been downplayed and its problems overstated (calling its rejection a ‘fashionable’ cultural studies response). Their case is interesting, but it is worth remembering that the first – and perhaps still most powerful – critique of this approach came not from cultural studies, but from within sociology, from Philip Elliott.

It cannot go unremarked that their fourth section, on the experimental tradition, feels very different from the rest of the book – first of all just in its choice of opening example. Unlike the others which use near-contemporary examples, this one reverts to Albert Bandura's Bobo-doll experiments – and indeed effectively makes 'audience experiment' synonymous with 'measuring effects', indeed 'measuring the effects of violence'. Space is given to considering criticisms of Bandura's research, but the only ones really considered are those that have to do with the design and management of the experiment. There is no apparent interest in the debates over either the fundamental conceptualisation of media/audience relations (perhaps because of that opening gambit to try to rescue the transmission model), or of the politics of this whole research tradition (I am thinking here in particular of the work of Willard Rowland and Timothy Glander[2]).

So what are the problems I perceive in this book? Some are already signalled by my comments on that fourth section. While its strengths are its range, its clarity, and its determined call for an expansion of audience research using a wide range of tools, there is to my mind a studied avoidance of the very political debates which have been so important in the field. I found this to be the case in two places in particular. The first occasion was something of a red rag to this particular bull. In their introduction to reception research, Schrøder et al. comment on the frequent recourse to Stuart Hall's encoding/decoding model – well, actually, more than that. They write: 'We challenge our readers to find a single work from the empirical reception tradition that does not make a reference to Stuart Hall's article'. (p. 127) I have to report that I took up that challenge, and wrote to Kim Schrøder referring him to the book I co-wrote with Jane Arthurs and Ramaswami Harindranath[3], where no such reference is made – because the methodology we were using was *at odds* in important respects with Hall's model. The same was true with my earlier study of the audiences for the movie *Judge Dredd* – although I had to concede that we had *mentioned* Hall's essay, but precisely to argue how unhelpful it was. I don't intend this to be an issue about who knows about what research (our world is far too complicated for every author to know of everyone else's research). It is because their account of reception research *effectively equates* it with an encoding/decoding approach. And although they do (as Kim pointed out in his reply to me) themselves propose ways of superceding Hall, this is only at the methodological level. In other words, the kinds of *theoretico- political* criticisms of the Hall model which were made in the *Judge Dredd* book are entirely missing from Schrøder et al..[4]

This problem was even more strikingly present in the book's discussion of the 'effects tradition'. Betraying, as I've suggested, a different tone of voice from some other parts, this section mounts a virtual defence of this tradition of work. I certainly don't want to argue that there is something wrong with mounting defences – but to do so in this context has costs. In the earlier parts of the book it is clear that the supervening picture of audiences is as socially-located, historically-grounded members of particular (and probably multiple) communities. It is this which makes ethnographic research so valuable. And the kind of discourse analytic approaches which have fuelled reception

research have also, in their turn, given emphasis to capturing the social moments within audience responses (shared discursive resources for instance, and displayed membership of interpretive communities). It is to a lesser extent true that this audience-focused study acknowledges also that the media for which there are audiences also have complex semiotic structures and belong within historical traditions of meaning. But in the discussion of the effects tradition, and in particular in the defence of the tradition of 'violence' research, that sense of the locatedness of audiences just disappears. Here is one illustration of this (p. 328), where the authors[5] are imagining the design of a research project: '[L]et us imagine that we want to study the relationship between children's watching violent TV programmes and aggression. … Our ambition is to determine whether watching violent television (independent variable = content of television programmes) increases the likelihood of aggressive behaviour (dependent variable = subsequent aggressive behaviour).' The direction of this entire language is *away* from any sense of the audience – including children – as belonging to societies and cultures, towards an individuating psychology of childhood. And at the same time, the 'television content' becomes blurry, loses all sense of generic and other categorical forms of presentational form – instead becoming 'more or less violent' (with attendant content analytic methods of measuring these, though this remains effectively unstated in the book).

I don't think the authors can duck the problems here. But they have tried to do so by their virtuous-sounding appeal to methodological pluralism, and the need to reconnect quantitative and qualitative traditions. It may of course be one for which the cultural studies tradition bears some responsibility. Critics like Ien Ang have come very close at times to asserting that quantification equals objectification – a claim that resonates back to older complaints from sections of the Frankfurt School critique. In the face of that, and the resolute refusal of most cultural studies researchers to use even primitive forms of quantitative research, the door has been opened to the reverse move. I would make an argument that we need to make a clearer distinction between the *techniques* of investigation – which may, by question, approach, and available resources be either, or both, quantitative and/or qualitative, and the *conceptualisation* of the audience, and the media. But this book has much less to say about the latter. It is as if research could now take place without much framework of thinking about the general nature of the media/audience interface, or of the contexts within which 'audiencing' is framed. That, to my eye, is a serious limitation.

I have one final problem with the book but it is a problem for the future. At the end of the book they return to the relations between quantitative and qualitative research, and argue for mutual respect between them. But in doing so, they curiously fall back towards the model implicit in so many current debates – that qualitative research, with its rich situationalism, is good for giving depth, but bad at representativeness – therefore can do little more in the end than propose models which quantitative research will be able perhaps to test; while quantitative research may have problems with depth, but at least we know its rules for validity and reliability. I am beginning to doubt the truth of this whole

case. I would now want to argue that there may be ways in which qualitative research, differently conceptualised and on some specific questions, may be able to give us *checkable*, *generalisable* explanatory models of media/audience relations. To get to this point, we may have to give up certain still common assumptions – the most crucial of which is that *from the point of view of research, all audience members are equal*. There is a much bigger argument to be had here, but I hope at the very least to signal an invitation to engage in it.

For all my criticisms, this remains an exceptionally valuable book. Where is it pitched? It is a little hard to say. Some parts of it undoubtedly would work well with final year undergraduates. Each of the four sections has a pretty clear section introducing a 'basic methodological toolbox'. But in their final Section 7 the authors appraise future directions, and introduce their own favoured direction: a merging of quantitative and qualitative methods through the use of Q-Methodology, which is a several-stage approach permitting researchers to construct meaning-profiles from preliminary research, then measure responses to that quantitatively, so hopefully overcoming the problem of category-formation that has bedevilled content analysis, for instance. This approach, to my eye, is not unlike an elaborated version of the Osgoods' semantic differential test (and not necessarily any the worse for that). There may be great strengths to it. But I honestly doubt that anyone below a doctoral student could gain very much from this very dense discussion. The rest of the book is, as I have said, informative, clearly written, enormously knowledgeable (even as I complain about the gaps) and – that 'violence' problem aside – very persuasive.

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[1] David Morrison, *The Search for a Method: Focus Groups and the Development of Mass Communication Research*, Luton: University of Luton Press 1998.

[2] Willard Rowland, *The Politics of TV Violence*, NY: Sage 1983; Timothy Glander, *Origins of Mass Communications Research During the American Cold War: Educational Effects and Contemporary Implications*, NY: Lawrence Erlbaum 1999.

[3] *The Crash Controversy: Film Censorship and Audience Reception*, London: Wallflower Press 2001.

[4] In other small ways the book displays the problem which this Journal will hopefully help to overcome. On p. 353 they write on the problem that discourse analytic procedures can be less than transparent, and can make it difficult for anyone other than the analyst to validate the conclusions. Celebrating the fact that one analyst, Tamar Liebes, took steps to make it possible for her readers to see *how* she had conducted her analysis, they write: 'Liebes (1984) showed the way (never taken up by later researchers) by publishing a two-column analysis of one *Dallas* interview …'. Actually she is not alone in this, but once again incomplete circulation of knowledge of published researches is thwarting proper evaluation.

[5] Of course one of the authors, Stephen Kline, has recently mounted a major attack on the St. Louis Amici Court Brief, which amounted to a critique of the effects tradition, but was used to support an appeal against a lower court verdict denying young people access to a range of video games, a Brief to which I and a number of others contributed). For Brief and critique, see the following web sites: (Folks, I can either hunt out the original sites, or we could post the two pieces within the Participations website – what do you think is best?)

http://www.fepproject.org/courtbriefs/stlouis.html