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Making a genre relevant to society : popularization of science research articles in news magazines.

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MAKING A GENRE RELEVANT TO SOCIETY: POPULARIZATION OF SCIENCE RESEARCH ARTICLES IN NEWS MAGAZINES

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THESIS

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SUMMARY

The aim of this work is to investigate the popularization of science by looking at the differences in the discourse of research articles (RA's) in the field of cancer research and popularizations in news magazines. A corpus of twenty articles dealing with news in cancer research in 1994 was taken from *Newsweek*, *Time* and *US News & World Report*.

Firstly, a genre analysis of the popularizations is carried out to produce a new genre analysis model of popular bioscience texts, and new definitions of move and genre are put forward. Following this, a new model of popularization, the narrative of society, is proposed. This suggests that the genre of the popular science article is a completely different type from the RA, since it includes two different types of moves, scientific and social. The differences between the popularization and RA genres are demonstrated further by a detailed comparison of sample popularizations with the source RA's.

The model is then contrasted with the standard model of the popularization of science, the diffusion model, and various deficiencies of the current model are highlighted. The current model is shown to be not only incompatible with the model put forward here but also to be dependent on a code model of communication which fails to reflect the way popularization is achieved. A non-coding, more cognitive model of communication, Sperber and Wilson's relevance theory (1986/95), is then discussed. This theory is then used to further support the narrative of society model proposed and various difficulties with the use of relevance theory to analyse genres are discussed.

The thesis thus a) proposes a new genre model of popular bioscience texts, b) puts forward a substantive new model of science popularization, and c) demonstrates that different approaches to discourse analysis can be combined to yield a richer theory.

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CHAPTER 1

INTRODUCTION

This work is concerned with an area which is of interest to a number of different types of people from different disciplines: linguists, scientists, sociologists, educators and journalists among others. The field investigated is the popularization of science and we shall examine how it can be characterised from a number of angles. Each discipline has its own concerns and interprets the popularization in terms of these concerns, but, whatever the way of looking at the problem, popularization of science involves communication and thus the use of language. Thus we shall examine how a view of the popularization of science as text-based communication can throw new light on a field which has attracted a disparate variety of viewpoints.

The reason for this multiplicity of approaches to the study of science communication is the very place that science holds in contemporary society. Almost every time we open our newspapers or turn on the television it seems to be the case that a new scientific discovery has been made. Certainly, in the last year or so, at least two major science stories have made the front pages of the newspapers both in the UK, where the stories originated, and around the world: the BSE scare (Jasanoff, 1997) and the case of the cloning of Dolly the sheep (Wilmut et al., 1997). Though these two stories are exceptional in the amount of publicity they generated, they illustrate two important points which we shall be concerned with in this work. The first is that science impacts on the wider society, and secondly that popularization of science is concerned with just that impact. That these two stories generated the amount of attention they did is precisely due to the fact that the stories had an effect on society as a whole and not just the scientific community. Thus the BSE outbreak, for example, caused serious problems in the UK's relationship with the rest of the EU, while the cloning of Dolly brought calls in Congress for a ban on cloning research because of its potential implications for humans (Williams, 1997). This work is concerned with an explanation of the popularization of science in terms of that impact on society as a whole.

Another way in which the importance of science and its place in society can be indirectly estimated is in the ongoing arguments between the proponents and opponents of science. That science is currently under threat from attacks by a host of misguided enemies is a perception common among many scientists concerned with the position of contemporary science. Thus, on resigning from his post at the Brookhaven National Laboratory in New York last year, the Director, Nick Samios, declared: "There's an anti-science mood in the world today... You see it all over; there is a distrust of science" (McIlwain, 1997, p. 208). His solution relates directly to our theme, as he called for scientists to communicate better the importance and value of their work. Popularization of science, then, is seen by some scientists as necessary because their work is misunderstood by the wider society.

The kind of scepticism about science that scientists such as Samios are afraid of is not so much the recurrent belief in such things as astrology evidenced by the columns in the tabloids, but attacks such as that by Appleyard (1993). In his introduction to the paperback edition of the book, Appleyard details the vitriolic reception the book received from scientists who reacted to his attack on science as having no soul. Books such as this one are considered by many scientists to be part of an anti-science movement which includes so-called 'Creation Science' (Futuyma, 1995), but which is principally made up of an array of thinkers in the humanities and social sciences who contend that the scientific description of the natural world is not in any way special. Thus Signorile (1989, p. 69) claims that "(t)here is a conceit among apologists for the scientific faith that the body of work and thought we call science has a privileged status."

Scientists have fought back against such ideas with defences of their own, either justifications for the study of science, like those by Medawar (1985), Wolpert (1992) or Dunbar (1995), or polemics such as the fierce attack on critics of science by Gross & Levitt (1994). But the most famous recent quarrel is, of course, that set off by Alan Sokal's hoax article in *Social Text* (Sokal, 1996a, 1996b). Sokal's hoax and the resultant quarrel came to be known as the Science Wars ("Science Wars", 1997) and was to epitomise the differences between natural scientists and their social sciences

and cultural studies opponents. Some of the details of the quarrel will be analysed further below, but, at this stage it suffices to indicate that the position of science in contemporary society is not a secure one. At least as far as many scientists are concerned, it is perceived to be under threat. Considered in a more positive light, it can be seen that the position of science in society is under discussion and thus how science is presented to the public is not just a specialist question of interest to science educators and journalists but is relevant to the wider question of the importance of science as a description of the natural world.

That scientists need to communicate better to guard against such attacks on science is one reason given for the necessity for better popularization of science. Some scientists, e.g. Khoury (1997), argue further in terms of scientific knowledge being necessary for citizens in a democratic society. Another reason given is that science is paid for by the public and therefore scientists have a duty to communicate the results of their work to the public (Durant et al., 1989). Scientists may even look on popularization of their work as a form of advertising, as, although popularizations do not count towards the publication record that gets tenure or promotion, they may help attract research grants (Dunwoody, 1986; "Nice picture", 1997).

That scientists' motives for popularizing their work may be not simply to do with a disinterested desire to spread knowledge has also been suggested by a number of other commentators. The political position of science is stressed by Aronowitz and Hilgartner, for example. The former claims that scientists set up their work as privileged by their discourse, special rules of investigation and so on (Aronowitz, 1988). For his part, Hilgartner goes on to claim that scientists also use popularization as a way of defending their right to say what constitutes real science (Hilgartner, 1990).

Thus popularization is intimately bound up with the status of science in our society and thus an investigation of how popularization takes place is integral to defining that position. Many scientists are clearly quite aware of the importance of popularization and some of the greatest of them, including Einstein himself, have written

popularizations (cf. Carey, 1995). More recently, Stephen Hawking's book *A brief history of time* (Hawking, 1988) was at the top of the bestseller lists for weeks. Closer to the area that this thesis will concern itself with, popular writers in genetics and biology who are also professional scientists include such well-known names as Richard Dawkins (e.g. 1986, 1995), Stephen Jones (1994, 1996) and Stephen Jay Gould (e.g. 1991, 1993).

Many scientists, therefore, do concern themselves with writing for a wider audience than their scientific peers and look upon the communication of science to the general public as an important task for professional scientists. The fact remains, however, that, despite the success of such writers, they are a small exception to the general rule that most scientists do not write for the general public. In addition, the readers of such books are atypical of the general population in that they are part of what Miller calls the "attentive public", the 20% of the public who have a high level of interest in science and sufficient background knowledge in the field (Miller, 1986). Investigation of this type of science popularization written by scientists and looking at broader aspects of science is valid and interesting but it is concerned with a relatively small segment of the population.

This work will look at a type of popularization which is likely to reflect more accurately the place of science in the wider society, that of news texts in mass market news magazines, written by science journalists rather than professional scientists like Dawkins. Studies have been carried out before which have looked at popularization in magazines, most notably by Myers (1990, 1994). But Myers considered magazines like *New Scientist* and *Scientific American*, rather than the general news magazines looked at in this work. Similarly, as will also be discussed below, Nwogu (1991) examined a small corpus which included some examples of texts from *Newsweek*, but these were mixed up with texts from other sources, *The Times* and *New Scientist*. Newspaper coverage of science has been investigated by Evans et al. (1990), and Long (1995) among others, while more general accounts of media coverage of science include Evans and Hornig Priest (1995). However, examination specifically of science popularizations in news magazines as a type has not been attempted,

especially not from a genre or discourse rather than sociological perspective. This genre is an important part of such magazines as such stories run weekly in special sections devoted to medical and scientific news. In addition, *Time*, for example, brought out a special issue in January, 1998, celebrating recent scientific advances, called *The new age of discovery* (Geary, 1998).

The field: cancer research

The field to be discussed in this work, that of cancer research, was chosen for a number of reasons. First of all, the decision to look at the reporting of medical research rather than some other area of science was based on the fact that this is the most common type of science reporting in the press, if we consider all the natural sciences but exclude the social sciences (Evans et al., 1990). In addition, medical science is considered by the general public as the paradigmatic type of science, at least in the UK (Durant et al., 1992). So, although this might seem like a specialist subtype of science reporting it is in fact the dominant and typical genre. As will be seen, this is quite in line with the general predictions of the model set up in this work, as the general public's interest in health and medical matters (Durant et al., 1989) is likely to be reflected in the choice of medical, as opposed to other scientific, topics.

Initially, the topic of AIDS research was considered as a suitable specific area to look at. However, after the initial flurry of reporting on AIDS in the eighties and the early nineties AIDS research did not progress very fast until the advent of protease inhibitors and multidrug treatment in 1995-96 (Balter, 1996; "Battle fatigue", 1994). Moreover, AIDS, because of its association with risk groups and the resulting stigma, together with the fact that to date a cure has not yet been found, holds a rather atypical place in the public imagination (Sontag, 1988) and is thus unsuitable as a model of biomedical reporting.

Cancer, for its part, is still one of the most widely reported diseases, a role it has held at least since the forties and fifties, when cancer research began to really develop in the United States (Patterson, 1987). In fact, cancer in the early part of this century

held something of the same place in the mind of the public as AIDS does now, as an incurable and shameful disease (Patterson, 1987). In the last twenty years, however, there has been an explosion of research in cancer genetics, so that now "(h)ow cancer develops is no longer a mystery" in the words of one of the pioneers of that research, Robert Weinberg (Weinberg, 1996, p. 62). The oncogene theory of cancer origin, first put forward by Bishop and Varmus in 1976, by the mid-eighties had become the dominant model in cancer genetics and was yielding rich research dividends in conjunction with advances in recombinant DNA techniques (Fujimura, 1996; see also Latchman, 1995). By the nineties, though, this model had been overtaken as the cutting-edge of research by the search for tumour suppressor genes (Weinberg, 1991). In 1993, an increasing number of cancers were linked to the tumour suppressor gene *p53* and *p53* was celebrated by *Science* as the Molecule of the Year (Culotta & Koshland, 1993; Koshland, 1993).

For all these reasons it was decided to investigate the reporting of cancer during 1994, as it was expected that the research impetus and excitement generated by the findings related to *p53* would be likely to lead to further discoveries and resulting reports in the news magazines. In fact, this indeed took place and 1994 saw several important breakthroughs in cancer research, including the long-awaited discovery of the gene for hereditary breast cancer, *BRCA1* (Miki et al., 1994). As a result there was a corresponding wave of reports in the news magazines, leading to a total corpus of 20 news items. Although the year 1994 was thus a suitable time frame for looking at articles on cancer research, the field has continued to be covered fairly closely since then and at least three covers of the magazines studied have since featured stories on cancer (*Time*, April 8, 1996, on prostate cancer, *Newsweek*, January 13, 1997 on *p53* itself, and *Newsweek*, May 18, 1998, on angiogenesis inhibitors).

It could be objected that such reporting of 'hot' research is slightly unusual, but this is precisely the type of research that is reported and it had the benefit of a reasonable number of reports in the same general area, which made for a very unitary corpus. The corpus will be discussed further below in Chapter 2, but the number of articles made it possible to choose all the articles on a particular topic within not too long a

time frame, producing a corpus without the normal worries of sampling and representativeness.

Although the corpus is specifically on cancer, because cancer is now considered a genetic disease (Cavenee & White, 1995; Fearon, 1997; Rasko & Downes, 1995; Weinberg, 1996), much of the research is concerned with genetics. The place of genetics in contemporary science is such that it has thoroughly permeated contemporary culture, with the double helix an icon which is instantly recognizable (Nelkin & Lindee, 1995). Genetic explanations are increasingly favoured over environmental ones, so that the old nature versus nurture debate seems to be swinging back towards nature (Nelkin & Lindee, 1995). Big science has moved from nuclear physics to genetics with the implementation of the Human Genome Project, budgeted at around $3 billion. (Cooper, 1994). On the popular front meanwhile, major television series explain the mysteries of DNA to the layman (*Cracking the code* and *In the blood* on BBC television).

Cancer genetics, therefore, is fully in line with the contemporary emphasis on genetic research, so that, for example, the latest edition of the standard genetics textbook by Lewin, *Genes VI* (Lewin, 1997), has as its final part a section on "Cell growth, cancer and development", showing how the description of cancer is falling into place as a key component in the explanation of the cell cycle. Research on cancer of the type described in the corpus, therefore, is not purely medical, but is most definitely bioscience with important implications for general research in genetics and molecular biology. The model developed, therefore, may be said to describe biomedical rather than purely clinical or medical texts and is therefore potentially more generally applicable to bioscience texts as well.

Popularization: towards a more complete theory

The importance of science in contemporary society and the concomitant central place that the popularization of science holds in communicating the findings of scientific research to the general public would suggest that there would be a powerful developed

theory lying behind the idea of popularization. In fact, as will be demonstrated below, since much of the popularization of science has been discussed by sociologists and scientists rather than linguists, the existing theory of the popularization of science leaves much to be desired from a linguistic point of view. This diffusion theory (Lewenstein, 1994) sees communication as a simple process of information flow from research article to popularization. When this does not work properly we then have the perception of the popularization having been distorted (Hilgartner, 1990). The weaknesses of this as a model of communication mean that it is worth considering in much greater depth just how communication is achieved in the popularization of science.

Similarly, the Star Wars conflicts alluded to above between scientists and sociologists of scientific knowledge mean that we need to examine, to the extent that it impinges on the argument, the position of sociologists concerned with science. Since a lot of work on scientific discourse has in fact been carried out by sociologists like Latour & Woolgar (1986), this type of approach needs to be looked at to see if it can throw any light on how communication of science is achieved.

These approaches by those who have examined science in broader terms need naturally to be supplemented by more detailed linguistic analyis, which makes up the bulk of the work. The type of approach taken to the analysis of the texts under discussion develops through the thesis. Though the initial approach is that of genre analysis (Swales, 1981, 1990) and a critique is made of the only current genre model of popular biomedical texts, that developed by Nwogu (1991), this type of analysis is complemented in later chapters by other approaches. A more in-depth approach to the comparison of the individual original research articles and the popularizations based on them, following more the approach of Myers (1990), enables a more detailed and subtle description of the differences between the two genres: the focus on the new research claim or the importance of the research implications for the wider society. The third type of analysis used is that of relevance theory (Sperber & Wilson, 1986/95), which once again allows a more precise analysis of just how communication is achieved in the popularizations.

The use of these different methods of discourse analysis has two main purposes. The first aim is to try and put forward a model of the popularization of science that holds up in linguistic and communicative terms. It takes account of work already done in this area by sociologists, journalists and educators, but strives to produce a theory which has more validity as a theory of linguistic communication. The layered analysis approach means that it is possible to produce insights which will be of interest to analysts in several discourse traditions. Secondly, the claims made are both substantive in terms of the model proposed and also suggest insights into the theory used to produce the model.

Thus the first model put forward, a genre analysis model of popular biomedical texts, suggests serious modifications to the model proposed by Nwogu (1991). Moreover, the model itself has implications for genre theory, as the analysis of the corpus leads to suggestions on the nature and definition of the concepts of genre and move which are pivotal to genre analysis. The latter concept certainly has not been adequately characterised till now.

The genre model is then taken further in qualitative terms to enable us to tackle areas which cannot be adequately covered by a purely genre analysis approach which does not examine the discourse of any one text in great depth. In this respect the narrative of science model of Myers (1990) is extended further to cover the type of text analysed here, i.e. texts from mass news magazines, a type of popularization of science not examined by Myers, who restricted himself to texts from *New Scientist* and *Scientific American* (Myers, 1990). This further develops the genre model proposed by enabling us to support the model given with detailed comparisons between research articles and popularizations. In terms of methodology, it demonstrates that detailed qualitative analysis of an individual text or pair of texts can supplement genre analysis to give added depth to the analysis of texts of this type.

The final type of discourse analysis, that based on relevance theory (Sperber & Wilson, 1986/1995) connects up the argument with more general work in pragmatics and enables the model to stand as a model of communication rather than simply a

static textual model. This once again acts to strengthen the model proposed. For genre analysis generally it shows how a somewhat product-based model can be developed in more dynamic terms. As regards relevance theory it endeavours to argue that this theory, which is often rather dismissive of other approaches to pragmatics and discourse analysis, is capable of coexisting with a totally different type of approach and is not as incompatible with more traditional discourse analysis as is claimed by relevance theorists like Blass (1990, 1993). In addition, it demonstrates that concepts like move can be explained in terms of relevance theory and thus widens the explanatory power of the theory to include such hitherto excluded areas, as well as illustrating its applicability also to written texts.

This work, then, attempts primarily to suggest a new model of the popularization of science in more linguistic and communicative terms than proposed hitherto. In terms of genre analysis, it suggests a new genre model of popular biomedical texts. As regards relevance theory, it argues that this model of discourse analysis is compatible with genre analysis and that it can throw new light on aspects of genre analyis.

Since, as indicated at the beginning of this chapter, popularization of science is a multidisciplinary area, this work will touch on a number of fields. Due to limitations of space, however, as well as the primarily linguistic focus of the work, such discussion will be limited to that necessary to illuminate the main theme. In addition, certain approaches, such as the cultural studies analysis of medical themes exemplified by authors like Epstein (1995), or the more radical critiques of science studies, whether by feminist writers like, for example, Donna Haraway (1996) or proponents of new literary forms (Ashmore et al., 1994; Cooper, 1997), will not be examined at all. Similarly, analysis of the processes of scientific research by philosophers of science such as Laudan (1996) or Lakatos (1978), although interesting and suggestive at times, would take us too far from our main focus.

Even within discourse analysis and pragmatics themselves there are, of course, widely different approaches to the analysis of discourse (e.g. Schiffrin, 1994). The approaches taken here are followed for two main reasons. First the analysis builds on

the work already done in this area and tries to show how such work can be extended in useful directions (e.g. that of Swales, 1990, and Myers, 1990). Secondly, where the approach used is novel, as in the use of relevance theory to analyse written genre texts, this is in order to deepen the model and illustrate how the theory can be deployed more broadly. It is not to suggest that useful advances might not come about by use of other discourse models as well, such as that of the register analysis of systemic linguistics (e.g. Halliday & Martin, 1993), which may yield greater insights into areas of concern to that theory, such as grammatical metaphor (Halliday, 1994).

In an analogous fashion, it is not claimed that the model proposed here says everything there is to say about the type of texts analysed. Clearly, it would have been possible to analyse even the existing corpus in much greater detail, but analysis was limited to that necessary to demonstrate the main lines of the thesis. Thus various aspects of the popularizations, such as the use of visuals, have not been analysed at all. As regards the implications for relevance theory, even greater restraint has been shown. Some possible further developments of relevance theory are suggested, but since the main thrust of this work is towards the development of a new model of popularization rather than a new version of relevance theory, no attempt is made to alter the theory in its main outlines. The arguments in terms of relevance theory are aimed more, first of all, at strengthening the model, and secondly, at showing that relevance theory is compatible with the other main discourse theory used, genre analysis.

Another area not touched on is the relevance of the model to the teaching of English for Science and Technology (EST). Although the author of this work has been involved in teaching EST for many years, this thesis is not concerned with language teaching as such. It aims instead to use applied discourse analysis to characterize a different type of communication of science. The model of the popularization of science proposed may be of interest to teachers of EST, but it is aimed more at those concerned with such communication: journalists, educators, and those concerned with the Public Understanding of Science, as it is called (Wynne, 1994). In addition, of course, it is directed at those concerned with the description of science texts of various

types, whether in the framework of genre analysis or more broadly. Other genre analysts, investigating non-scientific genres, might also find it of interest. Those not primarily interested in scientific texts of any sort, but who use, or are interested in, relevance theory, should, I would suggest, find those chapters concerned with the application of relevance theory of interest to them in the context of the general development of the theory. More generally, other discourse analysts, whatever their theoretical persuasion, may find the attempt to integrate what is normally considered a socially oriented text-based product theory, i.e. genre analysis, with a cognitive utterance-based process theory, i.e. relevance theory, yields some insights.

The thesis starts off in the next chapter with an investigation of the genre of biomedical texts before continuing with a detailed comparison of the original research articles and the popularizations based on them. The genre model developed, that of the narrative of society, is further contrasted with existing models of description of science communication, coming from outside linguistics. We then turn to relevance theory and see how that approach can produce additional advantages for the model, before showing that the two approaches of genre analysis and relevance theory are compatible. We finish by looking at some implications of the model put forward.

CHAPTER 2

A GENRE ANALYSIS OF POPULAR SCIENCE ARTICLES

In this first substantive chapter we shall investigate the texts in our corpus in terms of genre analysis in an initial attempt to characterise the texts as a type and establish their characteristics. The description of the genre of popular biomedical texts is of interest in its own right and new suggestions will be made as to how this genre might be described. In addition, though, this will also lead us to make some proposals regarding the use of the terms 'genre' and 'move' themselves. First of all, however, we need to have a look at just how the idea of 'genre' has been utilised by other authors.

The concept of genre

Although the term `genre' has a long history in literary studies (e.g. Frye, 1957), it is only relatively recently that it has come to be widely used in linguistic analysis of texts. As a result, there is no broadly agreed definition of the concept of genre, or the related idea of register (Biber, 1995). The extent of the difference between genre and register is also unclear, with some linguists, e.g. Leckie-Tarry (1993, 1995), making a principled distinction between the two, while others seem to use a definition of one term which another linguist would equally happily apply to the other. Thus Biber (1994, p. 32) uses "the term *register* ... as a general cover term for all language varieties associated with different situations and purposes", while Bhatia (1993, p.13) defines genre as a "recognizable communicative event characterized by a set of communicative purposes" used by a professional or academic community.

Perhaps as a result of this vagueness of definition, de Beaugrande in fact has described register as a "concept in search of a theory" (de Beaugrande, 1993). In an attempt to pin down the concept of genre, Paltridge (1995a) listed five different approaches to the concept, but noted that in fact all of these followed either a basically systemic-functional approach or the approach of Swales (1981, 1984, 1990). But even within

the systemic-functional approach there seems to be considerable disagreement about the relationship between register and genre, with Halliday (1994) seeing register as being broader than genre, whereas Eggins and Martin (1997) see genre as the overall concept. For her part, from an American perspective, Hyon, in her survey article of work in genre, includes the American New Rhetoric analysts, along with the work in ESP and the systemicists (Hyon, 1996). From a New Rhetoric point of view, Miller (1984) sees genre crucially as a form of social action, so that a text type may fail as a genre despite having a clear formal structure if there is no pragmatic component, or way of understanding the genre as action in society.

Halliday likens a register to a dialect, noting that it is constituted by a cluster of features associated together at greater than chance levels and that "like a dialect, it can be identified at any delicacy of focus" (Halliday and Martin, p. 54). This is, of course, in line with Halliday's functional approach generally (Halliday, 1994), and is arguably useful in the practical description of a register. But even Biber, who uses a register approach, criticises Halliday's "open-ended parameters" as being imprecise and underdetermined, thus making comparison across registers difficult (Biber, 1994, p. 34).

Given Biber's interest in comparative analysis (Biber, 1995), this criticism is understandable, but it has relevance to more than comparative cross-linguistic work. This openendedness makes it very difficult to identify just what features are constitutive of a register. Biber (1994, p. 34), for his part, argues that registers should be defined in terms of "register markers, which are distinctive linguistic features found only in certain registers", in addition to differing uses of more general features. The problem with such an approach to identifying registers is pointed out, however, by de Beaugrande (1993), when he argues that we cannot specify a register in terms of features all of which it must have or which it cannot have.

Thus there seems to be a central area of disagreement in register studies as to how registers are constituted. One standard review of register studies characterises them as being distinguished by a commitment to aiming at formal linguistic characterisation of

language varieties and a link between form and situation (Atkinson & Biber, 1994). Such surface characterisation typifying work on register is at odds with the more discourse-oriented work on genres characterised by the analysis of move structure. Whereas register analysis has tended to focus on what might be termed more surface aspects of texts, genre analysis has attempted a deeper level of investigation (Bhatia, 1993). Thus most genre analysts agree in making a fairly sharp distinction between genres and registers, with genre being seen as a more multifaceted concept (Bhatia, 1993; Swales, 1990; Yunick, 1997). Within the area of genre studies concerned with scientific texts, the most standard approach is probably indeed that of Swales (1981, 1984, 1990). Swales' model is based on the concept of communicative purpose taken over by Bhatia and is also centrally concerned with the concept of discourse community. Although this latter concern might seem to link Swales' ideas with the more socially defined work of Halliday, out of which the idea of register developed (de Beaugrande, 1993; Halliday, 1994; Halliday & Hasan, 1989; Halliday & Martin, 1993), there are, in fact, considerable differences of approach between the two.

This social nature of genre is highlighted by Bazerman in his definition of genre, where he sees a genre as a "social construct" (Bazerman, 1988, p. 68). This conception of a genre as "a socially recognised, repeated strategy for achieving similar goals" (Bazerman, 1988, p. 68) bears a close similarity to Swales' idea of discourse community (Swales, 1988, 1990). Where the concept of discourse community goes somewhat further is in its focus on a group communicating with one another as those producing a genre.

Swales (1990) in fact gives a total of six defining characteristics of the discourse community, which indicates that it is not a simple idea. But basic to the concept is the idea that a discourse community shares common communicative goals and has mechanisms of intercommunication. Where there seems to be a difficulty in using the idea of discourse community to identify and define a genre, however, is that one of the defining characteristics, for Swales, of a discourse community is use and ownership of a genre. There would, hence, appear to be some circularity involved in saying that a genre should be defined in terms of the discourse community which

shares it, when discourse community is defined, at least partially, in terms of the genre(s) it uses. But certainly this is what both Swales and Bhatia in fact do. Perhaps we might say that genre and discourse community are mutually reinforcing or mutually defining concepts, but there, nevertheless, would appear to be a problem in this aspect of the idea of genre.

The mutually reinforcing nature of genre and discourse community is brought out in another way by Bazerman (1991), who argues that not only are genres produced by a particular discourse community but that the converse relationship also holds. Thus a particular discourse community may be established as distinct by virtue of the discourse positions it maintains in contradistinction to other discourse communities. This implies, therefore, that each discourse community will employ a particular discourse that will, in constituting that community, also act to separate it from other similar, or not-so-similar, communities. Just because of this aspect of the nature of particular genres, therefore, different discourse communities will find it hard to communicate across these generic barriers. In order to communicate, a discourse community would have to make use of some aspects of the discourse of the community it wished to communicate with. Thus scientists working on the boundaries of discourse communities might find it hard to communicate. This is just what Journet (1993) found in examining the work of S.F. Jelliffe. The latter, working in an area which straddled psychology and neurology, tried to "blur the generic conventions" of both disciplines "by `translating' the values and principles of one discipline into the language and discourse forms of the other" (Journet, 1993, p. 511).

Genres, therefore, would seem not to be fixed and unchanging, but may be adapted in the light of circumstances. It is this dynamic aspect of genres which is focussed on by Berkenkotter and Huckin (1993), who stress the changing nature of genres which develop in terms of the changing needs of the discourse community in response to changing circumstances. This is echoed by Reynolds, who notes the dynamic changing nature of genres which alter in response to both producer and receiver needs (Reynolds, 1997). Despite this insistence on the dynamism of genres, however, there was never really any doubt that genres changed, since it has been clear for some time

from Bazerman's work on the historical development of the research article that genres developed in response to the communicative needs of the scientific community (Bazerman, 1988).

Such an emphasis on the nature of genres as "inherently dynamic rhetorical structures" (Berkenkotter & Huckin, p. 477), however, is in danger of distracting us from the very fact that makes a genre a genre. To constitute any sort of genre at all, a text type must display some sort of recurring pattern; otherwise it could not be recognised to be similar to other texts, which would also belong to the genre, and different from others, which do not. The question consequently arises as to just what is the proper unit of analyis to use when characterising genres (Reynolds, 1997).

We have seen that register analysis also faced the problem highlighted above by de Beaugrande as to whether a register can be defined in terms of obligatorily existing features or not. Paltridge (1995b) has tried to solve the problem of obligatory moves in genres by arguing that we think of genres in terms of prototype theory and that particular texts should not be said to be an example of a genre because of the presence or absence of a particular defining feature. Such an attempt to finesse the problem by saying it is a non-problem does not, though, help in the practical analysis of texts. Even if we say that a text should not be characterized by the presence or absence of a feature, in any particular instance the genre analyst is still faced with the decision as to whether a specific text is an example of a particular genre or not.

Where Paltridge's appeal to prototypes may be valuable is in pointing out that not every example of a genre may necessarily contain every feature found in that genre. This, however, had been recognised by Swales in his classic work on article introductions (1981, 1984, 1990), with Swales in fact also making reference to prototype theory (Swales, 1990). He notes that not all moves need be found in every introduction, a finding extended by Taylor and Chen (1991) in their analysis of articles by Chinese scientists in various related fields. Despite the fact that in the latter work even such an important step as reference to the literature might often be

missing Taylor and Chen were still able to analyse the Chinese scientists' article introductions in terms of the Swales model.

It would seem then that we need to examine rather more closely the idea of the move, since the characteristics of the move seem to be what is at issue here. The move is perhaps the particular aspect of genre analysis that has served most clearly to differentiate it from register analysis. It is that factor which most evidently identifies a text as being an example of a genre and the move structure, i.e. the existence of particular moves and their occurrence in a particular sequence, that which definitely identifies that genre. Despite some criticism by Fairclough (1995a,1995b) of this emphasis on moves, to the effect that it leads to the neglect of other factors in the genre, the structure of moves is so important to genre analysis that it deserves some more attention.

Moves are so taken for granted and Swales' 4-move, and later 3-move CARS, model (1981, 1990 respectively), is so well-known and has been so influential that it is puzzling that the concept of move which the earlier work introduced into genre analysis is so little defined. Similarly, Bhatia, in his standard work on genre analysis, although he details the move structure of such genres as the sales promotion letter and the legal case, does not discuss what constitutes a move (Bhatia, 1993). This is not because the thinking which lies at the root of the genre has not been identified. Swales himself relates in some detail the influences on his work that lead to the approach to genre analysis that he uses. It is clear from the introduction to his groundbreaking '*Aspects of article introductions*' that the original inspiration was pedagogic and grew out of work on the sections of reports, projects or theses (Swales, 1981). In fact what Swales does is describe the particular moves involved: but he does not explicitly explain what a move *per se* consists of. The concept of the move, however, seems to have been taken over from Sinclair and Coulthard (1975). In discussing genre in the later work, too, he refers to the ethnography of speaking tradition by sociolinguists such as Hymes (1972) and Saville-Troike (1982). Their breaking down of speech events into separate stages can be seen as a clear precursor of the idea of a move. Similarly, although not specifically referred to by Swales, the

well-known and highly influential theory of speech acts originating with Austin and Searle can be discerned in the background (Austin, 1962; Searle, 1969). A less specific, but all pervasive, influence would seem to be the general patterning of research articles in science, which traditionally follow an Introduction-Methods-Results-Discussion format (Biber & Finegan, 1994).

Although some clear precursors to the idea of move can thus be identified and the importance of the idea to genre analysis is obvious, the lack of precise definition of a term which is so central to the whole idea of genre analysis is worrying. Different researchers may be using the term and appear to be referring to the same phenomenon when in fact they are using it to mean different things. Even if two researchers are in fact referring to the same thing when they speak of a move, this is hard to appreciate if the term continues to be used in such an impressionistic way. A researcher does, though, seem to be referring to something concrete when she talks about a move, and concrete examples of a move abound, whether Swales' `Establishing a niche' in research article introductions or Bhatia's `Offering incentives' in sales promotion letters, to name only two of the most well-known examples (Bhatia, 1993; Swales, 1990). This does not, though, tell us what constitutes a move.

We can come a little closer to answering this question if we examine Bhatia's main criterion for identifying move structure. For Bhatia it is "one of the fundamental principles of genre analysis... that the focus of analysis be on the functional rather than the formal characteristics of linguistic data" (Bhatia, 1993, p. 86). Thus for him it is the communicative purpose and the discoursal value which help to identify the move, noting that there is no one-to-one correspondence between a formal feature and its discourse value. The same discourse value can be realised by different formal features and different formal features can instantiate the same discourse value. A move, therefore, for Bhatia is a functional rather than formal unit.

Similarly, Swales explicitly rejects, having initially attempted, a structural analysis following Sinclair and Coulthard (1975) and aims for a process rather than a product model (Swales, 1981). Though this would seem also, therefore, to be a functional

rather than structural approach, Henry and Roseberry attribute to both Swales and Bhatia a view that "genre is a formal property of texts" (Henry & Roseberry, 1996, p. 473). Most commentators, e.g. Hyon (1996), or Flowerdew (1993), also consider Swales' concept of genre to be a product approach. This lack of agreement on such a fundamental principle as to whether a formal or functional approach is being used is indicative of the still problematic status of the concept of move.

Nevertheless it would appear that a feature common to all the authors discussed who work within the Swales approach is that a move can be characterised in terms of its communicative purpose. What Henry and Roseberry seem to have in mind with their description of genre being a formal property of texts is that genres can "be described as a sequence of segments, or *moves*" (Henry & Roseberry, 1996, p. 473-474). It is the fixed number and sequence of moves which give genres these formal properties. The question, therefore, arises as to what extent moves can be termed fixed in number and sequence.

Henry and Roseberry (1996) admit that moves may show variations in order and that some moves may be omitted, following Hasan (Halliday & Hasan, 1989). The latter work in fact provides a notation to show obligatory and optional moves, which is used by Henry and Roseberry and also by Paltridge (1995b). It should be noted, though, that Hasan is here talking about genre and what she terms "generic structure potential", rather than register (Halliday & Hasan, 1989). Thus research in the Hallidayan tradition and genre analysis which is influenced by this, like that of Henry and Roseberry, Paltridge and Ventola (1987) is concerned with identifying obligatory and optional moves. But whereas Hasan identifies certain moves as obligatory in a genre, Ventola does not see obligatory elements as genre-defining (Ventola, 1987). In her framework, representation of a genre via a flowchart as a dynamic process allows the presence or absence of certain moves within an overall genre type.

This might seem an attractive option, but it is more suited to the type of ongoing interactive type of genre that Ventola studied (service encounters) than to the more static written text-based genre we are concerned with. As Paltridge points out,

however, the central Swales approach has been less concerned with problems of obligatory and optional moves and recursion (Paltridge, 1995a). Nevertheless, it is clear that Swales does not see 100% presence of a move in a particular order as being crucial, referring only to "suitably robust preferred orders" (Swales, 1990, p. 145). The fact that the CARS model has only 3 moves makes each move practically obligatory, but the use of steps in each move and the optional nature of many of these shows that a precisely numbered and ordered sequence is not mandatory. Other researchers, like Hopkins and Dudley-Evans, who investigated parts of the research article like the discussion section and who found larger numbers of moves, found that not all moves were mandatory (Hopkins & Dudley-Evans, 1988). Similarly, it is clear that some moves occur in a cyclic way, recurring throughout the text (Crookes, 1986; Hopkins and Dudley-Evans, 1988; Swales, 1990).

The question remains, therefore, as to just how to characterize moves. The problem is both a theoretical and a practical one. Practically, the researcher needs some criteria by which moves in particular texts may be characterised. Without such criteria moves cannot be consistently identified and thus genre analysis cannot be carried out at all. In terms of the theory involved, unless we can say just what sort of entity a move is then the whole status of genre as a theoretical construct is left hanging in the air and genre analysis itself has no validity other than as an *ad hoc* pedagogical device which may be useful to certain types of student of English.

Clearly, this is not a satisfactory state of affairs. In the remainder of this chapter, therefore, I shall make a move analysis of a corpus of texts in popular science. This analysis has two main purposes. It will attempt to describe the genre of popular science articles in news magazines in terms of their move structure. Secondly it will try to determine what problems there might be with existing ideas of move structure and define the concept of move and genre more tightly. Before setting out the specifics of this analysis, however, we need to look in more detail at what work has already been done in analysing the discourse of science texts.

A genre analysis of popular science texts

As noted above, much of the research which has taken place in genre analysis has in fact been on the basis of science texts and science texts have, after Swales, been the core type of text which has been subjected to genre analysis. This has been for a number of reasons, the most important one being the pedagogic necessity of training foreign students of science in the rhetorical structure of the research article (RA) in English. Various aspects of the RA have been investigated, ranging from the classic work by Swales on the introduction sections already discussed (Swales, 1981, 1990) to work on other sections by a number of researchers. Thus Hopkins and Dudley-Evans (1988) for example, have worked on discussion sections, with such sections in biochemistry articles being investigated by Thompson (1993), and those in history, political science and sociology examined by Holmes (1997). Wood (1982) looked at the methods sections of certain types of chemistry articles. An examination of the results sections of sociology articles has been carried out by Brett (1994).

As noted above, most work in the analysis of scientific texts in the genre tradition has been on the research article. However, the research article is by no means the only type of text in science. Sticking only to written texts, other types of scientific text that have been investigated, in one discourse tradition or another, include the review article (Myers, 1991), the MSc dissertation (Dudley-Evans, 1994), the university textbook (Love, 1991; Myers, 1992; Swales, 1993, 1995), the school textbook (Halliday & Martin, 1993), the scientific poster (Dubois, 1985) and the scientific report (Marshall, 1991) and genres more specific to a particular discipline like the case report in medicine (Salager-Meyer, 1994). However, if we turn to the genre that we are centrally concerned with here, that of the popular scientific article, we shall find that not much work has been done within a genre approach on this.

That the popular scientific article is likely to differ from the research article, or indeed any of the other genres listed in the previous paragraph, has been recognised by a number of researchers, as far back as Fleck (1935/1979). Thus Hopkins and Dudley-Evans hypothesise that the "serious" scientific article may differ both from the

"popular" scientific article and the "serious" non-scientific article in different ways (Hopkins & Dudley-Evans, 1988). Although they do not in fact analyse the popular article, it, nevertheless, seems to be accepted that the genres are different. If the central feature of genre is communicative purpose, this is quite understandable, as it seems quite likely that the research article and the popular science article have different purposes.

If we examine, however, actual analyis of popular science articles, we find rather a shortage of genre analysis work in this area. Research from a wider rhetorical perspective like that by Fahnestock (1986), or Rowan (1989) will be discussed later, but even more specifically linguistic papers like that by Adams Smith on the popularization of medical research papers for the layman take a rather structural approach to the phenomenon in much of the paper, analysing "Surface differences between source and derived versions" (Adams Smith, 1987). Where Adams Smith does analyse the discourse she examines the differences in terms of a `Problem-Solution' structure in the popularization, as compared to an `Unexplained-Explanation' structure in the original. This is an interesting adaptation of Hoey's (1983) model, but it is not one that could be characterised as a genre approach since such problem-solution structures can apply to a wide variety of genres.

Dubois's classic study on what she terms "scientific translation for the laity" clearly indicates that the NEJM articles are altered both in structure and language, but she does not attempt to characterize the structure of the popular article (Dubois, 1986). Although Crismore and Farnsworth (1990) also compare popular and professional science writing, they restrict themselves to a particular aspect (metadiscourse, or the author's commentary on her own text) and do not look at the kind of text we examine here. Thus there would seem to be a dearth of genre and more broadly discourse accounts of the popular science article. Hence Nwogu, whose approach I shall analyse in detail below, states that, "there have so far been no studies which have attempted a detailed investigation into the discourse structure of popularized science texts." (Nwogu, 1991, p. 111), apart from the work in which the quote itself appears. Aside from Nwogu's work, therefore, there have been no major genre analyses of

popular science texts. In addition, Nwogu's research, while suggestive, has several problematic aspects which make it necessary to re-examine the question of the genre structure of popular science texts.

Popular science texts, consequently, have not been extensively analysed within genre studies. Yet, as Myers (1992) points out in his examination of two biology review articles, we cannot be satisfied with a state of affairs where the sole type of scientific text under analysis is the research article. As he says, we must look at other types too, including popularizations. Although Myers is concerned with wider issues than genre analysis here, arguments on wider issues are to some extent at least dependent on a prior analysis of genre structures.

Corpus

It is interesting that the three studies on popularization of science cited above, i.e. Adams Smith, Dubois and Nwogu, have all in fact looked at the popularization of medical texts. This would seem to indicate that medical texts are a central type of popular science. As indicated in chapter 1, this is in fact the case and provides a justification for analysing this type of text in terms of its validity as the most important type of popular science text.

The particular focus on the specific type of text investigated, i.e. popular science articles in general news magazines, can be justified because of the relative neglect of this type of text in research on popularization of science. Although Nwogu (1991) does in fact examine one or two texts from *Newsweek*, these are lumped together with texts from *New Scientist* and *The Times*. Adams Smith (1991) also analysed only a very small number of texts (four), also from *New Scientist* and *The Times*. Even those studies, usually content-analysis based, which are much wider in their coverage, tend to examine newspapers rather than magazines. Thus, for example, Evans et al's study of science coverage in the prestige and national tabloid press in the US looked at stories in the *New York Times* and the *Philadelphia Enquirer* on the one hand, and the *National Enquirer* and the *Star* on the other (Evans et al., 1990). Large studies like

that by Singer, which attempt to cover as wide a variety of media as possible, include the news magazines, but in such studies the characteristics specific to these magazines get lost as the more general conclusions are drawn (Singer, 1990). If we look, on the other hand, at a study which focussed more specifically on the topic covered here, that of cancer, we find that a major study on the coverage of cancer, sponsored by the National Cancer Institute, looked only at news stories in the top fifty newspapers in the United States (Freimuth et al., 1984).

It seems then, that analysis of weekly news magazines seems to have been relatively underemphasised in the research to date. There is one exception to this generalisation and that is that articles in weeklies like *New Scientist* have been analysed, as we have seen. But *New Scientist* is much more akin to the kind of popular science writing seen in a magazine like *Scientific American* than it is to general news magazines. As Myers points out in his study of articles written by scientists for *New Scientist* and *Scientific American*, the audience for these magazines is at least partially made up of other scientists, and, at the very least, those interested in science (Myers, 1990). Readers of general news magazines need have no particular interest in science at all. Thus the former two magazines are more akin in their writing to scientific research articles than to general newspaper or magazine reporting of science (Myers, 1990). Myers himself, therefore, calls for further study as to whether the model he sets up for popular science writing "applies to popularizations written by science journalists in general-interest magazines and newspapers" (Myers, 1990, p. 185). Given the different type of audience involved in the general news magazines, the hypothesis was that the type of popularization would differ considerably from that described by Myers. In order to see whether this was indeed the case, or whether Myers's model would also apply to these texts, as well as to investigate more generally the characteristics of such a type of popular science article, it was decided, therefore, to examine texts in the three general news magazines, *Newsweek*, *Time* and *US News & World Report*.

In order to ensure that the corpus was relatively homogeneous and thus that appropriate generalisations could be made on the basis of the analysis of the corpus,

one particular topic area was chosen, that of cancer. The reason for the choice of this topic area was twofold. Firstly, it has been established that the general area of medical research is the most widely reported area of popular science (see Chapter 1 above). Within this area the field of cancer research was in a state of explosive growth at the time (1994), evidenced by the fact that the tumour suppressor gene, *p53*, had just been named `Molecule of the Year' by *Science* in December, 1993 (Koshland, 1993) and that, as we shall see, 1994 was to witness the discovery of *BRCA1*, the long sought after breast cancer gene (Miki et al., 1994), among other discoveries. This meant that the topic was in the news and a sufficient number of popular science articles could be expected, as was indeed the case.

To avoid problems with regard to sampling and decisions as to whether or not to include an article in the corpus, simple and relatively clear cut parameters were decided on. Firstly, for reasons indicated above, the corpus would come from the three general news magazines, *Newsweek*, *Time* and *US News & World Report*. Secondly, the calendar year 1994 was chosen as the time period involved. Because of the rapid growth in the field indicated it was felt that this was a sufficient period to gain a reasonable number of texts for analysis and there was a clear upper limit to the period from which could be drawn while remaining reasonably up-to-date in terms of content.

It was unproblematic to establish whether there was an article in any particular issue which might be relevant. All three magazines had sections variously labelled with headings such as `Science', `Health' or `Science and Society', etc., where any article on cancer research would be found. It was also relatively simple to determine whether an article was on the topic under investigation. Most of the articles had a clear major focus on cancer research. In the few cases where the topic was not immediately obvious from the title of the article, a reading of the article established the topic without much doubt. One article, Text 17, *It's not as simple as taking a blood test*, was included, because it was about cancer, even though it was slightly different in that it reported clinical recommendations. Only independent articles standing alone were chosen. Those shorter articles on cancer which were only, for example, a short

section among more general news items, e.g. *`News you can use'* in *US News & World Report* ("New prostate prescription", 1994), or independent boxes within a larger article, were omitted (e.g. Watson, 1994). In the latter case this was because the discourse structure of these texts was completely different. The boxes were neither completely independent in that they were usually an aspect of the larger text, nor were they exactly part of that larger text as there was no overt link to the larger text. This particular exclusion will be examined further below as part of the discussion of move structure and in comparing RA's and popularizations.

In addition those articles which talked about cancer only incidentally as part of a larger issue, e.g. the estrogen complex (Begley with Glick, 1994), were omitted. Because of the importance of cancer genetics in the general development of genetics and such areas as the cell cycle, this meant that more general articles on such issues as the Human Genome Project were likely to mention cancer (Elmer-Dewitt, 1994), but these too were excluded as not being primarily on cancer.

Only one article was excluded that was obviously mainly on cancer and that was an interview with the author of a book about cancer for patients and their families (Brink, 1994). The reason for the exclusion here was that this article was a) an interview rather than a news report, and b) not on recent news about cancer but rather direct health advice. On both those counts it would seem to be a different genre. Other articles, which would seem to be part of the same genre of reports of biomedical research and had the same move structure as the cancer research articles included in the corpus, e.g. *`A gene that says `No more!"* on the discovery of an obesity gene in mice, were nevertheless excluded from the corpus (Seligmann with Namuth, 1994). However, such similar articles, which seemed to have the same structure as the corpus articles, but which happened not to be about cancer, are important in indicating that the genre identified is rather, at a minimum level of generality, `popular biomedical research article' rather than 'popular cancer research article'.

The twenty articles found are divided fairly equally between the three magazines in the ratio 7:5:8 *Newsweek*:*Time*:*US News & World Report*, respectively. They are

listed and numbered for ease of reference in Appendix A. Copies of all the news magazine texts themselves are given in Appendix C. The articles vary in length from a minimum of two paragraphs (Text 4) to a maximum of 34 (Text 12). The typical length, though is about a page and the median number of paragraphs is 11. They are thus fairly similar to the other articles in the magazines in which they feature in this respect.

This total number of twenty compares favourably with the numbers of texts typically found in genre studies. Swales (1990) lists a total of some 40 studies in textual analyis of science texts with the number of sources ranging from 2 to 110, (where the corpus size is known), but the median number is 10. Swales' own 1981 work used a large number of sources (48), but these ranged over a wide variety of disciplines, so that those from the life and health sciences totalled only 16. Despite the obvious advantages of having a larger sample, there are dangers in using texts from too wide a range of disciplines. Tarone et al. (1981/1985), in their well-known work on astrophysics texts, note the specific nature of these texts and Swales in commenting on their work himself cautions against overgeneralising their findings (Swales, 1985). It is clear from a number of studies that RA's in a number of different disciplines may vary (Brett, 1994; Dudley-Evans & Henderson, 1990; Wood, 1982).

That it is not at all necessary to have a large number of texts if we wish to make a more detailed analysis is seen in the work not only of Tarone et al. (two texts) but also that of writers like Myers (1990, 1991), the latter paper looking at only two articles. Although Myers does not work within a genre analysis framework, it is clear that he hopes to make worthwhile generalisations about a whole class of texts as a result of his analyses and does not feel that this is vitiated by being based on a relatively small number of texts.

A factor which must also be considered in designing the corpus is to what extent the corpus reflects a natural class of texts. Since my corpus is drawn from a particular type of source, i.e. general interest news magazines, which is widely recognised (e.g. Myers, 1990; Singer, 1984) as a text type (although not a genre, because it *includes*

various genres such as news articles, letters to the editor etc), it has much greater face validity than a corpus drawn from a wider variety of sources like that of Nwogu (1991) which lumps together such disparate sources as *New Scientist*, *The Times* and *Newsweek.*

Since I shall analyse Nwogu's work in detail below, it is also worth pointing out another problem with Nwogu's sample which my corpus expressly avoids. Although Nwogu explicitly gives a rationale for the choice of such a disparate range of sources, basically in terms of what he calls "balance and representativity" and "their reputation as organs for the popularization of scientific research findings" (Nwogu, 1991, p. 113), he does not explain how he chose his actual sample of texts. Fifteen texts were chosen which "were published between May 1985 and June 1987". Since all three of his sources were chosen because they published weekly on popular science, clearly the total number of texts published during this more than two-year period would have run into the several hundred. It is not made at all clear how these 15 texts were chosen out of the several hundred, except that to say that they were from the field of medicine. Given though that, as we have seen, medicine is the most frequently covered area of popular science, there still must have been many more than 15 texts to choose from. If this was a random sample this is not stated; if not, the basis is not evident.

My corpus, however, uses the *total* number of texts published in a certain field. This field, cancer research, is probably typical of the broader range of biomedical research, as far as I can see from looking at other texts on biomedicine but which were not specifically about cancer. Focussing precisely on this narrower field, however, made it possible to be fairly certain that all the texts which were about this field were covered and that nothing outside the field was also included. Alternative methods of sampling were tested in slightly different sources to see if they produced valid samples. Thus a search by the keyword `cancer' in the title of the article was used to examine the CD-ROM of all Guardian and Observer articles for 1994. The problem with such a method of sampling was that it threw up a large number of articles of the type `Famous person dies of cancer', or similar, where the fact that the person

concerned died specifically of cancer was irrelevant to the story and had no relation to reporting on cancer research.

The method of assembling a corpus used in this study, therefore, may be considered to be an accurate and rigorous, if somewhat time-consuming, method of assembling a corpus . It avoids the statistical problems involved in any sort of random sampling by taking the total number of texts found as the sample. The field studied was defined in precise but relatively broad terms as any study concerned with any area of cancer research. Thus specifically clinical papers were ruled out, but only one of the texts was rather clinical (Text 17), and even that was also research-based. Texts were rather what might be termed biomedical. The research described was by any standards important mainstream scientific research, much of it reported in journals of the stature of *Science*. Finally, the sources themselves were a clearly representative type and covered all three such magazines published in English. The only other magazine which might be considered comparable, *The Economist*, differs from my three sources in being a more specialist source concentrating on economic and political news and in not regularly having a science column *per se*, although it often covers scientific topics. It is not, therefore, really similar to the other news magazines.

Move structure

We have already mentioned above the problematic status of the concept move and the importance that the identification of the moves has for genre analysis. It would seem, therefore, that the best way to try and resolve the difficulties, or at least clarify them, would be to attempt a move analysis of the corpus described and see what light this sheds on the problem. Since the main attempt to date to carry out a genre analysis of "popularized medical texts" is that by Nwogu mentioned above, we shall pay considerable attention to his suggestions as the most important proposal in existence at present (Nwogu, 1991). The importance of this analysis is increased by the fact that a parallel genre analysis of the medical research paper has recently been published by the same author (Nwogu, 1997). Although my analysis in the end will

go much further than that by Nwogu, it is important that his analysis be considered since it is the most recent and only current analysis in the field.

Nwogu (1991, 1997), despite an orientation which explicitly follows Swales (Nwogu, 1991, p. 112), in fact defines a move in linguistic rather than functional terms, thus: "By the term "move" is meant a text segment made up of a bundle of linguistic features... which gave (sic) the segment a uniform orientation and signal (sic) the content of discourse in it." (Nwogu, 1991, p. 114). As noted above, Swales for his part does not actually define what constitutes a move but the names of the moves indicate a rhetorical or functional outlook rather than a purely formal one. Linguistic exponents of the moves are given (e.g. Swales, 1990, p. 144) but the moves themselves are not defined in terms of any purely linguistic properties and the Swales approach, as with Bhatia, is explicitly functional

For his part, Nwogu would seem to be putting the emphasis more on the formal elements of the text, since he says that "Moves and their constituent elements are determined partly by inferencing from context, but mostly by reference to linguistic clues in the discourse" (Nwogu, 1991, p.114). This reference to linguistic clues in the discourse may cause problems if what is meant is that particular items of language must be present, but if we focus on the idea of clues, i.e. indications, then Nwogu would seem to be on firmer ground.

My approach below is rather to turn things round and to establish criteria for the identification of moves which are based on the function of the move in the text, but which take account of linguistic exponents of these functions. Thus the moves are identified primarily in terms of the function they play in the text, the linguistic exponents of which may help to identify the purpose. What I attempt to do below and what I shall argue is a necessity for the further development of genre analysis, is to give explicit criteria whereby the moves can be identified.

I shall claim that moves may be divided into two main types, which may be called 'social' and 'scientific'. The aim of the latter is to give aspects of the scientific information related to the topic being discussed, thus, for example, to tell about the discovery of a gene for hereditary breast cancer. The point of the first type of move is

to put the `popular' into the science and to make the story `popular science'. In other words these moves show why the news has some relevance to the life of the wider society in general and the reader in particular. Each move, although it may contain some elements of both social and scientific, has a tendency towards one type or the other.

As well as the moves I shall describe various `submoves'. These correspond to the `steps' of Swales (1990) and the `constituent elements' of Nwogu (1991). I prefer the term `submove' as it makes clear the relationship to the larger move structure. In addition, the submoves in my corpus are not successive steps like those identified by Swales, but rather alternative ways of realising the overall move. In other words a move like Move 1, Relating to the reader, may be effected by using any of the four submoves listed. For this reason the general term 'move' will normally be used for simplicity. How the moves are identified is described below. Examples of the different moves as found in the texts under study are also given to illustrate the moves and submoves. The move structure found is itself laid out in Table 2.1, *Move Structure*, below. After a relatively brief description of the general approach to setting out the move structure an explicit explanation of how each move is identified will be given and the moves described.

Identification and description of moves

In examining the texts of the corpus for a possible move structure, the first thing that should be borne in mind is that this genre, popular biomedical science text, if it did turn out to be a genre with a specific move structure, would be likely to have aspects of its structure which relate to both sides of the coin, so to speak, i.e. some to the `popular' and others to the `science'. Genre, as we have seen, is standardly defined in the Swales/Bhatia model in terms of communicative purpose. The communicative purpose of the genre `popular biomedical science text in a general news magazine' would seem at first glance to be to present the results of recent scientific research to the general public. We may, therefore, hypothesise that its move structure will include elements which must relate to the science that is being talked about, but that it will also differ in being aimed at a wider audience than the original research.

MOVE 1: Relating to reader

a) Description of personal example

b) Description of general personal example

c) Direct address to reader

d) General social situation

MOVE 2: Description of main research

a) Announcement of main findings

b) General description of research project

MOVE 3: Description of background ideas

MOVE 4: Description of background research

MOVE 5: Further description of findings

MOVE 6: Evaluation of findings

MOVE 7: Description of further findings

MOVE 8: Explanation of findings

MOVE 9: Significance of findings for research

MOVE 10: Significance of findings for reader

a) Direct advice to reader

b) General importance

Table 2.1. Move structure: The moves and submoves found in the corpus

The term `social' is used in preference to `popular' because it is not claimed that the more purely scientific parts of the genre are not also `popular'. Clearly the way the science is described in these parts is not the same as the way it would be in a scientific research article.

In fact, this is just what we do find. There are certain moves, which are listed below as `social', whose purpose seems not to be to describe the science *per se*, but to make that science comprehensible and relevant to the general reader. The `scientific' moves concentrate more on the actual research which has just been carried out, or which had been carried out by earlier researchers, and which was described in the original research articles. The distinction, therefore, is twofold. The scientific moves have an origin in the original research, which the social do not have. And the communicative orientation is focussed on the reader or it is focussed on the research itself.

How are these two basic types of move identified? The detailed identification of each individual move is set out below, but in general we can say that the social moves in some way involve a mention of either a particular member of the society or a discussion of some aspect of the society, both of which are fairly clearly social. A third possibility is that they are considered social because of an orientation towards the reader. By this I mean that the move, although describing science, and thus seeming on the surface to be scientific, does not in fact have its origin in the original research being reported. Rather it is mandated by the necessity to make the original research accessible to the general reader with no research background, and thus its communicative purpose is to make the reader understand the research. For example, Move 3 gives the reader some missing scientific background which the research scientist already has. Alternatively, this 'orientation towards the reader' shows the relevance of the research to the life of the reader.

The scientific moves, on the other hand, have their origin directly in the original research and the ideas involved can be traced back there, either to the new research being reported, or to other related research. There is always some research article which is being reported, whether it is the specific main new research which brought

the popularization into being, related research, background research, or even future research which the research reported might have an impact on.

The specific scientific moves have relatively close counterparts in the research article moves discussed by Swales and other researchers like Hopkins and Dudley-Evans. Thus we can expect a reference to the main research being reported. Otherwise what is the article going to be about, after all? Similarly, Reference to background research is similar to Swales' Move 1, Step 3. The other scientific moves, Moves 5 and 7, Further description of findings and Description of further findings, respectively, are relatively unproblematic descriptions of either more of the same research or descriptions of related research, respectively. The final research move, Move 9, Significance of findings for research, is familiar from the Discussion section of RA's (Hopkins and Dudley-Evans, 1988).

The kind of move to look for among the scientific moves is thus relatively uncontroversial, since this type of move has been analysed before in the context of the RA. The type of move that might constitute a social move, is, however, more difficult to establish. These were ascertained broadly on the basis of communicative purpose within the general overall purpose of attempting to relate the research more closely to the reader. How this was done, though, can be seen more clearly if we look at all the moves more specifically and identify precisely how each might be identified.

The moves

MOVE 1: RELATING TO READER - SOCIAL

This first move is social and its purpose is to draw the reader into the story by relating the scientific topic to the interests of the reader and ensure that he reads the rest of the article. (In line with the convention in work on relevance theory, so that the writer and reader may be clearly distinguished, henceforth the former will be referred to as 'she', the latter as 'he'.) It is made up of four submoves. Any of these submoves may occur although some are more common than others. They are listed in the order from most specific to most general. Several of these submoves may occur in the same text,

being various realisations of the same overall move with the same purpose of relating the events described in the article to the interests of the reader.

a) Personal example

This is a very simple move to identify since it involves mention of an individual name. A particular member of the public is used as an example to bring the point home more vividly to the reader and ensure more of an identification by the reader. Not any name, of course, would be a realisation of this move, since names of researchers appear in Moves 2, 5 or 7. Names must be names of members of the public and normally they are the names of people suffering from the type of cancer discussed in the article. An example would be the first paragraph of Text 7, which begins "Charlene Cunningham was 25 when she discovered a lump in her breast nearly four years ago". The object of the submove is to relate to the reader by making the abstract disease more personal and real by illustrating the plight of an actual individual.

b) General personal example

The difference between submoves a) and b) is that the latter does not mention any particular individual by name. Instead it talks about people, often generically, e.g. Text 1, par. 4: "People are more confused than ever about the link between diet and health". The generic use of the article can be seen in an example from Text 2: "A breast is not a lung or a colon and when it's stricken by cancer, a woman feels special emotions that don't cling to those other body parts." Here the use of the phrase "a woman" is meant to appeal to every woman in the audience of the article.

c) Direct address to reader

This is not such a common submove but is easy to identify and is obviously meant to attract the attention of the reader and to hook him into reading the text, as in the very beginning of the very first text: "You there - you breakfasting on a chocolate doughnut with a chaser of vitamin E." Relating to the reader is up front and quite obvious here.

d) General social situation

While not so clear in either its purpose or its identification as the above submoves, this submove nevertheless also acts to relate the text and the information in it to the reader. Often it talks in general about the risk of the disease, as in Text 3: "A family history of the disease greatly increases the risk". Thus here the reader is brought face to face with the social aspect of the disease and cannot dismiss it as a purely scientific or medical phenomenon. At other times there is a description of the general society, particularly in epidemiological terms - "Thousands of American men are now discovering prostate tumors that would once have gone unnoticed." (Text 5). Although here there is also a link to the science aspect, it is still the social that is to the fore. This is a kind of generalised more impersonal variant of Submove 1a), where the patient is given a particular identity. Here the individual patient may not be named but the extent of the problem is emphasised by showing how it pervades the whole society. As a member of that society, the reader is therefore involved.

MOVE 2: DESCRIPTION OF MAIN RESEARCH - SCIENTIFIC

This move is clearly scientific rather than social as it introduces the main scientific topic of the research, the new findings which are the reason for the article being written. It is made up of two submoves, one much more common than the other. As with all the submoves, either submove can be used to realise the move.

a) Announcement of main findings

This is by far the most common type of submove and is easily recognised. Vocabulary like "study", "announced" etc is used and almost always one or more of the following three types of information: i) name of researcher(s), ii) name of institution or iii) name of publication source. For example, Text 7 has the sentence: "Last week a consortium of 45 scientists, headed by Mark Skolnick of the University of Utah, announced that they had discovered and isolated the gene, dubbed *BRCA1*." In order to establish whether this is in fact Move 2, two elements are important. The first is the position near the beginning of the text, but to rely on this would be to

prejudge the issue as to where such a move is found. Rather we can identify the move by the fact that it introduces the main study. This may be the only study, in which case it is easy to identify. However, if there are several studies mentioned it can be differentiated from other studies, as in the above example, by the time element. The time adverbial identifies the study as a recent study. Even without the time element, though, knowledge of the field can help identify the study as a new finding or background research, the latter being Move 3. Normally only a brief mention is given of the study in this move and more details are given further on in the text in Move 5, Further description of findings.

b) General description of research project

This submove is not so common, but is sometimes found in a longer text which covers a general field of research rather than focussing just on one study. Thus Text 1 is on the general topic of phytochemicals and does not focus just on one study. It introduces Move 2 with a b) submove as follows: "In the world where science merges with health, phytochemicals are the next big thing. The National Cancer Institute is so excited it has launched a multimillion-dollar project to find, isolate and study them."

MOVE 3: DESCRIPTION OF BACKGROUND IDEAS - SOCIAL

This move involves the description of ideas that are necessary for the reader to understand the new findings described in the text. It is not directly related to the research and as the purpose of the move is to help the reader understand the text it is classified as social rather than scientific. The orientation is towards the reader rather than towards the science, illustrated by the fact that there is no corresponding move in the research article move structure. There is no specific explanation of the findings, which occurs in Move 8, and it is differentiated from Move 4, Description of background research, by there being no mention of scientists or researchers or particular work. Thus in a text about testing for prostate cancer, Text 5, there is an explanation of what the prostate is: "At the center of the mess is a puny gland that sits

at the base of the bladder, forming a ring around the urethra." It may clearly be seen that this is not a research result and its purpose is to enable the reader to better understand the research described in the text.

MOVE 4: DESCRIPTION OF BACKGROUND RESEARCH - SCIENTIFIC

This is similar to the previous move in that it provides background information for the reader but it differs in that it relates specific research that forms a background to the new findings reported. It has a corollary in the research article, i.e. reviewing the literature, as described in Swales' Move 1, Step 3, or Nwogu's Move 2, Constituent Element 1 (Swales, 1991; Nwogu, 1997). Thus this move is classified as scientific rather than social. An example would be the following from Text 7: "Ever since Berkeley, Calif., geneticist Mary-Claire King showed in 1990 that familiar breast cancer is often tied to a particular gene, researchers all over the world have been racing to identify it". In contradistinction to Move 3, specific research, sometimes, as here, with date and researcher, is mentioned. Background research can also of course be identified as such not by the language but by the content of the research described. King's work in localising the breast cancer gene to 17q21 is well known to have spurred the race to specify the precise location and is referred to in all papers on the topic including that by Miki et al. which describes the discovery of *BRCA1* and which is the source for this popularization (Hall et al., 1990; Miki et al., 1994).

However, sometimes the text simply gives a general indication that it is relating findings based on past research without being specific about names and dates, e.g. Text 13, "... it is biologically plausible, scientists say. Laboratory experiments have shown...". The use of the simple perfect is another indication that relevant past research is being cited. One clear indication necessary of course is that the research referred to in this move must predate the research reported, a fact which is often indicated, as here, by the date being given. Roughly contemporaneous research would be classified as Move 7, Description of further findings.

MOVE 5: FURTHER DESCRIPTION OF FINDINGS - SCIENTIFIC

This move follows up Move 2, where the main research findings are announced. Therefore it must physically come later in the text than Move 2. More details are given of the same research which is introduced in Move 2, so where different, although perhaps related, research is described, this would come in Move 7. The move may follow closely on the heels of Move 2 and refer back to it specifically, as in Text 9, " This news comes from the University of California at San Francisco, where researchers studied AIDS patients with lymphoma, a cancer in which lymph cells grow wildly." More often it comes later in the article and there may be no specific mention of the previously mentioned study but the topic is clearly the same, as in Text 16, where paragraph 7 picks up the research introduced in the second paragraph and gives more information about *BRCA1*, e.g. "*BRCA1* is very large". This information is obviously about the research identified in Move 2 where the new discovery of this gene is mentioned.

MOVE 6: EVALUATION OF FINDINGS - SOCIAL

This move is not found in the majority of articles and is borderline social rather than scientific. It focuses on reaction to the findings described previously and discusses and evaluates them. This evaluation may be by the journalist herself, the author of the research or other scientists. When there is a direct or indirect quote from another scientist, the move would tend towards the scientific rather than social, e.g. Text 7, par. 2: "It is a tremendous advance". Otherwise the move can be classified as social as the evaluation relates to the society rather than the research, as in Text 5: "Doctors differ vehemently on whether the trends are all for the good."

The move differs from Move 9, Significance of findings for research, in not talking at all about the future. Instead it gives a pro- or anti- reaction and discusses the good or bad points. Thus it can also be negative, e.g. Text 8, paragraph 5: "Equally disturbing was the way some scientists knowledgeable about the case - and some government officials - kept quiet about Poisson's misdeeds for years."

MOVE 7: DESCRIPTION OF FURTHER FINDINGS - SCIENTIFIC

This move differs from Further description of findings, Move 5, in that it does not give more information about the same research but information about another research study, not the main one identified in Move 2. Many articles may report only one study and will thus not have this move, but longer articles will often have news from more than one study. Thus Text 14, par. 10, has "Last year, half a dozen laboratories...". Almost always this move will come later in the text than Move 5, but exceptionally it can come before it, as in Text 1. Sometimes it may be difficult to distinguish between Background research, Move 4, and this move. Thus Text 12, which is a kind of review article in RA terms, being a cover story going over a much wider spectrum of results than normal, has a lot of both Background research and Further findings. But they can be distinguished by, for instance, date. Thus Text 12, paragraph 7 has "A pivotal discovery came in 1976, when Drs. J. Michael Bishop and Harold Varnus ...", which dates it as background research. On the other hand, Text 12, paragraph 12's "Over the past 5 months, competing teams at ..." identifies it as Move 7, because of the current date. It can only be differentiated from Move 5, Further description of findings, by reference to the current text. This illustrates the way move identification is dependent on the discourse structure.

MOVE 8: EXPLANATION OF FINDINGS - SOCIAL

This differs from Description of background ideas, Move 3, which also has an explanatory function, because it specifically explains the findings of the new research reported, unlike 3. So in contrast to Move 3, it is always found after Move 2 and either Move 5 or Move 7, depending on which finding it is an explanation of. In one case, though, there is a Move 8 immediately following an unusually detailed Move 2. Background ideas, on the other hand, precedes a Move 2, or a Move 5/7, rather than follows it. Both are the result of the need on the part of the journalist to counteract a lack of understanding on the part of the reader, and are both, therefore, social moves. However, the function of Background ideas is to guard against an "I don't

understand" response by the reader, whereas Explanation of findings rather counteracts an "I don't understand *why*" response.

MOVE 9: SIGNIFICANCE OF FINDINGS FOR RESEARCH - SCIENTIFIC

Move 9 is a very significant and common move, often found in final position and it is aimed at explaining the importance of the research for future research. It is thus clearly a scientific rather than a social move. It thus differs from Move 10, Significance of findings for reader, which is oriented towards the reader. The difference between the two can be clearly seen when looking at the final paragraph of Text 17, paragraph 10: "Researchers hope that several large studies - including a National Cancer Institute project that plans to recruit 74,000 women - will help determine who should be screened and sharpen the interpretation of abnormal test results." Here the research vocabulary - "researchers", "studies", "project" and "National Cancer Institute" indicates the research focus. This is also indicated by the purpose, which is clearly a research purpose and not one of interest to the individual layperson. The next sentence then shows a switch to Move 10: "Until such research is completed, women concerned about ovarian cancer would do better to talk with their doctors about taking oral contraceptives - shown to reduce the risk of the disease - than to request a sonogram or CA 125 test." Here there is a clear switch to the interests of the patient.

MOVE 10: SIGNIFICANCE OF FINDINGS FOR READER - SOCIAL

As mentioned above, this move differs from Move 9 in being aimed at the reader as a layperson concerned about cancer rather than the research implications. This move is extremely common, being found in 14 articles, in 11 cases in final position. Either this move or Move 9 closes the article in all but one case in the corpus.

Two main submoves are found, as discussed below.

a) Direct advice to reader

As in the example discussed above under Move 9, this move is often expressed in terms of direct advice to the reader. This is the clearest form of the move and is very easy to recognise because of linguistic features like the "would be better" in the above example. Such changes of modality show the advice being given.

b) General importance

But the move need not be expressed in terms of direct advice. Rather it may be in the form of a general statement, where here it corresponds to submove b) of Move 1, talking in general terms about the importance of the findings to the reader. This can be seen, for example, in Text 3, paragraph 7, where we have "One of several new women's health studies funded by the National Institutes of Health will examine which fertility drugs are most hazardous and who are most at risk. For infertile women, that may eliminate at least one frightening uncertainty." This might seem from the first sentence to be Move 9, but the introductory prepositional phrase in the final sentence clearly indicates who the research is most relevant to.

Thus the moves can all be fairly clearly identified by a mixture of discourse, content and linguistic features. Where there may be confusion between two moves, clear demarcation criteria and ways of distinguishing between the two have been described. As in any field of scientific study, there will always be borderline cases, but the vast majority of sections of text are shown to fall clearly into one or other of the moves using the criteria listed above.

Establishment of move structure

Having looked in outline at how the moves are established, let us now examine one text in some detail as an example of how this works in practice. Since the first text is rather long and complicated we shall examine Text 2, "How safe is lumpectomy?", which is more typical and shorter in length (see Appendix B for the list of moves and

Appendix C for the text). This text has six moves in all, a normal sort of number for texts of one page in length.

The article starts, as is normal with Move 1, specifically submove 1b), General personal example. This can be seen quite clearly in the use of the generic 'a' used to introduce "a woman" in the first sentence. This generic `woman with cancer' is then referred to in the rest of the first paragraph as "she". The report of the findings, Move 2, then comes next in its natural place, identified by the use of vocabulary such as "reported" and "a major US study".

In third place we have a common phenomenon; instead of Move 3, there is a recycling of Move 1, but this time a different submove. Paragraph 2 is a description of a personal example, with the name of a particular patient who had the disease/condition/operation discussed, in this case a lumpectomy. The patient is then quoted at some length. It is noticeable here that the patient is named and quoted, so this is Move 1a), Personal example, but the patient actually talks in general terms using the generic 'you' pronoun, so this is also an echo of Move 1b).

The next paragraph introduces Move 4 in fourth place, with a description of the background research involved. The fact that it is related to the research already mentioned can be seen by the use of anaphoric 'the' at the beginning of the paragraph, in "The study that concerns Schneider". The nature of the research as background research, Move 4, can be seen from the name of the research project, which is given, and the dates involved. The project began in 1957 and the background facts about the research necessary to understand the current situation took place in 1990. There is a fairly detailed description of the background, more in fact than normal, because of the nature of the research reported, a suspected falsification of research data.

The switch from Move 4, Description of background research, to Move 5, Further description of findings, i.e. current research, is signalled by a switch in tense from past to present tense: "Dr. Bernard Fisher, a pioneering researcher in the biology and treatment of breast cancer who heads the NSABP, is satisfied that the survey's

results...". The indication that this is Further description of findings and not Move 7, Description of further findings, is that there is a reference to the same project, the NSABP. That it is a further description of the current situation can also be gleaned from the date given, "not until last July". That we are talking about current news is also indicated by the mention of the prospective publication of the re-analysis in the NEJM the following week.

The next paragraph goes on to a rather less common move, Move 8, Explanation of findings. We can identify this rather less clear cut move in a number of ways. Firstly, the paragraph opens with a reference to another group of people who have not figured up till now, medical ethicists. One of them, identified as a medical ethicist and with his name given, is then quoted directly. This move also shows how we must take account of the discourse context in deciding on the moves. The last part of the paragraph describes the importance of the original research, so this might seem to be a shift to Move 6, Evaluation of findings. However, this is an evaluation of the original study, not the new research just released, so it has a different communicative purpose from a real Move 6, which is to assess the relative importance of the new research reported. Here the purpose is to explain the surprise of the medical ethicists that the researcher should tamper with the data, as the original study was such a major one.

The final paragraph then shifts to the typical final move of such an article, Move 10, which shows the significance of the research described to the reader. Here the relevance to the reader, the reader most directly addressed by the article, the woman with breast cancer, or who fears that she may have/may develop breast cancer, is clear. The uncovering of the fraud discussed in the article makes the safety of the surgical intervention involved, lumpectomy, questionable. The final paragraph, therefore, indicates to the reader the current evaluation of the risk involved, that lumpectomy is probably safe, but not definitely so, thus answering the question which is the title of the article.

We have seen, therefore, that we can identify the moves in the text by looking closely at the language of the text. Nevertheless, this cannot be a routine process which could

be automatically carried out by a computer in the style of Biber, or Henry and Roseberry, for example (Biber, 1988, 1995; Henry & Roseberry, 1996). In the latter's approach to identifying moves, these are established by collocations of lexical items found by a concordancing program. However, as noted above, we have to take into account the context of the article and the communicative function of the part of the article involved. Thus the evaluation of a study is not Move 6, because it is not the current study being evaluated, but Move 8, because the aim of the evaluation is to explain to readers something about the experts' opinion. Hence the identification of moves uses language to help explain the communicative function, but it is the communicative function that is uppermost. This is in line with Swales and Bhatia's position and that of Dudley-Evans (1993), where he talks about the purpose of the writer. Where my approach differs, though, is that I have tried to specify much more exactly precisely how the moves may be identified and have laid out above here a detailed description of how this was done in practice.

Presence and order of moves

Having ascertained what the moves and the submoves are, the next question that arises is one that was identified above as somewhat problematic for genre analysis. This is the problem of the order of moves. In fact, there are two interrelated problems. To what extent is any one move obligatory and what position in the text does that move occupy?

We have seen that Swales was not too concerned with this aspect of genre analysis because the move structure he identified had only three moves and therefore each move was obligatory and came in a fixed order. It might be argued, though, that this was an artefact of the type of genre analysed by Swales since a 3-move structure, being so simple, is likely to be rather invariant. Any structure with a larger number of moves is likely, on purely mathematical grounds, to have more structural variety. This seems to have been borne out in practice by those researchers who have looked at more complicated sections of the RA than the introduction (Brett, 1994; Dudley-Evans, 1994).

Within a Hallidayan perspective, Hasan (1994) distinguishes between obligatory and optional elements. In addition, she discusses the different possible orders of determinate and variable elements and whether an element occurs iteratively or not. We can see, therefore, that the idea of a move as an absolutely necessary element which always occurs just once and in a particular position in the text is something of an oversimplification. Although Hasan's ideas are helpful, she is still somewhat overprescriptive. Thus she claims that optional elements are irrelevant to the identification of the genre. Here we run up once more against the problem of to what extent moves are obligatory. As noted above, Swales, followed by Paltridge (1995b), does not consider that a move need be obligatory, since in discourse analysis, unlike syntax, we are unlikely to find consistently 100% compliance with the discourse analyst's description. Similarly, Flowerdew notes that moves, (or structural elements, as he puts it) "may be omitted, repeated, occur in a different order, or be embedded within one another." (Flowerdew, 1993, p. 308.) Looked at from a different perspective, of course, embedding is another type of cyclicity, which is the way it has been analysed in the present corpus. Put another way, Thomas (1995) argues that in pragmatics we are concerned with principles, not rules, and that principles are not found to be in operation in every case. For their part, Bloor and Pindi question the discrete nature of moves, noting the "subtle interweaving [of moves] that occurs in most texts" (Bloor & Pindi, 1990, p. 59).

Turning to the description of moves in popularizations, then, we find that some moves do not occur 100% of the time, i.e. in all texts which are taken to be examples of the genre. This is to be expected, in that any model is an idealisation of the data and to be generalisable at all must abstract away from the specific data on which it is based. Thus Nwogu in his article on popularized medical texts established a move structure of 9 moves (1991). But none of his 15 texts seems to have all these moves, since he states that the average number of moves was 6, but one of the texts had only 4 moves and another had 7. This would seem to indicate a minimum of 4, a maximum of 7, and an average of 6 moves. In his later analysis of medical research papers, where we might expect a stricter move structure, he established an 11-move structure, but articles 1, 6 and 8 had only 7, 9 and 6 moves, respectively (Nwogu, 1997). Thus

Nwogu consistently has less than the total number of moves theoretically expected actually occurring in any particular text, especially in the popularized medical texts of the kind we are examining.

As regards the order of the moves, an indication that the rather strict order postulated for the medical research paper might be somewhat overidealised is the admission that Move 8 is "highly flexible" in its positioning (Nwogu, 1997, p. 131). The popularized medical article seems not to have a very strict order at all, since Nwogu states that only the first two and the last two moves seem to come consistently in that position. The other moves, the medial moves which make up the bulk of the moves in the text, seem to be rather variable in position (Nwogu, 1991).

A related issue, raised above, which is not covered at all by Nwogu, is that of cyclicity. In other words it is not at all obvious whether a move occurs only once or more than once in a text. What we find in Nwogu's Table 2, *The Distribution of Moves per Text*, is only whether a move occurred or did not occur in a particular text. There is no indication as to whether a move occurred more than once or not. Dudley-Evans, in his work, however, stresses the prevalence of cyclicity and it seems clear that certain moves can reoccur several times in a particular genre (Dudley-Evans, 1994; Hopkins & Dudley-Evans, 1988).

It would seem then that we should not expect any actually occurring texts necessarily to have the full complement of moves found in the move structure set up for the popularized medical text and that the order involved is highly variable. These findings by Nwogu are an indication that we are unlikely to find that all the texts in our sample have all the moves which we have set up or that a strictly invariant order is likely to be found.

With this caveat, let us examine the actually occurring move structure in the texts analysed. In order to do so a further distinction regarding recurring moves must be brought in at this point. Although Nwogu only gives an indication of whether a move occurs or does not occur, to give a full picture of the actual situation in a text, we need

to show whether a move occurs once or more than once. Thus an actual text might have, for example, twenty moves in it, although I have identified only ten kinds of moves, since occurrence of a move may be cyclic, i.e. a move may occur more than once. It is necessary, therefore, to distinguish between a realisation of a move and the move itself. For example, Move 1 may be realised 3 times, often in the form of different submoves, as it is recycled through the text. I shall distinguish, therefore, between actual moves, the tokens, and the move type, the type involved, so that, for instance, in a text Actual Moves 1, 2 and 3 may be Move Types 1, 2 and 1, as the first move type recurs.

The results of the examination are set out in Appendix B, where a detailed list of the actual moves and the move types found in every article analysed, is given. More useful for analytical purposes is the restructuring of the raw data from Appendix B, shown in Table 2.2, *Actual moves and move types*, shown on the next page.

Here we can see the real order in which the moves occur in each text. Down the left-hand side we can see the actual moves, beginning with 1 and going as far as the total number of actual moves in the text. Across the top is the number of the text. In the grid itself we have the move types, as listed in Table 2.1, p. 34. Thus, for example the second actual move in text 1 is Move type 3, Description of background ideas.

If we examine, therefore, the occurrence of move types as compared to the ideal that is laid down in Table 2.1, we can see that in actual practice, as in Nwogu's case, the move structure found in a typical text is not that of the theoretically ideal move structure. There are a number of reasons for this. The most important is that the texts vary greatly in length, so that the shortest text does not have space for all ten moves, while the longest text has space for a great many more. Thus Text 4, which has only two paragraphs, could not possibly fit in all 10 moves, for example.

Text	T1	T2	T3	T4	T5	T6	T7	T8	T9	T10	T11	T12	T13	T14	T15	T16	T17	T18	T19	T20
Move 1	1	1	1	1	1	1	1	4	3	4	1	3	1	4	2	2	1	4	1	1
Move 2	3	2	2	2	3	2	4	2	2	2	2	1	2	2	7	9	2	2	2	2
Move 3	2	1	1	10	1	10	6	1	5	1	1	4	1	9	9	10	5	8	6	10
Move 4	1	4	4		3	5	2	4	9	10	5	2	3	3	4	1	10	1	7	4
Move 5	7	5	5		4	8	6	10	5	5	3	4	4	4	5	3	9	10	1	1
Move 6	5	6	1		1	10	10	1	3	1	1	3	3	9	9	5	10	4	6	7
Move 7	4	10	10		2		7	6	5	10	6	1	5	7		9		1	1	1
Move 8	1		1		10		9	5	9	9	1	4	1	9		10		5	6	7
Move 9	7		10		6		10	9			6	7	6	7		3		10	10	4
Move 10	1		9		5			8			5	1	1	9		10		9		7
Move 11	7		10		6			5			1	8	9			9		5		1
Move 12	3				9			9			3	3						10		7
Move 13	7				7			10			5	7								10
Move 14	10				10			6			8	8								9
Move 15	7							10			1	7								10
Move 16	10										9	3								3
Move 17	6											7								10
Move 18	10											3								1
Move 19												7								10
Move 20												3								1
Move 21												7								10
Move 22												3								1
Move 23												4								10
Move 24												5								1
Move 25												3								10
Move 26												4								1
Move 27												3								
Move 28												9								
Move 29												3								
Move 30												5								
Move 31												9								
Move 32												4								
Move 33												9								

Table 2.2. Actual moves and move types

Given the overall argument, however, we should expect that even in the shortest text there would be a mixture of both social and scientific moves and that the basic social move, Move 1, would appear, as well as the basic scientific move, Move 2. The reason for calling these moves basic moves is, first of all, that the article could not very well be written without reporting the research involved, which is Move 2, Description of main research, and that it would not be a typical popular science article if it did not involve Move 1, Relating to the reader. Consistent with this, Text 4 has both of these moves, in the appropriate order, as Move 1 and 2. The third, ending, move is not Move 3, which would not be needed in such a short text, but the typical ending move, Move 10, Significance of findings for reader.

Only two texts do not have Move 1 in them, indicating the importance of Move 1 to the genre. Of these two, one, Text 15, actually has a number of Move 1's, but in the separate boxed text which is included in the article. The function of relating the research to the reader, therefore, has been delegated to the box rather than the main article. The other article without a Move 1 is Text 9, which is a rather different text in that it relates cancer to research on another disease, AIDS. Perhaps it is felt here that because of the prominence of AIDS in society that this is sufficient in itself to create a Move 1, but certainly this text is an exception. However, 90% of the texts do have a Move 1, which is quite sufficient to establish it as a standard move by any normal criterion (cf. Swales 1990, cited above on this).

As might be expected, Move 2 occurs in every text, as without Move 2 the article would not be reporting research. These two moves, 1 and 2, typically occur at the beginning of the text. The corresponding closing moves, 9 and 10, typically occur at the end of texts, so that every text but one ends with either Move 9 or 10. The only text which closes with another move actually has a recurring Move 10-Move 1 structure which occurs 5 times, so that the text has Move 10 as a penultimate move. Of the other texts, 11 end with Move 10, 8 with Move 9. Thus the texts end with either a social or a scientific closing move. Both of these moves occur in the majority of texts – 14 each.

The basic moves, therefore, seem to be Move 1 and Move 2, coupled with either Move 9 or Move 10. Other moves are not quite so common, though several occur in the majority of the texts. Thus Move 5 occurs in 15 texts, i.e. 75%, and Move 4 in almost as many, 13, or 65%. Only two moves occur in less than half the texts and can truly be called optional moves – six and eight. Normally, however, several of the 'central' moves occur in the middle part of the text if the text is of any length. Move 10, as well as occurring at the end of most texts, also occurs earlier in many cases.

These popular science texts, then, seem to have a less fixed move structure than those typically found in academic texts. But this seems generally to be more typical of non-academic texts. Thus in their analysis of what they called "brief tourist information" texts, Henry and Roseberry also found that many moves could occur and reoccur at many different places in the text (Henry & Roseberry, 1996). In fact, we would not expect that non-academic texts should have the fixed structure of an academic text where the move structure, at least in broad terms, is often fixed by the journal editor. It is only to be expected that in a genre where the communicative purpose is more open-ended there will be a greater flexibility in the moves employed. Thus we have seen that in Nwogu's popular medical science corpus also it was only the first two and last two moves that seemed to be at all common and that the middle moves were often missing and could occur in virtually any order (Nwogu, 1991).

In my corpus also it is the first two and last two moves that seem more fixed. However, it is difficult to see what in fact the middle moves do in terms of their order. What clouds the picture is the fact that there is a considerable amount of cyclicity. For example, the typical actual third move in a text is not Move 3, Description of background ideas, but a recurrence of Move 1, Relating to reader. Move 1 recurring again is actually the move most commonly found in position 3. Typically, it follows a Move 2 in its normal position and the probable reason for its reoccurrence is the need by the author to reassure the reader of the relevance of the information to him immediately after the introduction of the scientific Move 2, which introduces the research. In fact, in no Actual Move 3 does Description of background ideas occur.

In several texts we find quite a consistent cycling pattern. Thus, in the major cover story, Text 12, which is the longest text in the corpus and which is almost like a review article in its coverage of the history of research on cancer genetics, there is a consistent pattern for several paragraphs, from the beginning of paragraph 16 (Actual Move 16), labelled "Yearnings for immortality", through to paragraph 22, labelled "Pulling up stakes". This is a Move 3/Move 7 pattern, which recurs three times. The journalist here gives the background ideas necessary to understand the new research reported prior to detailing the new findings. Hence, for instance, before mentioning Harley's research on telomerase, she first explains about telomeres, information necessary for the understanding of Harley's research. Another long article, Text 20, also has a recurring pattern. Towards the end of the article there is a cycle of describing the social situation regarding genetic testing and indicating the significance this has for the reader. This type of pattern is a bit unusual, but arises because of the focus of the article on tests for cancer genes and people's possible reactions to their availability.

This cyclic pattern of recurring moves, then, might tend to obscure the pattern of the move structure. If there is a genuine pattern, though, this would be seen from the structure of the moves in terms of their move numbers. The moves start from Move 1, at the beginning of the text and progress in order in the ideal text through to Move 10 at the end. But recurrence of a move, together with omission of less common moves, means that this progression will not be a smooth one. The progression from a lower number to a higher will be broken when there is recursion of an earlier move. Nevertheless, if there is a general progression from lower to higher moves, after the recurring move the trend towards a higher number would recur.

In Table 2.3, *Occurrence of higher and lower moves*, we can see the analysis of the move progression. What I have done here is to test the hypothesis that there is a progression of moves through the texts. If there is such a progression, there will be a tendency for a later move to be higher in number than the one immediately preceding. Even if this trend is broken by a recycled earlier move, then the trend will reassert itself.

	T1	T2	T3	T4	T5	T6	T7	T8	T9	T10	T11	T12	T13	T14	T15	T16	T17	T18	T19	T20
Move 1																				
Move 2	H	H	H	H	H	H	H	L	L	L	H	L	H	L	H	H	H	L	H	H
Move 3	L	L	L	H	L	H	H	L	H	L	L	H	L	H	H	H	H	H	H	H
Move 4	L	H	H		H	L	L	H	H	H	H	L	H	L	L	L	H	L	H	L
Move 5	H	H	H		H	H	H	H	L	L	L	H	H	H	H	H	L	H	L	L
Move 6	L	H	L		L	H	H	L	L	L	L	L	L	H	H	H	H	L	H	H
Move 7	L	H	H		H		L	H	H	H	H	L	H	L		H		L	L	L
Move 8	L		L		H		H	L	H	L	L	H	L	H		H		H	H	H
Move 9	H		H		L		H	H			H	H	H	L		L		H	H	L
Move 10	L		L		L			L			L	L	L	H		H		L		H
Move 11	H		H		H			L			L	H	H			L		L		L
Move 12	L				H			H			H	L						H		H
Move 13	H				L			H			H	H								H
Move 14	H				H			L			H	H								L
Move 15	L							H			L	L								H
Move 16	H										H	L								L
Move 17	L											H								H
Move 18	H											L								L
Move 19												H								H
Move 20												L								L
Move 21												H								H
Move 22												L								L
Move 23												H								H
Move 24												H								L
Move 25												L								H
Move 26												H								L
Move 27												L								
Move 28												H								
Move 29												L								
Move 30												H								
Move 31												H								
Move 32												L								
Move 33												H								

Table 2.3 Occurrence of higher and lower moves

Popular medical texts of this type, then, seem to differ from academic texts in that they have a fairly free move order, with only typical beginning and ending moves, rather than a strict pattern throughout the text.

Moves in the RA and popularization compared

The above finding on move order is in agreement with Nwogu (1991, 1997). In fact, though, Nwogu claims that his Medical Research Paper (1997) has a somewhat tighter structure than his Popularized Medical Text (1991). On closer examination, however, the distinction between the two that Nwogu seems to be making breaks down. If we look at the names and functions of the moves in the research (R) and popularization (P) texts and list the research moves in the original order with the equivalent popularization moves opposite, we find they are practically the same (see Table 2.4 below).

Thus we find, for example, Move 2, Reviewing Related Research, in the RA becomes Move 3, Reviewing Related Research, in the popularization. Occasionally, there is a slight change of nomenclature, so that Move 10 in the RA, Explaining Specific Research Outcome, becomes Move 8, Explaining Research Outcome, in the popularization. The only two moves in the RA which have no equivalent in the popularization are Moves 6 and 8, which are optional moves in any case. Granted that some moves appear in different positions in the popularization and the research article, nevertheless the moves themselves are basically the same. There is a difference in order, but in fact Nwogu states, as we have seen, that normally only Moves 1 and 2 occur at the beginning and 8 and 9 at the end in the popularization. Moves 3-7 do not necessarily occur in that order. The only really significant difference then between the popularized medical article and the medical research article is the position of Move 2 in the popularization, which is equivalent to Move 9 in the RA. In fact, as Swales (1990) has shown, a short mention of the main finding, i.e. Move 2/9, usually comes in the introduction of the RA, as Move 3, Step 2, so in the same sort of place as Move 2 in the popularization.

R	P
1	1
2	3
3	4
4	6
5	7
6	
7	5
8	
9	2
10	8
11	9

Table 2.4. Corresponding moves in Nwogu's research and popularization texts

Nwogu, therefore, basically seems to find little difference between the popularization and the research article. The popularization still focuses on presenting the new research, relating it to previous research, describing how the findings were obtained etc. The popularized medical article is seen as a simplified, toned-down version of the research article. Let us examine just what sort of implicit claim is being made here: the moves of the research article and the popularization are at bottom the same, except for the fact that the moves occur in a slightly different order.

If we consider a genre as being constituted by the communicative purpose, then it would seem very strange that a research article and a popularization have the same communicative purpose. The communicative purpose of the research article is to announce new research findings to the research community and, following Bazerman (1988), Myers (1990) and Skelton (1997), among others, to have these findings accepted by fellow researchers and incorporated into the body of accredited scientific knowledge. The readership of a popularization is not the research community but the public at large, so it would at the very least be strange if the aim of the communication

was to convince the research community. This would be even stranger when the writer of the article is a science journalist rather than a researcher.

Thus it appears highly unlikely that Nwogu is right in claiming that the research article and the popularization have virtually the same moves with only a slight change in the order. Our results indicate that there are several moves which are quite specific to the popularization, which grow out of the popularization's quite different communicative purpose and different audience. These 'social' moves have no counterpart in the research article since they are not needed when a researcher is communicating to fellow researchers.

The claim being made here is that, contrary to Nwogu, there are significant differences between the genre medical research article and the genre popularized medical news article, to use Nwogu's nomenclature, or more broadly between RA's and news popularizations. The aim of the research article, as Myers (1990, 1994) has shown in his concept of the `narrative of science', is to present new science as a contribution to the progress of the scientific discipline involved, to have that contribution accepted by the scientific community of the writer's peers and to be acknowledged as the originator of that knowledge in future. None of this applies to the popularization. What the popularization aims for is to present the new science involved to the reader of the news magazine and demonstrate that the reader should pay attention to this news by showing the social relevance of the news to the reader.

Move and genre defined

It would seem from the above, therefore, that the move structure of popular scientific texts is not so fixed in terms of move order as academic texts. However, the fairly strict criteria laid down for the identification of moves and the fact that the moves identified clearly did turn up in the corpus means that the idea of moves and move structure is basically sound and serves to identify key features of such texts. What is problematic about move structure in such texts is the idea that there must be a determinate fixed order for these moves. Other attempts to find a rhetorical structure

in news texts, such as those by van Dijk (1988) or Bell (1991), have also failed to find a fixed order of steps. Thus Bell uses the structural unit of event, which may recur many times in a news item. There is no sense here of one event having to precede another as in move structure. Within each event, too, certain categories, such as evaluation or attribution, may occur, but need not. Similarly, van Dijk talks of elements of news story structure which may or may not occur. There is thus a rather free and unfixed structure with certain elements sometimes but by no means always occurring.

This agrees with the findings here that news magazine popular biomedical science stories do not have a rigid structure. It seems, then, that we are on fairly firm ground in stating that such articles do not have a move structure in the same sense as that introduced by Swales (1981) where there is a fixed sequence of moves. But the concept of move need not be tied down to the idea of sequence and we have already mentioned that Swales was not so concerned with sequence and why not. The concept of move retains its value provided it is characterised, as has been done here, in such a way that the moves can be clearly identified and have a clear rationale in terms of the communicative function of the text.

The idea of communicative purpose is, as we have seen above, tied in the Swales model to the related idea of discourse community and we noted the potential problem in saying that genre and discourse community are each defined (partially) in terms of the other. Thus a genre has a "parent discourse community" (Swales, 1990, p. 58) as part of its definition. There is a problem, though, it seems, in saying that a discourse community has in its turn as one of its defining characteristics "mechanisms of intercommunication among its members" (Swales, 1990, p. 25). Clearly the writers of research articles of the type Swales investigated are also the readers of other such articles and other readers are at least potential writers of such articles. However, the readers of popular science news articles of the type examined here are quite distinct from the writers of such articles and have no knowledge of or contact with each other as readers of such articles. They thus are an example of the 'Cafe Owner Problem' (Swales, 1990, p. 25), i.e. a discourse community cannot have as members people, like

like cafe owners, or in our case news magazine readers, who do not communicate with each other. Yet, clearly, Swales intends that discourse types like newspaper articles be seen as examples of genres and indeed specifically mentions news items as a genre (Swales, 1990, p. 26). Similarly, Bhatia (1993) discusses the genre of the sales promotion letter, where the recipient of the letter may not even *want* to be a member of that particular discourse community; yet, he, nevertheless, considers this a genre. It seems, therefore, that discourse community is not criterial for a mass genre like this, or we must define discourse community in such a way that most members of the community do not communicate with each other and have no knowledge of each other's existence.

We are thus thrown back on the idea of communicative purpose as specific to a particular genre. But without something more specific as a definition of genre then communicative purpose is rather hard to pin down. What is missing here it seems is to make the concept of move, which is a distinctive feature of genre analysis, definitive of genre. At present it is an undefined, underdetermined characteristic which is present in all discussions of genre but which seems to have no theoretical status in the definition of genre.

What, therefore, is distinctive about the idea of move? As we have seen, even researchers like Swales and Bhatia take the move as a given and do not attempt to define it, rather assuming that it is obvious what a move is. Although it would seem to have its roots in the early work on classroom interaction by Sinclair and Coulthard (1975), Swales (1990) does not refer to this work. Swales (1981) simply talks of sections of text with a particular function. Dudley-Evans (1993, p. 136) refers to "writers' different purposes", in a section devoted to move analysis. The name 'move' itself suggests an intention to move the argument of the text forward. We have seen above that it is also related to the acts of ethnographic analysis.

A related problem is the unit of text that a move stretches over. Is it the sentence, the paragraph or something longer? Dudley-Evans (1993) states that it is typically the paragraph but can be smaller than that. In the corpus examined here, the move can vary from smaller than a paragraph to several paragraphs in length. Since the move is

a discourse unit it is unlikely to bear a one-to-one correspondence to a syntactic unit like the sentence or a typographic unit like the paragraph. To the extent that a paragraph is also a discourse unit, though, it may correspond to a paragraph in many texts. Nevertheless, it would be better, given this range, not to define the move in terms of any unit.

Bearing in mind, though, the agreed concept of communicative purpose, we can try and define the move in those terms. The move may be said to be *a rhetorical strategy which has the purpose of advancing the argument of the text in which it is found in a way which is distinctive for texts of that type and which is typically found in texts of that type. There is a limited number of specific moves which are constitutive of any text type.*

This definition avoids two things. First of all it avoids specifying the position of a move in a text, since, as we have seen, moves may reoccur at different points in a text. This is not to say that moves will not often occur in a certain order. Clearly, they often do, but it would be too strong a claim to say that moves always occurred in a particular order. But it is not necessary to make such a claim to ensure that those moves which occur at a certain point in the text appear in that position. Those moves which must occur in a certain position because of the nature of the move will do so because of their specific purpose of "advancing the argument of the text in a way which is distinctive for texts of that type". Thus Move 5, Further description of findings, will automatically come later in the text than Move 2, Description of main research, because of the intrinsic nature of the moves. Where there is not such a necessary sequential relationship, for example with Move 10, Significance of findings for reader, which can occur at different points in the text, it would be a mistake to specify a position. Move order, therefore, falls out as a consequence of the communicative function of each move as part of the genre and does not need to be specified.

Secondly, the definition refers to text type rather than genre, as otherwise defining genre in terms of move would lead to the kind of circularity we saw with genre and

discourse community. In addition, defining move in terms of the text and making it distinctive for that type of text helps to distinguish it from ideas such as speech act, which otherwise might fall under the same definition. Thus a speech act such as promising is not specific to a particular kind of text, but may turn up in many different kinds of texts and genres, whereas this is not true of move.

It might be objected that we need a further level of analysis intermediate between move and genre, or perhaps a level below genre, as in the progression from act to lesson found by Sinclair and Coulthard (1975). However, because several levels were found in the specific context of the classroom does not mean that this transfers to the written text. In addition, it is clear that Swales (1990) implicitly recognises that genres have sections, since he talks about the genre of the RA but his own work has been mainly on RA *introductions*, not the structure of the whole RA. This is a further advantage of specifying text type rather than genre in the definition, as moves may go together to make up a section of a genre and then the sections go together to form the genre.

Having defined move, we are now in a position to attempt a definition of genre in terms of move rather than discourse community. *A genre is a text type capable of standing alone which is characterized by a limited number of specific moves which act together in order to achieve its communicative purpose.*

The advantage of such a pair of definitions is that first of all they specify the key term in genre analysis, i.e. move, which has not been defined hitherto. Secondly defining genre in terms of move helps to constrain what may be termed a genre and distinguish genres both from each other and from other related concepts such as register. To distinguish full genres from sections of genres like the introduction to the RA, a genre is specified as "capable of standing alone". Communicative purpose by itself is insufficient to define a genre since at some level many different types of text have the same communicative purpose. Thus the communicative purpose of an advertisement in the newspaper and a commercial on television may in both cases be to induce me to part with my hard-earned cash in exchange for the same goods on offer. Yet the two

genres are different in medium and in the moves they utilise. The newspaper advertisement may be simply a picture of a bottle of Pepsi and a text that a bottle of Pepsi is now reduced to $x. The tv commercial may be a melange of MTV-style video shots following each other in quick succession etc. The communicative purpose is nevertheless the same. Even if we choose the same medium, rather than newspaper and television, it is clear that the same communicative purpose can be achieved via different genres: the editorial and the guest article in *The Times* at election time may both have the communicative purpose of inducing the reader to vote Conservative, yet each would normally be considered a different genre.

Why is it that we consider the editorial and the guest article as constituting different genres? Surely it is because the move structure of each genre is different. We recognise different genres not only by the communicative purpose but also by the way this communicative purpose is instantiated, i.e. by the moves involved. This is recognised implicitly in the attention paid to move analysis within genre analysis, but it has not been recognised at the level of theory. It would seem to make sense, therefore, if the theoretical definition of genre paid attention to what actually happens in genre analysis and sets up the move as a defining element in genre analysis.

Having established, therefore, just what the move structure of the popular science article consists of and that it differs considerably from the RA, the question naturally arises as to whether the two differ only in their move structure. Since move structure follows from communicative purpose, it would seem likely that in fact the two genres would differ in more than move structure. In the following chapter, therefore, we shall examine in more detail the differences between RA and popularization by comparing some sample popularizations and the RA's they are based on.

CHAPTER 3

A COMPARISON OF RESEARCH ARTICLES AND POPULARIZATIONS

We have seen in the last chapter that popular biomedical science articles have a clear move structure and constitute a separate genre, different from the research article. This difference is a reflection of both the distinct target audiences for the two genres and also the different purposes involved. The popular science article in a general news magazine is concerned to bring news to a mass audience and aims to make a connection with the audience so that the news reported is perceived as being relevant to the interests of that audience. Thus the popularization has a series of moves which I have labelled 'social' whose aim is to make that connection with the audience and relate the science to the interests of the wider society outside the research community concerned with the latest advances in, in this case, cancer research.

If, then, there is such a clear generic difference as claimed between the research article and the popularization we might expect to see some distinct differences in the way the research is reported as between the original research article and the popularization in terms other than generic. Thus the focus of the popularization is likely to be rather different if the aim is to relate research to the interests of the reader and thus the wider society rather than present new research findings to the research community. To investigate whether such differences do in fact exist, this chapter examines in some detail several popularizations and compares them with the research article originally reporting the research.

That there are differences between popular science and the research science article can be seen from the very fact that we have the label 'popular science' itself. Recognition of a genre by society very often results in that genre being given a label (Swales, 1990). We have seen above that Nwogu sees little difference between the science research article and the popularised medical article in that almost all the moves in the RA are also found in the popularization (Nwogu, 1991, 1997). However, researchers working within frameworks other than genre analysis have pointed out several

differences between the popular science article and the research article. The first such study looking at a number of "paired communications that cover similar subjects but are addressed to audiences with different levels of background information and different degrees of interest" was that by Fahnestock (Fahnestock, 1986, p. 277). Working within a classical rhetorical model, Fahnestock notes how the popularization must accommodate to the different audience so that the basic appeal is different. Whereas the original scientific article is "forensic" , concerned with establishing the findings' validity, the popularization is "epideictic", aimed at celebration rather than validation. There are two basic subtypes of appeal to a wide audience – the "wonder" appeal and the "application" appeal. The first of these talks in terms of either the wonders of nature or the breakthrough character of scientific research, while the latter is concerned with the practical spinoffs.

Fahnestock here puts her finger on a clear difference between research article and popularization. Since there is a change in rhetorical situation, there is likely to be a change in genre. That popular science journalism often lays an emphasis on the "wonder" appeal is also recognised by Jerome (1986), who refers to what he calls "gee-whiz journalism", i.e. the oversensationalised reporting of new scientific discoveries. Rowan (1989), who also compares research articles and popular science texts in the style of Fahnestock, emphasises that while professional scientists are most concerned with giving evidence for their claims, popular writers are concerned with the novelty of those claims. An even stronger view of the changes that science research undergoes when it is popularized is referred to by Green (1985), who talks in terms of the science becoming degraded and distorted in the popularization process in the typical researcher's view. This view is echoed by Hilgartner (1990) who sees the "culturally dominant view" of science popularization as being a two-stage process, where at first scientists discover genuine scientific knowledge which is then disseminated to the public by popularisers with any differences being a matter of distortion or degradation.

Thus there seems to be a clear consensus that popularizations do differ from the original research and that they may change, sensationalise, even distort the science in

the process. However, as Hilgartner points out, such distortion is rather a perception by research scientists themselves, and is used by them to decide what constitutes 'real science'.

A more developed model of popularization in discourse terms has been put forward by Myers (1990, 1994), who sees research science as being concerned with the "narrative of science" and the popularization with the "narrative of nature". Myers establishes this distinction by comparing three research articles on biology with the corresponding popularizations in *Scientific American* or *New Scientist* by the same scientists. He convincingly shows by a process of detailed textual comparison that the research article is concerned with establishing the new knowledge claims of the scientists concerned and fitting those claims into the existing scheme of scientific knowledge. The aim is to have those claims accepted by the scientific community and the researchers' picture of events acknowledged as genuine by other scientists (Myers, 1990). The popularization, on the other hand, is not concerned at all with the existing scientific argument, but rather with describing the "facts" of nature.

However, Myers himself admits that the kind of popularizations he examined, in *Scientific American* and *New Scientist*, are atypical in that the audiences for these journals are made up largely of those already interested in science, in many cases themselves professionally concerned with science. He suggests that popularizations in more general news magazines like *Newsweek* need to be examined to see whether they also conform to this "narrative of nature" (Myers, 1990, p. 185). In the later paper cited above (Myers, 1994), he also looks at an article in the *New York Times*, but does not look at general news magazines.

In his comments on Tarone et al.'s paper, Swales (1985) also argues that there are likely to be several subtypes of popularization and that these different types should be investigated. Below I intend to follow Swales' and specifically Myers' suggestion. Since Myers has already established the concept of the narrative of science in research articles, I shall not be concerned with this aspect, but more with the popularizations

and in what respect they differ from the RA's. We shall see that, as indicated above, they are concerned rather more with society than with nature.

The main reason for this emphasis no doubt will lie, as Myers suggested, in the nature of the audience. We can extend Myers' argument quite easily in the desired direction. Thus the audience for the science research article is other professional scientists in the same research field. The research article is concerned with carrying on the narrative of science and establishing the claims of the researchers. For a journal like *Scientific American* or *New Scientist*, however, the audience is made up of scientists mainly in other fields and those generally interested in science. What this type of popularization is about, therefore, is the description of generally interesting new discoveries, the narrative of nature. With the mass audience popularization, however, such an interest cannot be assumed. As pointed out by Dunwoody, whereas the science research article is required reading for scientists in the same field, "the mass media audience must be lured into the story" (Dunwoody, 1986, p. 12). The audience for these three types of article, therefore, can be seen in terms of three concentric circles, with the inner circle of research scientists in a particular field surrounded by the wider mass of those interested in science, themselves surrounded in their turn by the mass public not particularly interested in science *per se*.

In the following section of this chapter, therefore, we shall examine in more detail the hypothesis that popular science news articles of the type we are examining do indeed differ from the type examined by Myers. More particularly, we shall examine whether they are concerned to report the research in a way that intersects with the concerns of the wider society, the narrative of society hypothesis introduced with the social moves in the previous chapter. In contrast to chapter 2, where the overall generic structure of these articles was examined, in this chapter we shall follow Myers' methodology and compare in some detail the popularization and the original science research article it is based on.

It is of course, impossible, in terms of space and time to examine in detail all twenty popularizations in the corpus, quite aside from the fact that it is not possible, unlike

with an RA, to determine in every case precisely the exact source of the article, as such articles are not concerned with allowing the audience to check sources. The articles chosen for examination were chosen on the basis of a number of criteria. The first criterion was that the source could be clearly established. Secondly, those articles in the corpus which reported the same topic but were in different magazines were chosen, as these allowed a comparison between different accounts of the same research. Finally, coverage of all three news magazines was assured.

Let us compare, then the popularizations with the corresponding research articles. The discussion takes the popularization as starting point and relates it to the original RA, so that all quotations from the popularization refer to the particular text under discussion, unless otherwise stated. The popularizations are included in Appendix C, while the original RA's referred to are shown in Appendix E, and the list of RA texts in Appendix D. We start with two articles on the link between abortion and the incidence of breast cancer, in *Time* and *US News & World Report* (henceforth *USN&WR*).

Comparison of popularizations and research articles

Comparison 1

Text 11

DO ABORTIONS RAISE THE RISK OF BREAST CANCER?

Gorman, C.

***Time*, 7th November 1994**

This *Time* article is squarely based on one major study cited by author and source in the article itself (Daling et al., 1994). The story is also picked up in the article '*Linking abortion and breast cancer*' in the *US News and World Report* of the same date (Rubin, 1994, Text 18).

The subject talked about, a purported increased risk of breast cancer among women who have had an abortion, is a very controversial one with obvious links between the

science involved and wider societal issues, which are in fact brought out in the article itself. For this reason it is a classic case of the social aspect of science reporting which I argue is the typical genre of science writing in such news magazines. Because of the controversial nature of the science involved, however, the journalist is faced with the problem of how to report the science while not at the same time alienating half her readers, split as they will be into pro-life and pro-choice camps on the abortion issue. The analysis of this story, therefore, will indicate how science reporting is not just about reporting science, but about relating that science to the wider society. When that society is split over an issue, as here, this need to take the wider society into consideration when writing about science is, therefore, brought into clearer focus.

The initial paragraph of the story overtly notes the relevance of the science research article to the wider society in four different ways. First it contrasts it with the normal lack of relevance of such articles, secondly it does so by noting the controversial findings and thirdly by noting how the research had already been noticed by groups in society engaged with the issue. Finally it tells how the story was picked up ahead of publication by the *Seattle Post-Intelligencer*. Thus the importance of the story to the wider society is heavily stressed in the opening paragraph, using examples of Move 1d).

Turning to the details of the findings as reported by the journalist, Christine Gorman, she states in reference to the original RA, (Daling et al., 1994), that "The paper asserted that having an abortion raised a woman's risk of contracting breast cancer 50% on average". This is a fairly typical example of the popularization of a precise scientific style of giving data in an `educated colloquial' equivalent. Thus the original formulation was: "Among women who had been pregnant at least once, a history of induced abortion was associated with an increased risk of breast cancer (RR=1.5; 95% CI = 1.2-1.9)"(Daling et al., 1994, p.1585). Clearly there is an immediate simplification here with omission of the confidence interval and the increased risk expressed in terms of a percentage rather than risk ratio, to say nothing of the abbreviations used in the original. However, given this reduction in qualification, this

is an accurate version of the results given, since obviously "RR = 1.5" means a 50% increased risk. But it is noticeable that the reporting verb used by the journalist is "asserted", which has the effect of distancing the writer from the point of view of the author (Thompson & Ye, 1991). So right at the beginning the reporter is signalling that she does not necessarily agree with Daling et al..

However, she then goes on to note the other side of the argument by telling the reader that an editorial in the journal in question "points out" that this is a very small risk and she contrasts it with the 3000% risk of the heavy smoker. The journalist here would appear to be even-handed since she is putting forward an argument that supports Daling et al., in that the risk is very small compared to that typically found in cancer studies. But Gorman then goes on to compare this to the risk that doctors tell their patients about routinely in a quote from an anti-abortion leader. The extent of the risk is increased once again as a result, especially being in such a rhematic position at the end of the sentence. There is a difference in the type of risk involved, however. What is being compared here is, on the one hand, the type of risk that a doctor would tell her patient about, and the risk which appears in an epidemiological study. For a patient, a 50% increase in risk would appear considerable, whereas in an epidemiological study it is the lowest possible. By juxtaposing the two, therefore, the journalist is implicitly granting the validity of the anti-abortionist's comparison, whereas this is not in fact a comparison of like with like.

We see here, therefore, that even where a journalist may appear on the surface to be scrupulously even-handed and balanced in the best liberal journalistic tradition where `both sides of the argument' are given, in fact there is something other than neutral reporting of the objective science going on. The social attitudes of the abortion debate keep intruding into even what might appear to be `neutral, balanced, objective' reporting.

The next paragraph gives an interesting example of how the journalist, while seeming to report exactly the data of the research in fact may mislead the lay reader. She indirectly reports the words of the author by saying that Daling "pointed out that the

investigation followed 1,800 women over a seven-year period". This could be misleading to the lay reader, who is likely to think that this refers to 1,800 cancer patients. If you are unfamiliar with case-control studies, it is unlikely that you would realise that half of these are controls. In fact only 845 were cancer patients and 961 were controls. The reference to following the women over a seven-year period is also misleading. The seven years refers to the period during which the patients were diagnosed with breast cancer. The use of the term "following" suggests that the patients were contacted at the beginning of the period and studied throughout the next seven years, which is clearly not the case. A seven-year period, though, makes it all sound extremely thorough, when in fact it was a result of the need to get a large number of subjects from a particular area. The study could have been reduced to, for example, three years, if a larger area had been chosen to enable a greater number of cases to be found.

The status of the study is further enhanced by the statement in the same sentence that it was "one of the largest studies ever to examine the relationship between abortion and cancer". Now clearly this was a major study, but to the reader it is likely to be understood as even bigger and more important than it was, for the reasons listed above. Here the interests of the researcher and the journalist coincide, since the researcher naturally would like to be seen as the author of a major groundbreaking study, while the journalist equally wants to be reporting a `big story'. There is no suggestion that such misleading of the reader is likely to be deliberate on the part of either of the parties, but it does show how the nature of the science may be misinterpreted by the layman because of the lack of a science background.

At the end of this paragraph the relationship between science reporting and science research is explicitly brought out in a quote from Daling in an interview: "I'm absolutely appalled that politics is entering into the science of the study". This is as clear and explicit a statement as you could wish for of the position that the facts of science stand by themselves and should be allowed to speak for themselves with no interference by others with an `axe to grind'. It is the classic position of science as neutral, objective and value-free probably still held by the majority of Western-

educated scientists. By quoting the researcher in her own words like this, the journalist implicitly subscribes to the same point of view. This is heightened by the following direct quote, where Daling states that "No one is getting any of the correct information out to the public". Such a 'getting correct information out to the public' is once again a solid statement of the classic American journalistic view of the duty of the journalist (e.g. Missouri Group, 1985). By putting it in direct quotes like this the reporter in question not only aligns herself with the view, but implies that "this is what I am now in fact doing by quoting you like this".

Once again on the surface, therefore, the scientist and the journalist seem to be united in their joint aim of transmitting the facts of science to the general public. The contrast between the public agenda and the actual situation, however, is revealed in the following paragraph. After noting in earlier paragraphs how the study had been picked up by pro- and anti-abortion groups in the US, Daling is labelled at the very beginning of the paragraph as "pro-choice". Thus she is immediately placed in one of the two opposing camps and her study therefore likely to be an expression of her pro-choice views. Having planted the suspicion of bias in the reader's mind, therefore, at least for the reader with a pro-life standpoint, Gorman now goes on in the same sentence to accuse Daling of hiding findings which conflict with her point of view. Coming as it does immediately after the ringing declaration that "No one is getting the correct information out to the public" this is doubly damaging, the contrast being signalled immediately at the start of the paragraph by the introductory "But".

What is stated is that "Daling ... buried one of the most provocative findings in the fine print of a table". At several points in this sentence the writer is criticising Daling both explicitly and implicitly. Thus the finding referred to is described as "one of the most provocative", implying not only that this finding is provocative, but that so are others. In addition the verb used is "buried" while the obscurity of the place where the finding is buried is "the fine print of a table".

There are several important points here. To the layman, "the fine print of a table" suggests, as it is surely meant to, utter obscurity. In a scientific paper, however, tables

form a very important part and scientists are quite used to scouring tables of results. Moreover, tables are an especially important part of this paper, with 6 tables in all, making three-and-a-half pages out of a total of nine pages. The two main tables in fact take up all of pages three and four of the paper. The tables are thus a very prominent part of the paper.

The table concerned (Table 4) is clearly labelled as "History of induced abortion in women with an in situ or invasive breast cancer and controls, by age at first induced abortion or birth and by length of that pregnancy". It takes up most of the top half of page 6 of the study. The risk ratio is shown as RR in the final column. Scanning down this column it is very easy to notice the data referring to the subgroup mentioned here, > 8 wks at < 18. The data for RR in this table are mostly 1's, with one 2 and one 3, so the figure of 9 for this group stands out of the data. Rather than being buried, the figure of 9 leaps out at the reader.

More importantly, however, rather than being "tucked away" or "buried ... in the fine print of a table", these findings are mentioned in the Results section in a separate paragraph, the first paragraph after the most general results. Therefore it is in a prominent position and clearly signalled in the first sentence of the paragraph: "We examined the combined influence of age at first induced abortion and the gestational length at the time the abortion occurred" (Daling et al., 1994, p. 1585). Thus the two interrelated topics of age and date of abortion are clearly indicated. The very next sentence gives the data on those younger than 18 with an abortion at > 8 weeks. The beginning of this sentence shows that it applies to patients, not controls: "Fifteen patients, but only five controls, had had this type of abortion". The risk ratio of 9.0 is given. The second sentence contrasts this data with abortions at the same age but earlier in gestation and notes that they are "associated with a far smaller increase in risk (RR = 1.3)". Then the sentence following makes a contrast with abortion occurring at this stage but in women older than this, noting "little variation in risk". Finally the analysis is extended to the last abortion.

By no stretch of the imagination could this be considered being buried in the fine print of a table. The results are not only given at the appropriate point in the text, but the figures are explicitly given and all aspects of the contrast discussed. Each variable, gestational length and age, is explicitly considered. These results are found in the Results section, as might be expected. They are not discussed in the Discussion section, but none of the specific results are discussed there. Rather, this latter section is concerned with the reasons for the results, possible problems etc.

Daling is quoted as saying that the researchers did not say that the risk went up 800% in this group so as not to alarm anyone. Since these results were in fact given, as discussed above, the question, therefore, concerns the formulation of the data. The researchers did not say specifically that "the risk of getting breast cancer before age 45 went up 800%" (Gorman, 1994), because nowhere in the paper was this kind of phrasing used. Instead the increased risk was indicated in terms of RR, i.e. risk ratio, the standard method of indicating increased risk in such studies. However, as indicated above, the findings were indeed described and the RR of 9.0, i.e. an increased risk of 800%, given. The combined effect of all of this is to suggest that Daling had deliberately concealed a controversial finding because of her views on abortion. In fact she did nothing of the sort. The finding was described in full and given due prominence.

It seems that the journalist, in her efforts to make the study of relevance to the interests of her readers, has generated a bit of controversy here. She has managed to suggest, by a misrepresentation of the text, and use of a quote by the author, a position which supports her claim about the political status of the report. In the first part of the article, the first three paragraphs, she has attempted to broaden the significance of the story by placing it in a wider societal framework. She did this by reporting other media reaction. In itself, though, such media reporting does not prove that the original research was relevant to the abortion debate. The reporter, therefore, attempts more specifically to position her article in terms of that debate. First she characterises Daling as a supporter of abortion. She then describes how Daling has supposedly hidden evidence which would be damaging to that position. As we have seen,

however, such an interpretation of the article is highly misleading. Given only the *Time* article and without access to the original RA, the reader finds the argument quite convincing.

Having put forward such an argument, Gorman backs up the impression by showing how Daling rebuffed a pro-life lawyer who wanted her support. This reinforces her credentials as a pro-choice advocate. It supports, therefore, the position of the researcher as having suppressed evidence, by showing once again how she had motivation to do so because of her pro-choice stance.

The following paragraph (paragraph 6) discusses what might have brought about those results. It is based on the original article, which, however, although giving an extensive discussion as to how to explain the results, is rather tentative. The interpretation is based on animal models, epidemiological studies and cellular studies. But the original is not very certain of this interpretation, using phrases like "we hypothesised" and "it is plausible that these pregnancies... could result". The extreme tentativeness can be seen from the conclusion that "Conceivably, these morphologic differences could be related to differences in the subsequent incidence of breast cancer as well." (Daling et al., 1994, p. 1592). But the next sentence admits that there is "but little information" to support even such a tentative idea. Thus there is a very considerable degree of hedging of the results here, and although studies on hedging such as those by Myers (1989), or Hyland (1996a, 1996b) have shown such hedging to be ubiquitous in such RA's, the degree of hedging here goes beyond the norm for such articles.

The popularization admits that nobody knows how pregnancy protects against breast cancer. It qualifies the report by saying that Daling's work "may have narrowed" the area to be investigated. The account of the theory is overtly simplified and labelled as such but accurate enough in those terms. Nevertheless, the high degree of hedging in the original noted above is omitted in the popularization. It is not made clear just how unsure is this theoretical basis.

The following paragraph continues the theoretical explanation in the preceding paragraph, demonstrating the relationship to spontaneous abortions in the same theory. This is done by two quotes from Daling personally. The explanation is again in quite strong terms in contrast to the RA's highly uncertain "We can only speculate on why this result [on spontaneous abortions - AW] did not parallel that for induced abortion." (Daling et al., 1994, p. 1592). So once again the hedging of the original is lost. In addition the introductory sentence of this paragraph frames this result in terms of "suffering a miscarriage". While on the one hand this could be seen as merely translating Daling's medical terminology of 'spontaneous abortion' in the following quotes, on the other hand it distances women who have had spontaneous abortions from the issue. By calling them 'miscarriages' such women are separated from those who have had the other type of abortion, while the use of the term "suffering" not only makes such abortions something that happens *to* them, but also empathises with their pain.

The final paragraph frames the whole story once again in terms of the politics of the issue. While this is typical of the circular closing device of news stories in such magazines, which return in the denouement to an example or figure introduced at the beginning, it also draws back from the pro-/anti-abortion debate. It abstracts from the abortion debate to represent the problem as "the clash between science and politics". It then concludes in a version of the scientific freedom argument that "it would be unfortunate if the fallout from America's abortion wars stops some scientists from following up important clues." This is justified by the statement that "no one knows just what chain of events" causes breast cancer. Although strictly speaking true, the general pattern of what causes cancer is well known and two breast cancer genes had been recently discovered or localised at the time of writing the article. This is a fairly uncontentious conclusion, however, and allows the journalist to close the article on a positive note. No woman, whether pro- or anti-abortion, would want a science vs. politics debate to prevent scientists from finding a 'cure for cancer'. Such an ending, therefore, allows the reporter to have her cake and eat it. She can please the pro-life lobby by attacking Daling for suppressing the evidence, but can please the more

liberal pro-choice faction by arguing for the freedom of scientific inquiry and keeping politics out of science.

This magazine article, then, indicates clearly that such popularizations are not simply a case of simplifying or distorting the scientific message for the layman or laywoman. Rather they are an expression of an aspect of the general society's view(s) of the issue under discussion. Where the science touches on an issue which is socially and politically highly charged, like the abortion issue, the extent to which this societal interpretation takes over from the actual hard science becomes more obvious. But the general principle is the same regardless of the issue. A magazine like *Time* appeals to a broad general audience throughout American society. It cannot, therefore, be concerned only with the advance of scientific knowledge, which is of interest really only to the professional scientist. The scientific finding must relate to the interests of the wider society and thus the typical reader of the magazine.

In order to bring about such a relation, the journalist will connect the topic to those wider societal interests. It is not simply the case that, as here, the story is linked to the debate over abortion. Rather I have tried to show how the details of the way the journalist interprets the original scientific article can be explained by the need to make the science relevant to the interests of the audience. This can be seen not just in the general topic but in the choice of what aspects of the original to include and also in the way the original is explained. The changes made by the journalist in the reporting of this research can be put down to attempts to relate almost every aspect of her report to the wider sociopolitical issue, even to the extent, here, of misleading the reader.

In addition, this article, in the attempt to relate to as wide a segment of society as possible, somehow manages to take up two distinctly opposing views. As indicated, the reporter both criticises the researcher for hiding evidence which conflicts with that researcher's pro-choice views, which implies a pro-life viewpoint, and puts forward a 'science free from politics' argument that is the diametric opposite of such a viewpoint. The most likely explanation for such a subtle putting forward of two fundamentally incompatible viewpoints is a desire to appeal to as wide a proportion of

the readership as possible. This is not normally a huge problem, but in this case, with the likely readership just as split down the middle on the issue as is American society as a whole, then the journalist risks alienating half her readers. To avoid this she skilfully criticises Daling for bringing politics into science, stating that politics should be kept out of science. In the abstract, of course, this is a perfectly consistent argument, but in the context of the article it also enables her to appeal to both pro-life and pro-choice sides of the abortion issue. By doing this, therefore, she can appeal to both sides of the debate and thus keep all her readers happy.

Comparison 2

Text 18

LINKING ABORTION AND BREAST CANCER

Rubin, R.

***USN&WR*, 7th November 1994**

This article appeared in the same week, is on the same topic and is based on the same source as the *Time* article, '*Do abortions raise the risk of breast cancer?*' (Gorman, 1994) . However, there are clear differences in the way the story is reported in each magazine, especially in how the story is focussed on the social aspect. Thus, although the present article does provide a social interpretation of the topic, as, I would argue, do all such articles, nevertheless, the controversial social aspects of the topic are not played up to the same extent as the *Time* article.

The title of the article itself, '*Linking abortion and breast cancer*' is fairly similar to the RA original which talks about the '*Relationship to induced abortion*'. The subheading includes the 'risk' which is also in the title of the original. In contrast the *Time* article has a rhetorical question which includes the idea of raising risk. It is noticeable that both popularizations differ from the original in including the concept of increasing risk in their headline or subheading, while the original is neutral in this respect. The fact of increased risk is, therefore, taken to be the result of the research which will resonate most with readers.

The report of the main research finding in the *USN&WR* article is exactly in line with that of the original article to the effect that those women who have had an abortion are 50% more likely to develop breast cancer by age 45. The formulation is similar to the *Time* report but the latter mentions the age of 45 only in the context of a more detailed finding, the finding discussed above about the risk in women who had an abortion at < 18 and > 8 weeks. Thus the *USN&WR* report is more specific, more accurate and more in line with the original here. We shall see that these characteristics are typical of the whole report.

It is in the focus on the abortion debate that the two popularizations most differ. The *USN&WR* report mentions the debate on abortion only in the third paragraph. Even then the second sentence of this paragraph stresses what all parties agree on. The *Time* article, though, as we have seen, is very much focussed on the abortion debate and characterises not only Daling's position but the whole article in terms of that debate, as can be seen from the subtitle 'A report replying yes becomes a weapon in the war between pro-life and pro-choice groups in the US'. Both articles include a quote from Daling on the issue and the difference between the two can be seen in the quotations used: *USN&WR*'s "I feel scientists have to put their political and personal views aside and report the data" vs. *Time*'s "I'm absolutely appalled that politics is entering into the science of this study". The greater emotional heat in the latter formulation is evident.

The reporting of Daling's views on abortion is also different. As noted above, the *Time* article reports Daling as being pro-choice immediately prior to accusing her of hiding a provocative finding. *USN&WR*, on the other hand, notes that not only Daling but also "many of her peers" are pro-choice. The viewpoint is thus given legitimacy as being not an individual but a group view and, by the use of the word "peers", a professional rather than merely personal one. Similarly the Rubin article notes that Daling "favors abortion rights" rather than the pro-life/pro-choice phrasing of the *Time* article.

Another subtle difference exists in the way Daling is described. In contrast to *Time*'s presentation of Daling as being so pro-choice that she hides results which conflict with her views, *USN&WR* shows that she presents evidence even though it goes against her own views. Thus the article starts by noting that " the epidemiologist didn't believe studies that connected abortion with breast cancer" because of her own past research. But now in this new study "(t)he results surprised her.". Thus she is shown as being objective and changing her views in the light of new evidence in the best Popperian tradition. In this way she is presented as living up to the view ascribed to her that scientists' "views should not color how they interpret research results", while Gorman tries to show the exact opposite.

Turning to how the details of the study are presented, Rubin is more specific and exact in that she notes precisely the number of cases and controls in the study. More importantly, she does not mention at all the controversial finding which Gorman makes such a fuss about on women who had had an abortion <18 and > 8 weeks. Instead she mentions the data that women who had an abortion at < 18 or > 30, had twice as great a risk of cancer. The presentation of this data rounds off and smoothes out the figures in the original, as for both age groups the risk is presented simply as "more than double", whereas the original had RR's of 2.5 and 2.1 respectively. The effect, therefore, is to play down the increased risk of an abortion at age < 18, in contrast to the *Time* article, which plays this risk up.

This tendency not to dramatise the risk involved can also be seen in the second-to-last paragraph. Here the journalist states explicitly that "women who have had abortions should not assume that they are doomed to develop breast cancer". This direct reassurance of the reader is much stronger than anything in the corresponding article in *Time*. In addition she notes the smallness of the risk, a 1-in-a-hundred chance that would increase after an abortion to 1.5-in-a-hundred. Such a way of stating the risk of course makes it sound much less than saying there is a 50% increase in risk and is more in line with the original article's use of RR = 1.5.

The danger for a journalist, though, in thus seeking to minimise the increased risk is that it will make her story seem unimportant. She thus guards against this by noting the "considerable public-health implications" of the findings. This at the same time increases the importance of her story but downplays the threat for the reader, since it is *public* rather than *individual* health that is involved. Yet it still keeps enough relevance to the reader because of the societal link.

The concluding sentence in each story perhaps best illustrates the contrasting approaches of the two writers. Whereas the *Time* article generalises to the overall situation in an attempt to find a position that nobody could disagree with and thus generate a feeling of consensus after her sometimes rather partisan comments, the *USN&WR* reporter tries to gain agreement with a wry comment on the realities of abortion: "Whatever the findings, though, it is unlikely women will base decisions on abortion solely on how it affects their risk of cancer".

We see in these two reports, therefore, how different journalists with exactly the same main sources in terms of the written source and interviewee can present their material differently so that the reader gets a quite different picture of the research concerned. It may be the case that the two journalists have opposing views on abortion. Some evidence can be put forward for this point of view, the use of the term "abortion rights" vs. "pro-life and pro-choice" (note the order), for example. Not too much need be made of this, however. In both cases the journalists concerned endeavour to relate the research described to their readership and emphasise the social implications of the research.

That they do so in such contrasting ways shows that such a style of writing is not just a function of the 'sensational' nature of the subject matter. If it were simply the case that such popularization of science sensationalised the original research in a bid to make it 'interesting' or 'exciting' to the reader, we would not expect to find two such contrasting approaches. Such writing is rather a product of the need to reach out to the audience and make scientific research relevant to their lives. This can be done in different ways - by playing up or playing down risk - rather than by merely being

sensational. Thus whereas the *Time* journalist finds the relevance in the relationship of the research to the wider societal debate on abortion, the *USN&WR* writer sees the relevance more to the individual woman who has had an abortion. She therefore tries to calm her fears rather than inflame them. By contrast the *Time* journalist has to show that the research is controversial in line with a controversial subject and as a result she focuses on aspects of the research which she presents as biased and flawed.

Comparison 3

Text 17

IT'S NOT AS SIMPLE AS TAKING A BLOOD TEST

Rubin, R.

***USN&WR*, 15th August 1994**

The next article examined differs from the previous two in several respects, the type of cancer, the type of RA and in the way it is reported. The popularization is not so obviously 'social' as the other two we have looked at, but when examined in detail it is possible to detect how the journalist reinterprets the medical focus of the original in ways which relate it more specifically to the different readership.

This article reports on newly instituted recommendations for screening for ovarian cancer. It is thus directly relevant, or potentially relevant, to large sections of the general population who may read the news article. For this reason, it is especially interesting to examine here how this type of medical news, which disseminates medical guidelines, is reported in the mass media. The structure of the original article which is the main source is rather different from those investigated up till now, in that it takes the form of a review article (Carlson et al., 1994). However, the original article is still a research article in that it reviews the state of the art in research in the field concerned. It is similar to the research article in that it is targeted at fellow experts but differs from it in that these fellow experts are not only other researchers but also clinicians. The clinicians themselves, though, would be oncologists and specifically specialists in ovarian cancer, rather than general practitioners. Similarly,

the second source, a NIH panel statement, would have a similar audience (NIH Consensus Statement Online, 1994).

The first paragraph of the popularization makes no reference to the original article since it starts off classically with a personal example, Move 1a, and then continues with a general personal example. But the second paragraph begins immediately with a blanket unhedged statement that millions of healthy American women are wrong to believe that testing for ovarian cancer "can detect the cancer in time for a cure". This very strong statement is backed up by ascribing it to "two panels of experts". Straight away, therefore, we have a sweeping statement that ovarian cancer cannot be detected and cured, which is backed up by the authority of not one, but two, panels of experts. Such a blanket statement, though, would imply either that ovarian cancer can be detected, but not cured, or that it cannot even be detected, so logically cannot be cured. Thus it would seem that ovarian cancer cannot be cured.

This totally unhedged statement, therefore, does give misleading information, in that in some cases ovarian cancer can be both detected and cured. The next sentence expands on the initial statement by indicating that "there's no evidence" the two main tests for diagnosing ovarian cancer in healthy women save lives. But even this more explicit statement is far stronger than the corresponding statement in the original review article: "In our view, these predictive values are too low to justify routine screening with either CA 125 or ultrasound, particularly in the absence of any data documenting a decrease in mortality because of screening" (Carlson et al., 1994, p. 129). This is a nice example of how a popular article `translates' into general English from the specialist terminology of the researcher, but at the same time loses the specificity and hedging of the original. Thus the original hedges the recommendation by noting that it is the authors' viewpoint. The popularization states flatly that there is no evidence where the original merely notes the absence of documented data. There is also an interesting difference if we take account of the two different audiences. The original audience, clinicians, are interested in the predictive values of the tests mentioned and the values are too low at 3% and 1% respectively for women at average risk, thus making these tests not practical as general predictive tests. The

readers of the article, made up partly of women who may have such cancer, are not so interested in the statistical pattern. Similarly, the final sentence of the paragraph that most healthy women are not candidates for screening is firmly in line with the recommendations. But the popularization audience is not interested in whether the tests are suitable for use for screening for the general populace. They are interested in whether the tests would pick up ovarian cancer in them if they had it, which is a rather different matter.

It is in fact very easy for the journalist to be prescriptive here, since in this respect she is simply echoing the recommendations of the report. In this example, the task of the journalist is made simpler by the prescriptive nature of both originals. There is not much interpretation to be done: the journalist can with a clear conscience simply repeat the recommendations of the doctors concerned.

Paragraph 3 opens with a quotation from an interview with the lead author of the report, identified by name. The following quotation, after the identification though, is potentially misleading in that the statement that many women had asked for screening "before we really had a chance to think about the best way of using these technologies" could sound to an unsophisticated reader as if the doctors did not know how to use the equipment. This is an example of how even seemingly fair and accurate reporting, the doctor's own words, need not necessarily have the intended effect.

The next paragraph, however, begins with a strongly evaluative statement by the journalist to the effect that unnecessary anxiety and operations have been too frequent. In this she is foreshadowing the opinion of the doctor she next quotes. However, the Carlson study does indicate that certain kinds of benign cystadenomas can turn malignant but that the probability of this happening is unknown. The argument, therefore, is not quite so one-sided as Rubin makes it sound here.

The article next moves on to looking at screening methods in more detail, noting that the only method historically used to detect ovarian tumours has been pelvic

examination. She then goes on to note how ineffective this method is, but fails to note that this ineffectiveness is quite acknowledged in the literature. This makes it sound that doctors are using an ineffective method unknowingly when in fact it is well known that this method will usually fail to pick up early-stage tumours.

This paragraph again shows the journalist 'translating' and being less specific in giving her version of the original. Thus the statement that "the cancer has infiltrated the abdomen and often distant parts of the body" by the time a pelvic exam can pick it up appears to be a translation of "In general, ovarian malignancies have disseminated by the time they are palpable" (Carlson et al., 1994, p. 127). The following statement that symptoms do not usually appear "until the disease is advanced" seems to be a version of "Ovarian cancer is often asymptomatic until it has disseminated"(Carlson et al., 1994, p. 127). The question arises as to what constitutes "advanced" in this case. If dissemination is taken as the criterion, this means beyond stage 1, and 75% of patients are past this stage at first diagnosis. The conclusion then drawn from this is that "the chance of a cure is remote".

This is an interesting and important point regarding medical popularizations. Non-medical personnel tend in colloquial English to talk about a "cure" for a disease. Medical specialists rather operationalise cures in terms such as "remission" and "survival rate". In this review paper, cures are never mentioned, only five-year survival rates. The five-year survival rate for stage 1 diagnosis is given as 73%, for stage 2, 45%. This is not quite the dichotomy implied in the popularization – late diagnosis and chance of cure remote vs. earlier diagnosis and a presumed non-remote possibility of cure. First of all, there is no 'cure' as such, only five-year survival. Secondly, the chance of survival for such a period after diagnosis in stage 2 is hardly remote, rather 45%, in the average person's mind perhaps about half. Moreover, the survival rate for stage 1 diagnosis is only 50% better. The stark dichotomy envisaged by the popularization, therefore, does not exist in practice.

However the reporter's argument does serve to explain the search for earlier detection, which is validated by the original. The description of the different test methods

evaluated continues with a discussion of the CA 125 test. This is introduced initially in paragraph 5 not as a test, but as "technology", not a word that most laymen would apply to a blood test. The mention that this technology was "designed" to check women who already have cancer suggests that it is being used for something it was not designed for and if something is being used for a purpose it is not meant for it is surely being used wrongly! In fact use of a test to measure levels of a protein which is known to be elevated in patients is common enough in cancer studies – it is used also, for example, in tests for prostate cancer (testing for PSA)..

It is then noted that the number of women who are diagnosed with ovarian cancer has remained constant, but the use of the test has tripled since 1989. This would seem to suggest that the test has not been very successful, since it has not picked up any more cases. But in framing the results in this way the journalist is conflating the two measures involved in the testing of CA 125, sensitivity and specificity. Although not very sensitive in stage I diagnosis (studies quoted ranging from 25% to 75% for stage I and 67% to 100% for stage II), it is quite specific (98.8% to 99.4%). In other words it is very useful in confirming that a patient does indeed have ovarian cancer and not something else.

In the popularization the period given for the use of the CA 125 test tripling is not in fact since 1989, but "since the year Radner died". This is an interesting case of how the use of a particular personal example to make a connection with the reader may be misleading. The relationship may be taken as causal by the reader. The first paragraph in fact explicitly makes such a link: after Gloria Radner was reported of dying from ovarian cancer, because of it being front page news "millions of healthy women have since demanded to be screened for it". However the two large screening studies reported in the review article original were published only in 1992 and 1993, the studies taking place in Sweden and England respectively. Therefore the increase in screening does not follow from one case in 1989, but the arrival of the technique for use in screening in major trials.

The rest of the data in this paragraph is based more or less accurately on the review article. But again the popularization is more dogmatic than the original and less specific. The figure of 85% for raised CA 125 levels in women with confirmed ovarian tumours seems to be based on the original's figure of "approximately 80%" for women with epithelial ovarian cancer (Carlson et al., 1994, p. 128). The mention of the other sources of raised CA 125 is an accurate reflection of the original. On the other hand the statement that half the women with the earliest stage of ovarian cancer have normal CA 125 levels is rather interesting. The source here may be the NIH Consensus Statement, which mentions that only half the patients in Stage I have elevated CA 125 levels. However, since the journalist seems to be following the Carlson study generally, and the NIH Statement has only one page on screening, the former may be the source. As noted above the sensitivity range quoted in this study was 25% to 75%. Thus CA 125 levels were elevated to various degrees in different studies with the extremes being one-quarter and three-quarters of the patients studied. The journalist seems either to have taken the midpoint of this range or alternatively the summary estimate of 46% and rounded it up to a figure of half. The second problem here is the concept of "normal CA 125 levels". The journalist is here interpreting a level which is used for testing as "normal", when in fact the original nowhere describes this level as normal, but only as "a reference level of 35 U/mL" (Carlson et al., 1994, p. 128). The extent to which this level is a construct of the research method is borne out by the fact that if a positive test was redefined as "a CA 125 level greater than 35 U/mL that doubles within 6 months" the specificity increased to 99.9% (Carlson et al., 1994, p. 128). In fact the largest screening study done up till then used a different reference level, 30 U/mL.

This is an interesting case of a benchmark instrumental measurement being interpreted in normative terms. It misses completely how scientific measurements are made with reference to a standard. Thus in this case a particular woman may have had a CA 125 level of 33U/mL. This would mean that she fell below the reference level, but it may not mean that her level was 'normal' in the sense understood by a layperson, i.e. one shared by the majority of the population. Such a lack of understanding of the methodology of science is widespread among the general public

(Millar & Wynne, 1988) and it is interesting that even a competent and experienced science reporter like Rubin should perpetuate this misconception. However, if we consider that the reporter is concerned with relating to the interests and needs of the readership of the magazine, then we should not be surprised that this is the case. The kind of appreciation of the qualifications and limitations inherent in the scientific method called for by Millar and Wynne presupposes that the public is as interested in the narrative of science as scientists are. Of course, they are not interested at all. What they are interested in is, in this particular case, whether there is a reliable test for ovarian cancer or not. This kind of black and white answer is precisely the kind that the scientist is not willing to give in most cases.

The following paragraph, on ultrasound, is also rather critical, noting that only 5 women turned out to have ovarian cancer out of 5,500 screened, with 326 undergoing surgery. The source here is the NIH Consensus Statement (NIH Consensus Statement Online, 1994), although the number of patients is rounded up from 5, 479. The author notes that it is not even clear that this early detection improved their chances of survival, but since there is a 73% five-year survival rate for stage I detection versus 45% or less for later detection, this is again choosing a rather pessimistic interpretation of the findings.

Perhaps conscious of the negative tone up till this point the next paragraph begins the concluding section with a contrasting more positive outlook. It is notable that figures for incidence of ovarian cancer are given here towards the conclusion, whereas in the original they are part of the introductory epidemiology section – subsection 3.2 Incidence. The other figures for lifetime risk are adapted from the figures in the next section of the original, Estimating the Risk for Ovarian Cancer. So these too are presented here as important new information, whereas in the original they were merely introductory background information. However, the last sentence of this paragraph is in fact much more important, as it constitutes part of the main recommendations of the panels and comes also from the recommendations at the end of the review article.

Paragraph 9 is the only one discussing the combination of both ultrasound and CA 125, which is, however, in the popularization, placed in a different context, that of testing women with at least two affected first degree relatives. The main focus of the paragraph is in fact such women. The two paragraphs, 8 and 9, have an ever-narrowing topic. We start with women with no affected relatives (1 in 70 risk), then those with one affected relative (1 in 20 risk), then those with more than one affected relative (1 in 15 risk) and finally those with hereditary cancer (1 in 2 risk). The final sentence notes, however, that even this last group might not be helped by screening.

The first sentence of the final paragraph is concerned with future research but the very last sentence reverts to giving advice to the reader, Move 10. However, the final piece of advice is somewhat idiosyncratic. Throughout the article the journalist has stressed how unhelpful testing for ovarian cancer is and how each of the methods has many pitfalls. She is now faced with the dilemma of how to end her article on a more positive note. She has already informed the reader that in fact the risk of getting ovarian cancer is in fact quite small, so that avenue is closed to her. So she in fact advises the reader to talk to her doctor about contraceptive pills rather than ultrasound or CA 125. This information is accurate in that taking contraceptive pills does reduce the risk of developing ovarian cancer. But the article has not been up till then about reducing the risk of developing this cancer, but about finding out whether you have it or not. So this piece of advice at the end will not in fact help anybody who does in fact have ovarian cancer. This is a somewhat strange case where the journalist's seeming desire to end on a positive note might in fact do some harm, as it would not help anybody who in fact had ovarian cancer, who might in fact be helped by being tested and diagnosed at an earlier stage. Ironically, the reporter might well have recommended exactly the opposite strategy, as pregnancy reduces the risk of getting ovarian cancer even more than the contraceptive pill!

Unlike the first two comparisons, then, this comparison is neither so overtly political or social, in the sense that this does not have a connection with an argument ongoing in the wider society. Nevertheless, it is still clear that the argument of the original is considerably changed, because the journalist is aware of the differing interests of the

different audience she is writing for. In its own way, therefore, this article also shows evidence for the narrative of society I have postulated, although not in such a stark manner as the first article. Thus the narrative of society does not necessarily mean taking a strongly committed position on a controversial issue. What is essential to it is rather the recasting of the scientific argument in terms that make sense to a non-scientific audience and relating this to social concerns.

Comparison 4

Text 6

WARDING OFF BREAST CANCER

***Newsweek*, 3rd October 1994**

This text is rather different as it is one of the shortest texts in the corpus, being only three paragraphs long, with only five move types, 1, 2, 5, 8 and 10. For this reason it might be expected that it is bound to be different from the original at least in terms of length, since the original was 6 pages long (Bernstein et al., 1994). However, such a shortening is not fundamentally different from other popularizations, since all popularizations studied were much shorter than the originals.

Nevertheless, there were other differences which could probably not be put down to the difference in length which are worth pointing out. One of the most significant differences comes right at the beginning of the popularization where the topic of the article is introduced by relating it to the more general topic of the effects of regular exercise on the prevention of disease, specifically heart disease, osteoporosis and colon cancer. In contrast the starting point of the original research article is evidence that cumulative number of menstrual cycles and thus exposure to ovarian hormones help determine breast cancer risk. Thus the RA fits into a pattern of previous research on risk factors which are directly physiological in nature, while the popularization focuses on action by the reader. The contrast between the focus on the scientific argument and the social effects is clear from the outset. Although both articles discuss risk factors, the way they discuss these risk factors is completely different. For the RA the risk factors are physiological in nature and largely given, with the

research looking at how far exercise affected this risk factor. The popularization for its part starts off with the exercise, which is highly variable, and does not even mention number of menstrual cycles.

The reason for this is clear in terms of the relating to the reader focus. It is part of putting the reader at the centre of the picture. Furthermore, it not only centres on the reader but puts the activity of the reader at the centre and thus more in control. The average woman reading the article is not likely to feel that she is in control of her lifetime number of menstrual cycles and still less the cumulative exposure to ovarian hormones. On the other hand, she is likely to feel that she is in control of the amount of exercise she takes.

This shift in emphasis is also seen in the reporting of the research as showing that "some cases may be preventable". Thus while the reporter cannot in conscience go as far as to promise that if you exercise more you will not get cancer, she goes as far as she dares in doing so. Among those cases that may be preventable might be the case that any one reader may (not) get. The RA, though, as might be expected, stays at the level of the statistics of the general population, and talks merely in terms of reduced risk.

The second paragraph describes the patients studied and the methodology of the study. Although this might seem to be following fairly closely the narrative of science, in fact the description of patients and methods is drastically reduced compared to the original, where it took up over two columns of small print. All the problems with contacting patients and controls, especially the latter, are completely edited out, so that no hint is given of the difficulties involved. In fact, the number of cases and controls mentioned in the popularization is 199 less than those actually interviewed because a change in methodology meant that this number of case/controls had to be removed from the study. However, such detail is important only for those other researchers who might want to critique the study and is completely irrelevant to the average reader. Some procedures, though, are mentioned, in that it is said that the researchers corrected for known risk factors, two of which are mentioned. The reason

for the inclusion of such information would seem to be a desire to reassure the reader regarding the reliability of the results of the study.

As is usual in such studies, the phrasing of the results in the original and the popularization differs. The RA expresses the results in terms of the odds ratio, so that it is said in the group concerned, "the odds of breast cancer were 0.42 (95% CL = 0.27, 0.64) relative to that of inactive women" (Bernstein et al., 1994, p. 1404). The popularization expresses this as "nearly 60% less breast cancer", which is a quite accurate representation. The popular version also rounds up the number of hours a week spent exercising from "at least 3.8" to "at least four", again a figure which is close enough to the original to meet the needs of the reader. Where the research scientist requires precise figures, the average reader is not going to care about a difference of 12 minutes a week.

Rather oddly, the popularization here is in one respect more hedged than the original since it quotes the group concerned as being "Women who said they exercised at least four hours a week", noting the self-reporting element involved. But this could be said to be introducing a hedge simply at a different point from the original, since the latter had already noted this point. The final sentence, however, departs from the original in the expected direction, i.e. in terms of less hedged, more general statement. Whereas the original notes only that their "data suggest that women who maintain an activity level of 1-3 hours/wk could reduce their risk of premenopausal breast cancer by about 30% relative to inactive women" (Bernstein et al, p. 1407), this is reported as actually having brought about a 30% reduction in risk. Thus a suggested possible future effect of exercise is reported as an actual reduction in the trial. In fact, there was no precise group "women taking 1-3 hours a week of exercise". There was one group labelled as 0.8 - 1.6 hours and another 1.7 - 3.7 hours/week. In fact (Bernstein et al., 1994, Table 2, p. 1406), these two groups' results were inverted so that the first group had an OR of 0.65 and the second an OR of 0.80. For this reason probably these results are not mentioned or discussed in the text and are simply generalised to a probable reduction in risk of around 30%. The popularization here, therefore, overstates the research results in the effort to give firm advice to the reader.

The final paragraph turns to the causes of the proposed impact of exercise on breast cancer rates, with the first sentence a suitably hedged report of the suspected agents. But the second sentence is much stronger than anything in the RA: "The reproductive hormones progesterone and estradiol are known to foster breast tumors and past studies have shown that exercise slows the production of these chemicals." The original is in fact an epidemiological study and does not discuss at all any direct biochemical link between the hormones and tumour formation. Only "exposure" to the two hormones is mentioned and nothing at all about the mechanism of action, with no reference either to any other studies in the literature on this. A causal mechanism is therefore stated in the popularization which is much stronger than anything hinted at in the original. Rather than the straight one to one causal link between the hormones and tumour growth, the original talks instead about results in parous women being "consistent with" their hypothesis. Effect on risk of breast cancer is then stated as being by alteration of menstrual function by one of two mechanisms, which then in turn reduces the exposure to the hormones. The use of the word "foster" in the popularization to express the relationship between the hormones in question and the breast tumour is also somewhat ambiguous. Carcinogens can either be cancer-causing, cancer-promoting, or both. The use of foster suggests probably that the hormones are cancer-promoting, but is sufficiently lacking in clarity to potentially cause some anxiety in the reader.

There is an appropriate hedge at this point, though, in the magazine article, where it is pointed out that it is not known whether these effects last beyond menopause. Although this is consistent with the original the use of the phrase "cancer-fighting" could be very misleading here. What was under study here was the reduced risk of getting cancer. There is no claim that exercise has any effect against the disease if a women actually has cancer, which is what "cancer-fighting" suggests. It is also noteworthy that the three sports of jogging, tennis and swimming are prominently placed here right at the end of the article. The original mentioned particular sports only in the methods section and even there it was not prominent at all. The three sports named by the journalist are only some of those mentioned in the original and they are not mentioned in that order. By selecting these particular three and ordering

them in the way she has the journalist could give the impression to the reader that only these three sports, or these three sports in particular, have a protective effect. Once again, therefore, we have the efforts of the reporter to relate to the reader producing a change in the original in the sense that the reader is likely to come to a conclusion which is at variance with that intended by the original researchers. Again, though, this is understandable given the emphasis of the popularization on giving positive advice to its readers. This is likely to mean that the hedging and generality of the original is replaced by specific recommendations.

The final sentence is a typical Move 10, giving direct advice to the reader to exercise more. This differs from the original, which of course does not give direct advice to the reader. But the original does give a recommendation in terms of educational policies which parallels that of the popularization. Here we have a nice example of how the different audience produces a change in focus but not a distortion. By this I mean that the average reader could not be expected to have any impact on educational policy, but could be expected to be able to effect a change in her exercise habits. This is a change brought about solely by framing the information in a way that makes it relevant to the different readership. In this respect it differs from the "jogging, tennis and swimming" example quoted above in that the choice of these three sports could not be explained solely on the basis of the change of readership. It is an idiosyncratic choice on the part of the journalist to choose these three particular sports.

This article, therefore, although extremely short, follows the same general pattern in the way it reports the science in ways which are relevant to the readership and the wider society. In this case, we have an especially clear example of how the research is re(-)presented in such a way that the individual reader can relate to it. Rather than being a general picture of the effect of exercise on hormonal levels, the popularization becomes a call to exercise more to prevent breast cancer.

Comparison 5

Text 7

ZEROING IN ON BREAST CANCER

Shapiro, L. & Springen, K.

***Newsweek*, 26th September 1994**

This is the first of three popularizations reporting the same scientific discovery and thus relying on the same source as their primary source. The topic is probably the most significant one in cancer genetics during the year, the long awaited and sought for discovery of the first gene for breast cancer, *BRCA1*, by Mark Skolnick and his team in September of that year (Miki et al., 1994). It is reported in this article in *Newsweek* and another article, compared below, by Madeleine Nash in *Time* (Text 10). Although *USN&WR* also reports the discovery, its way of doing so is significantly different from the other two magazines.

The *Newsweek* article starts in the standard popularization manner with individual examples of patients with the disease in question, in this case breast cancer, or more particularly hereditary breast cancer, thus a classic Move 1a. Most of the first paragraph is concerned with the example and then towards the end brings in an announcement of the discovery of the gene. However, this is not a real Move 2, since the actual discovery is not mentioned, merely that a long-awaited discovery has been made. The reason for this is that before mentioning the actual discovery the journalists go back to mention some background research, Move 4.

This is a rather unusual pattern, but the reason for it is clear in this case. It would seem at first sight that this is an instance of the popular article also following the narrative of science, since it is fitting the new discovery into the ongoing scientific argument. However, this would be a misreading of the situation. The journalists in this article in fact discuss the discovery of the gene using the well-known metaphor of the race to make a discovery and the team who makes the discovery as winners of the race. To do so they must first demonstrate that there has been a race and thus they go back, so to speak, to the start of the race, the discovery of the location of a gene linked

to hereditary breast cancer in 1990 (Hall et al., 1990). The identification of the mapping of the gene to a specific chromosome arm in 1990 by King and the idea that there then ensued a race to be the first to actually identify the gene is confirmed by the discovers themselves: "Intense efforts to isolate the *BRCA1* gene have proceeded since it was first mapped to chromosome *17q* in 1990"(Miki et al., 1994, p. 66), with a reference to the Hall et al paper. This was recognised too by the team locating the gene to *17q21*. The overt aim of the research was to move from locating the gene on a chromosome arm to identifying the gene itself: "The ultimate goal of gene mapping of human traits is to move from a known chromosomal location to identification of the crucial gene and characterisation of its critical alterations." (Hall et al., 1990, p. 1688). Similarly, King and her team were conscious that they were engaged in a race to discover the gene (Suzuki, 1997).

Moreover, in an article reporting the discovery a *Science* journalist uses the same metaphor, which seems to be acceded to by the scientists themselves (Nowak, 1994a). Certainly, Skolnick himself appears to have thought in those terms, since he omitted the actual gene sequence and GenBank accession numbers from the draft version of the paper sent to *Science* for fear that competitors might get their hands on the information (Nowak, 1994a). In addition, he overtly described the process as a race at the press conference announcing the discovery (Nowak, 1994a). That this is not just a journalistic construction but that the effort to discover a new cancer gene is also construed as a race by the scientists themselves can also be seen from the efforts of the scientists concerned to be the first to publish the discovery of the *PTEN/MMAC1* gene (note the double name, showing the competition). One of the team leaders actually flew to Washington, D.C., to hand deliver the paper announcing the discovery to the offices of *Science* to make sure it appeared before the announcement of the discovery by another team in *Nature Genetics* (Pennisi, 1997).

Here we have then an example of what at first might seem like typical exaggerated popular journalism, a construction of scientific discovery to make it conform to popular stereotype, except for the fact that in this case the stereotype is rather close to the facts. In other respects, however, the popular version does omit some of the

hedging in the original. The magazine article announces that the team led by Skolnick had discovered and isolated the gene causing familial breast cancer, where the original RA is careful to talk merely about the discovery and isolation of "a strong candidate" for this gene.

The popular version is then faced with the difficulty of explaining the difference between what King had discovered and what Skolnick discovered. This it does accurately enough in general terms by saying that King had "narrowed the search" and the later consortium had "zeroed in on" the gene. However, such a simplification overlooks the fact that Skolnick's team had not jumped in one fell swoop from the relatively large region identified by King's team to identifying the gene. Other work in the interim had further narrowed the search. Thus Neuhausen et al (1994), had recently cut down the area to be examined to a much smaller 600 kb region and produced a physical map of the region. The popularization, therefore, despite showing to some extent how one discovery is built on another previous one, still tends to exaggerate the breakthrough nature of the discovery and underestimate the extent to which scientific research is cumulative. In addition, the use of the phrase "zeroed in on" tends to suggest a precise, specific location, when in fact the gene is distributed over "roughly 100 kb of genomic DNA" (Miki et al., 1994, p. 67).

The popularization then continues to give an extremely truncated version of how *BRCA1* acts to bring about cancer. It notes that it is a mutant form of a gene and that it is normally a "tumor suppressant". This is a simplification, of course, of the original picture, in which the normal gene codes for a tumour suppressor, with the mutated form having no or reduced function. It is the protein which is the tumour suppressor, not the gene itself. Overall, however, the oversimplification probably does not make any difference to a reader who does not know the biology, while one who does will recognise the shorthand. It is interesting, though, that the popularization uses the term "tumor suppressant", since the standard form is always "suppressor" and the substitution of the term "suppressant" is not likely to make the technical term any more understandable. There is no obvious reason for the shift and

it cannot really be said to make the text any more popular or understandable to the layperson.

At this point the magazine article makes its only reference to the fact that *BRCA1* is also linked with increased risk of ovarian cancer. The original article, on the other hand, is just as much concerned with ovarian cancer as breast cancer, although most of the article is concerned with the actual location and identification of the gene. The reason for the concentration on breast cancer in the popularization can be put down quite simply to the need to relate to the interests of the readership. Whereas breast cancer is the most common cancer affecting women, ovarian cancer is a relatively less common cancer (NIH Consensus Statement Online, 1994). The readership, at least the female portion of it who are the ones most likely to read the article, are almost certainly concerned at the possibility of contracting breast cancer, whereas ovarian cancer is much less salient to the general populace. Thus whereas mammographies, checking for lumps on the breast etc, are standard practice and familiar to practically all readers of the article, there is no standard effective screening procedure for ovarian cancer, as we saw earlier in this chapter (Carlson et al., 1994). News about breast cancer, therefore, is likely to draw a large number of readers to read the article, while the same cannot be said about ovarian cancer.

At this stage the article makes the point for the first time that the news is directly relevant to only a small proportion of women in that the gene identified affects only hereditary breast cancer not the majority somatic type. Earlier it had indeed been mentioned that King had located the gene for familial breast cancer, but this did not necessarily mean that the gene would not be implicated also in somatic mutations. In fact researchers had hoped to discover that mutations at the same loci also led to somatic disease (Hall et al., 1990). It was only at this point in 1994, with the simultaneous publication of another article in the same issue of *Science* by Futreal et al (1994) that the fact that *BRCA1* was not implicated in somatic cancers became apparent. Up till that point it had been widely expected, on analogy with other tumour suppressor genes, that *BRCA1* would also play an important role in non-hereditary cancers (Futreal et al., 1994; Nowak, 1994a).

Although the *Newsweek* article does admit that only a small number of women will be directly affected, it immediately tries as hard as possible to make that number as large as possible. Thus it talks about the number of hereditary cases of breast cancer as being about 5-10% of all cases in the United States, where both Miki et al (1994) and Wooster et al (1994) put the figure at around 5%. The source for this increased figure is probably the Nowak article in *Science*, which gives the same unattributed figures (Nowak, 1994a) Again the figure given for *BRCA1* is also slightly inflated at about half compared to the figure of 45% in the two studies cited in the previous sentence. The journalists seem here, then, to be playing up the importance of the findings by increasing the number of people who are affected by them. The original, which is not so much concerned with the number of people who have this type of cancer but with the discovery of a long searched for gene, does not need to labour this point. Every researcher in cancer genetics and many beyond will be affected by the discovery, so there is no need to worry about the fact that the number of patients affected may be relatively small. The importance in terms of the progress of the field is such that the clinical impact can be ignored for the time being. This impact is much less certain as a claim and cannot be backed up by the evidence yet, so it is better not to make too strong a claim in this direction.

To counteract the news that the discovery affects only a small proportion of women with cancer the reporters carry on by emphasising the extreme nature of the effects it has on those who are unlucky enough to have the gene defect: "the consequences are stark". They are thus able to balance the relatively few people affected by the size of the effect. This of course is a standard journalistic practice, with concentration on news which either affects a lot of people or is dramatic in its effects (Rensberger, 1997). The figures for women with inherited breast cancer are then given and compared with the general population, with a related small graphic underneath. This convincingly shows the claimed stark consequences, but it is again noteworthy that this information does not come from the source article on the discovery of *BRCA1*, which is concerned not with the epidemiology of the disease but the identification of the gene. Here again we see the social factors relevant to the more general readership winning out over the research science emphasis of the original.

The next paragraph then jumps to the topic of a blood test for the new gene. This is somewhat surprising, as it is improbable that any of the broader readership of the article would be thinking in those terms. However, it might very well be the case that those from families with hereditary breast cancer might be aware of the possibility of such a test. Again at first sight this might not seem related to the original research article, which does not discuss tests for the gene. Nevertheless, there is a link, although this link is not brought out in the article. Whereas the article simply identifies Mark Skolnick as being at the University of Utah, in fact Skolnick also, like many of the consortium, works for a company called Myriad Genetics, whose business it is to develop such tests. Thus the two paragraphs on gene testing are quite relevant to the topic, although the precise nature of the relevance is not made clear in the magazine article. The real social importance of the discovery is indeed the fact that it might lead to the development of a test for the mutated gene. The questions raised are very important and germane in this context. We have here, therefore, an example of where the magazine article clearly goes much further than the original article in discussing the social implications of the research and does so in a manner which is very much to the point.

The journalists may be doing this because they see it as their professional duty as ethically responsible reporters. However, such a viewpoint is not incompatible with a simple desire to generate a narrative of society which will make the news of maximum relevance to the readership. Scientifically, the identification of *BRCA1* is a major breakthrough and is an important step forward in the narrative of science. Nevertheless, it is impossible to make the original paper itself interesting to a general audience if the journalist sticks too closely to the content of the original paper. Most of the original article is concerned with a detailed description of the identification of the gene concerned, description of the mutations found in the patients studied, testing to see if the mutations correlated with haplotypes, the phenotypic expression of the gene and its role in cancer. As the paper itself states: "Identification of a candidate gene as *BRCA1* requires a demonstration of potentially disruptive mutations in that gene" (Miki et al., 1994, p. 67). Hence a large proportion of the paper is concerned with technical details such as a specific mutation at a particular point in an exon in

one kindred. These details are only of interest to other researchers, but most of the paper is concerned with such issues, since only in this way can the researchers convince other researchers of the merits of their case. The argument of the paper makes perfect sense in terms of the narrative of science; it makes no sense at all in terms of the narrative of society. The fact of the identification of the gene is taken as read in the popularization, while in the original it is just this that has to be established.

The magazine article ends with a reference to the location of another breast cancer gene, *BRCA2* and notes that the race is now on to isolate this second gene, although it could take years. This is likely to puzzle the lay reader, who could be confused as to why, if the gene has been located, it is so difficult to isolate. There is a contrast here between the use of the word "located" in the *Newsweek* article as contrasted with the "localized" of the original (Wooster et al., 1994). The gene had not in fact been located so that its precise position was known, merely localised to an area of 13q12-13. However, finishing this way fulfils the function of ending the article with the same metaphor of the race that it began with and enables the article to end on an upbeat note looking forward.

In fact, at the very end there is a further recycling back to the very beginning, bringing in a quote from the cancer patient whose profile had started the article. The quote here notes how the research may help other patients like her. Thus the whole of the research is framed within a pattern of social impact on actual patients with familial breast cancer, highlighting once more how the actual details of the research itself are secondary. Like almost all such cases it is the impact on society which is of the greatest relevance rather than the research itself.

Comparison 6

Text 10

CORNERING A KILLER

Nash, J.M.

***Time*, 26th September 1994**

This article covers the same topic and uses the same original article as source as the previous article in *Newsweek*, in other words the discovery of the *BRCA1* gene for breast cancer and the Skolnick team's article in *Science*. The two articles differ in their emphasis in some respects; yet in many ways they are quite similar, showing in both cases the interplay between social and scientific aspects which is typical of such popularizations. As we have done a considerable amount of comparison with the original RA already, the bulk of this comparison will be concerned with the similarities and differences in the way that the two news magazines report the same story.

Unlike the *Newsweek* article this article by Nash in *Time* does not open with a Move 1a) focussing immediately on a personal example. Instead this move is postponed slightly till the end of the first paragraph where there is a direct quote from a patient of her reaction to the news. Instead the article starts with the search for the gene and the actual discovery. As we have seen, the *Time* article uses the metaphor of a race to describe the discovery with the scientists trying to find the gene as the runners in a race and the Skolnick team as the winners of the race. Nash takes a different tack, the dominant metaphor she uses being not a race, but the hunt. Thus the title of the article is *Cornering a killer*, and the opening sentence describes the gene causing hereditary breast cancer as an "especially elusive quarry". She then goes on to describe the search for the gene and the finding is described as the gene having been "tracked down". Like the authors of the *Time* article, too, she describes the extensive search for the gene, adding the extra information that there had been rumours before of it having been found, but these, by implication, had turned out to be false.

Both popular articles are expressed in terms of an overall metaphor. This has the effect of giving the audience a familiar framework into which to fit the new information. This, too, derives from the differences between the research article and the popularization as genre. The RA has a given framework of the previous research in the field, the narrative of science, which the reader as research scientist in the field is already familiar with, and which the new information is inserted into and the goodness of fit estimated. The general audience has no such framework into which to put the new information provided by the journalist and so the journalist has to provide the frame as well as the picture.

Both metaphors function well to introduce the reader to the story and give the reader a way to conceptualise the new information. Perhaps, however, the *Time* version could be considered superior as it has added dimensions that the *Newsweek* approach does not. Whereas the metaphor of a race is effective in conveying the competitive ethos and the ongoing nature of the research effort, it is actually fairly neutral in terms of applying specifically to the search for the breast cancer gene. It could equally well be applied, for example, to the search for the top quark and the competition between different teams of physicists in the United States and Europe to find it. The description of the search for the breast cancer gene as a hunt, however, also brings in the idea of the gene as a killer which is being hunted down. It also includes the idea of competition, since the hunters are competing to see who will be the ones to find the quarry, but it focuses more on the deadly nature of the gene. This has the interesting effect of casting the research efforts of the scientists involved in a more positive light, since trying to be the first in a race is a rather selfish pursuit, with everybody else who is not first losing out, whereas hunting a killer is a more noble pursuit since it benefits the whole community.

This is not to claim that the authors of the respective articles consciously thought in those terms with the *Newsweek* journalists having a dim view of the researchers as selfish glory seekers. Such is almost certainly not the case. The pervasiveness of metaphor in our everyday thinking has been evident since Lakoff and Johnson (1980) and it is clear that a popular science journalist must make an effort to find a method of

framing the matter in a way which makes the science understandable to readers. But the choice of metaphor does mean that a certain way of conceptualising the matter has been chosen which precludes other approaches (Lakoff & Johnson, 1980). So the use of the race metaphor means that it is impossible to describe the cancer gene at the same time as something harmful. That metaphor must be dropped before the deleterious effects of the gene can be fully described.

If we look at the two descriptions in terms of their choice of these two metaphors, it is indeed clear that the *Newsweek* article is rather less positive in its reporting than the *Time* article. The latter lays much more stress on the importance of the discovery than the *Newsweek* article. This is laid out even in the quotation from the patient at the end of the first paragraph, where she states that it is the most significant event in the thirty years since her mother was diagnosed with breast cancer. Without actually acceding to that degree of importance, Nash, nevertheless, confirms that "The discovery is indeed important". In contrast the corresponding stage in the *Newsweek* article, the first sentence of the second paragraph, talks about "important clues to a still-mysterious disease". Thus the latter piece displays a considerably greater degree of hedging as to the importance of the discovery. In fact the Shapiro and Springen article talks about the consortium having "announced they had discovered" the gene. This is two stages away from the "discovery" described by Nash. First of all it is merely announced as a discovery by the scientists themselves, which leaves open the possibility that what they had discovered was not the breast cancer gene, or indeed, that the discovery was a complete artefact and that nothing had in fact been discovered. In this they are closer to the hedging of the original article, where only a "strong candidate" for the gene is announced. Secondly, Nash reifies the action of the researchers and makes it harder to dispute by labelling it as "the discovery". As Halliday and Martin (1993) stress, the use of the nominal form here already makes the accomplishment more solid and less open to question.

Nash here is interestingly closer to the narrative of science than her fellow reporters in the way she continues this approach. She backs up her claim for the importance of the discovery in no uncertain terms with a strong statement. After the claim of its

importance we get a colon which is followed by the statement that "it is a major step toward understanding the origins of a disease that kills so many women each year, 46,000 in the US alone". This is a very neat coupling together of the narrative of science and the narrative of society. The first part of the sentence with its mention of steps towards understanding is straight out of the narrative of science. This metaphor of science advancing closer and closer to understanding by a series of discoveries encapsulates a view of science which would without much doubt be acceded to by the majority of active researchers (Cole, 1992). The pure science aspect is brought out too by the reference to "origins": by getting to the origins of a disease in genetic terms one will truly understand the disease. The societal aspect then becomes dominant in the second part of the sentence where there is a switch to the effect of the disease on the wider, in this case US, society, with a description of the number of women who die from the disease.

Both articles, though, quickly indicate the limited nature of the discovery by pointing out the relatively small number of cases of cancer that are in fact hereditary and thus caused by this gene. But the *Time* article, as we have already seen for the *Newsweek* article, nevertheless overestimates the number of cases due to *BRCA1*, since it gives the figure of 5%, which is the proportion of cancer cases which are due to hereditary factors (Miki et al., 1994, p. 66), but not all of which are due to *BRCA1*, since *BRCA2* contributes to about the same number, with maybe other genes also causing hereditary cases (Wooster et al., 1994).

Again Nash is closer to the narrative of science and the original article in that she actually does say something about the *BRCA1* gene itself. She characterises it as unusually long and complex and crippled by many different mutations. In this she is accurately paraphrasing the original's description of the gene as being composed of "22 coding exons distributed over roughly 100 kb of genomic DNA" (Miki et al., 1994, p. 67). Clearly, such information would mean nothing to the average reader, whereas to be told that the gene is longer than normal and complex conveys the essence of the information. The description of "many different mutations" confirms the original's statement that frameshift, nonsense and regulatory mutations were all

found. Once again it would serve no purpose to mention the types of mutation found since the information would be incomprehensible to the average reader. Here we have a good example of the science journalist generalising the information given to the reader so that it is at a level of abstraction that the reader can understand.

The description of the gene as being almost unlike any other gene ever found strictly speaking refers to the protein coded for, but is applicable nonetheless. She goes on to explain in some detail the functioning of the gene and explains how it works, but without any technical terms like 'tumour suppressor' as the *Newsweek* article did. Instead she uses a standard engineering/computer metaphor, comparing the gene to a master switch and referring to a backup copy being damaged or lost. This is more comprehensible to the average, probably computer-literate, reader than the original's "loss or inactivation of the wild-type allele" (Miki et al., 1994, p. 66) with its specifically genetic technical vocabulary. Here we have then the journalist using somewhat technical language, but language which is more familiar to the reader than the highly specialised language of the original RA. However, using the language of engineering like this makes things sound rather more clear cut than they in fact are described in the original. Description of *BRCA1* as a master switch is to say rather more than that it appears to encode for a tumour suppressor, which is all that the original claims. The use of the expression suggests that the gene has a wide function, whereas in fact the gene seems to be implicated only in hereditary breast and ovarian cancer. Once again the journalist uses the same technique as previously used to demonstrate the importance of the findings: she relates the finding of the gene to a high death rate among women with the gene. So, although, Nash does drift off here into rather specific description of the actual genetics, nevertheless she finishes by relating the gene once more to actual cases in the community.

Like the *Newsweek* article the Time article also discusses a test for hereditary breast cancer, but in this case the link with Myriad Genetics is made immediately and Myriad Genetics had been identified as being involved in the research right at the beginning of the article. This article then is much more explicit about the commercial motivation behind the research. Nevertheless it is made quite clear by the reporter

that a "foolproof test" may be some time, even years, away. It is interesting that the first mention of the test is made before the description of the complexity of the gene, after which discussion of the test then continues. Seen in this light, the fairly extensive discussion of the gene itself, which takes up most of paragraph 3, takes on another perspective. It functions here not as a narrative of science, explaining the pure research findings themselves, but rather as an explanation as to why the test might be still some time away. In itself it would seem to be part of the scientific side of the story, but in fact, positioned as it is between the first mention of the test and further discussion of the test, it acts to explain why the reader's hopes for a test may be dashed. Thus its function is actually social rather than scientific in the context of the article, though I suspect that the journalist's interest in the science has perhaps led her to include more than she needed to of the scientific side of things.

After the fairly long description of the actual genetics the story swings back to the clearly social side. Once again there is an actual quote from a prospective breast cancer sufferer who talks about having a mastectomy if any *BRCA1* test turned out positive. This is a fairly dramatic appeal to the social, with the mention of an operation which is very common and at the same widely feared. But such a quotation does make it seem that a test is a real prospect, while the original article hedges heavily at the end when talking about developing a test. Having raised the possibility of a test, though, the journalist is careful to note the problems involved if a test proved positive.

The narrative now swings back to significance of the discovery with *Time*, like *Newsweek*, careful to point out that only a minority of breast cancer patients will be affected by the discovery. But here the *Time* article diverges somewhat since it goes on to discuss how it had originally been thought that the gene would also be involved in non-hereditary cases. We have seen above that this is indeed the case and Nash goes further by insisting that it might still be the case that *BRCA1* could still lead scientists to other genes implicated in somatic cases. This is again closer to the narrative of science, since this is a quite clear case of Move 9, Significance for further

research. And indeed the article ends with mention of the search for *BRCA2* and other genes related to breast cancer.

This article then is much closer to the narrative of science than the *Newsweek* article. It shows that it is possible for a popular article to include a considerable amount of more 'scientific' material. The reason for the relatively great amount of emphasis on the narrative of science in this article may lie in two aspects. The first is that the journalist herself may be interested in the science and may be in this respect closer to the scientists than the 'woman in the street'. That this is the case may be indicated by the fact that Nash, the reporter concerned, was the author of a long cover story on cancer genetics which had appeared in April (Text 12, Nash, 1994). This latter story is almost like a review article in its comprehensive coverage of the research on cancer genetics. She is likely then to be interested in the science *per se* and appreciate its place in the narrative of science in much the same way a research scientist would.

The other factor leading to a more narrative of science approach is the journalist's need to make the story an 'important' one. Since the immediate practical effects of the research were rather limited the journalist is led towards finding other important aspects. As she has downplayed the 'race to be the first', which was the approach taken by *Newsweek* she is left with the importance of the science itself. In fact, there is one other side to the story that could have been, and was, played up, and again it is one which *Newsweek* emphasised rather more, and that is the testing aspect. Thus, although this story has rather more of the narrative of science than other such magazine articles, it is still concerned with relating the discovery to the lives of the readers. The side of the discovery that has most relevance to readers, the prospect of a test for the gene, is given quite a lot of prominence. As we have seen, the part of the article which at first sight seems to be most concerned with the narrative of science, the actual description of the characteristics of the *BRCA1* gene itself, in the context of the text functions as an explanation of why a test was not immediately forthcoming.

Once again, therefore, we see that, even in an article which superficially seems to be much more about the narrative of science, there is, nevertheless, lying behind it a

strong dose of the narrative of society. Although there is a fair amount of description of the science, the emphasis is still on the tests that may be forthcoming from the research for the benefit of the readership.

Comparison 7

Text 16

HUNTING A KILLER GENE

Brownlee, S. with Watson, T.

***USN&WR*, 26th September 1994**

This is the third popularization dealing with the discovery of the breast cancer gene *BRCA1*, in the third of the news magazines studied, *US News & World Report*. The approach taken here is radically different from the other two previously analysed articles and a style which is not often found in these magazines, although it is occasionally found in articles other than this one. In contrast to Nash's report, which, as we saw above, veers in parts towards a more 'scientific' rather than 'social' style of reporting, in this case Brownlee and Watson, the reporters concerned, take a radically 'social' approach. What they do here is frame the whole article in terms of answers to questions that women might have in the wake of the discovery of the new gene. Thus the whole of the science is discussed in terms of the relevance of the discovery to the lives of readers.

Like the other two reports this article is also based on the discovery by Skolnick's team and like the other two it mentions the intense competition to be the ones to discover the gene first. In terms of the metaphors used this article is a mixture of those used by the other magazines in that the title is similar to that of the *Time* article, whereas the mention of competitors and winners echoes the style of *Newsweek*. Oddly, there is a third metaphor at one point, where the discoverers are said to have "done their homework". It seems, then, with this mixing of metaphors at the beginning, that, since the bulk of the article is strongly social in approach and oriented to helping the audience to understand the implications of the article, there is not such strong pressure to find an overall metaphor to provide a framework. The framework

is instead given in the subtitle of the article: "How families should respond to the discovery of a breast cancer gene"|.

The slant of this article is definitely away from the purely scientific aspects of the discovery towards an emphasis on the development of a test. In fact the beginning of the article plays down the discovery itself, noting that "no one will be fully satisfied until other labs replicate the results". This would seem to be a nod in the direction of the supposed importance of replication in the advance of science, although, as Collins has argued (Collins, 1992), precise replication of a discovery is practically unheard of. A further reference to the real work beginning and the discovery as the first step towards unlocking the secrets of breast cancer also tends to downgrade the importance of the science of the discovery. The statement that the discovery is the first step towards finding the secrets of all forms of cancer would seem to be a rather strong statement, given what we have seen about the lack of linkage between *BRCA1* and non-hereditary forms of cancer.

In turning to the test, the article then lays out clearly that the test is not immediately available and may not be for at least two years. It then states overtly the problems with such a test, i.e. that knowing the gene is present does not mean that the cancer can be definitely prevented. Here the magazine is stating as starkly as it can the main implication of the discovery for the readership. It is also ensuring that they realise that the discovery of the gene does not mean 'a cure for cancer'.

The article then turns to answering putative questions that "women are asking" following the gene's discovery. There is no attempt to say who these women are or who is answering the questions. There is an obvious idealisation here, in that the article is taking the form of an interview when there is no obvious interviewee or indeed interviewer. However, this format enables the journalists to focus their article on, literally, questions which they think the reader wants an answer to. This format may also be used because some of the information used may have come from a press conference on the discovery of the gene, as a press conference photograph is used to illustrate the story. The personalisation of this Move 1 can be seen in the way the

questions are taken to be asked by a typical reader who is worried about the discovery and stated in the first person.

Like the *Newsweek* article the first answer, to the question on the chances of having the gene, gives a figure of 5% to 10% on the number of cases caused by an inherited gene, in contrast, as we have seen, to the Miki et al. article's figure of 5%. It is then stated that *BRCA1* is responsible for about half that figure, a reasonable approximation to the 45% of the original RA. But this is then followed by a statement that epidemiologists estimate that 1 in 200 women have inherited a mutated form of *BRCA1*, which is a figure of 0.5%. These differing figures are likely to cause some confusion in the reader in that there is a shift from the proportion of cases of breast cancer which are inherited, to the proportion caused by *BRCA1* and then the proportion of women who have a mutated form of the gene. The journalists here have juxtaposed a series of figures which could easily be misinterpreted by the naive reader.

Interestingly, the next paragraph, in answer to the same question, then gives some background information on the causes of cancer, so that the answer about mutated genes makes sense to the uninformed reader. A strong generalisation is given that all cancers are caused by mutated genes, coupled with what happens to cause a tumour to form. This is quite unexceptional in terms of the etiology of cancer, but it is not something that the reader necessarily knows and it is vital background information necessary to understand the importance or otherwise of the discovery. Such information is taken for granted in the other two magazine articles, perhaps because they are relatively shorter, but more likely because they focus much less on the implications of the discovery for the reader than this text.

The text then goes on to point out that the vast majority of cancer-causing mutations are not inherited, which is of course accurate. However, by saying that "mutations in *BRCA1* or other genes are not inherited" in most cases, this suggests that *BRCA1* is also implicated in somatic cancer, which is not the case. The vast majority of mutations in most genes are not inherited but in BRCA1 this is not so, since, as we

have seen, there is a mutation in one of the inherited alleles (Miki et al., 1994, p. 66). This part of the text then finishes by noting that scientists have not yet uncovered all the genes that can cause cancer, a statement which any oncologist would agree with.

The next paragraph then goes on to answer a question on the availability of a test. It notes correctly that a test is still some time away and again that the gene is comparatively large. Again this information about the gene itself is given in the context of an explanation as to why the test is still some distance away: because of the size of the gene it is likely to take some time to search. The text then goes on to say that researchers have "identified eight different mutations, each shared by members of eight different cancer families". However, the Miki et al paper (in Table 2, p. 69) identifies only 4 mutations, with another inferred. It specifically states that in three of the eight kindreds mutations were not found (Miki et al., p. 70). The companion paper by Futreal et al (1994) identifies a further 3 mutations, with a repeat of one of the mutations found in the first paper (Futreal et al., 1994, Table 2 p. 121). Although this could be said to be a total of eight mutations, the statement that they are shared by the eight kindreds would seem to indicate that this refers to the eight kindreds studied in the first paper, not the different kindreds studied by Futreal et al..

The article then goes on to suggest that "with luck" only a few more mutations would be found in the 130 cancer families known worldwide. What is interesting here is that the reporters seem to be unaware of the implications of the Futreal et al findings, since these indicate that the *BRCA1* mutation was found in families who were not one of "the 130 breast cancer families known worldwide". Hence even if "only a handful more mutations" were found in those families, this would not indicate that all possible mutations in *BRCA1* had been found. In their efforts to reassure their readers here the journalists seem to be putting a more positive gloss on events than was warranted. Although the basic findings were mentioned in the article by Nowak in the 23rd September issue of *Science*, the actual Futreal et al. article was not published till 7th October, the same issue as Miki et al.. Thus it seems that Brownlee and Watson did not know about the Futreal findings when they wrote this article.

In fact in the second part of the second paragraph of this section, they emphasise once more the difficulty in finding the mutations, with a quotation from Francis Collins, the head of the Human Genome Project. It seems that the journalists here are being constrained by the actual known difficulties of finding the mutations, but because of their desire to give good news to their audience are arguing at the same time in the opposite direction.

The answer to the next question on whether someone should take the test when it becomes available bears little resemblance to the original research. This is a straight Move 10, giving advice to the reader, with no direct reporting of the original story. The description of what constitutes high-risk is a clinical description which would be out of place in the type of biomedical research paper genre which Miki et al. is part of. This is just about the clearest example possible of straight medical advice to the reader. Readers are in fact advised to contact their local cancer centre or the National Cancer Institute. The article even goes as far as to give the telephone number of the National Cancer Institute.

The second part of the answer counsels women who might have the test regarding the problems of the test. The following paragraph spells out the genetic implications and the final paragraph points out that even if a woman tests negative this simply means that she has the same chance of contracting breast cancer as the rest of the population, and the actual odds, 1 in 8, are given. This whole answer reads like a piece of counselling of the kind that a woman who was thinking of taking the test might receive from a specialist in the field. At the end of the section, the likely source of the information and the reason for the feel of this section is indicated. An actual recommendation from a named genetic counsellor is given and it is likely that the rest of the section also came from this or similar sources.

This whole section, spelling out the potential implications of the research for the reader, is a very good example of the narrative of society. Not only does it bear no direct resemblance to the original RA in terms of the information it gives, but the type of information given is quite different to the type of information given in the RA.

Where the RA focuses on the details of medical genetics, the popularization gives advice to actual or potential patients. One type of information is detailed and scientific, the other is general and clinical.

The level at which the information is given is quite different. Where the RA discusses particular genes, codons and nucleotides, the popularization discusses patients, and not even specific patients but potential ideal types. Thus the discussion in the popularization is at the level of "Any woman who has more than one first-degree relative". Here we have an interesting example of how in fact the RA is not more abstract than the popularization, as often supposed, but the other way round. The RA is so specific at times, talking in terms of a single nucleotide change in a single codon in a single gene, that it is far outside the experience of the average reader. Difference of scale here translates into differences of perception for the lay reader and the expert. Whereas the latter can appreciate that a single nucleotide insertion can cause, for example, a frameshift insertion that disrupts gene function, the lay reader lacking the relevant background in genetics cannot possibly appreciate the significance of such a fact. We have to move up the scale several times before any information becomes meaningful to the reader. Thus we have a scale as follows: nucleotide insertion causes frameshift mutation, which causes disruption of gene function, which causes cell division to continue, which causes tumour formation, which causes breast cancer in individual. At this point there is a further shift, as this individual is only an example of a further 5% of the population, at which point it impacts on the reader, as she may be a member of that 5%. Even this kind of chain vastly oversimplifies the cell biology, as there are several other steps involved within the cell cycle, for example, which could be further enumerated (Weinberg, 1996). Thus the journalist has to scale upwards to the human scale where what has been found has an impact on the reader.

The next question moves back a little closer to the original paper in that it does discuss something mentioned in the original RA, the link between carrying the gene and actually getting cancer. The answer given that there is an 85% chance of contracting the disease is not taken directly from the Miki et al. paper but from general health statistics that we have seen quoted in the other popularizations as well.

However, the statement that there is at least one woman usually in families carrying the gene who does not develop cancer is mirrored in Miki et al. who state that in the four kindreds with mutations at least one woman lived until 80 without getting a malignancy. It is worth noting in this context that whereas this article, like the other popularizations, talks about a "breast cancer gene", the original RA talks about a "breast and ovarian cancer susceptibility gene". There is thus no suggestion in the original, even in designating the gene, that the gene inevitably causes cancer. Calling it a breast cancer gene, though, does suggest that it definitely causes cancer. More exactly, of course, the gene *BRCA1* codes for a tumour suppressor when functioning normally and only a mutant *BRCA1*, in conjunction with loss or inactivation of the wild-type functioning allele, leads to tumour formation. All of this more exact information in the original RA gets shortened to "the breast cancer gene, known as *BRCA1*" in the popularization. Here we have a clear example of how simplification almost inevitably leads to misunderstanding in the general audience. This misunderstanding then has to be cleared up by explanations in terms of a good and bad copy of the gene, as here.

In explaining the loss of the "good copy", however, the journalists go somewhat further than the original, which merely talks about other genetic or environmental factors. The magazine article, though, here talks about carcinogens in cigarette smoke as a potential factor, "or simply a random mistake". This is to be a bit more specific than the original, since the reader of the original can fill in potential environmental or genetic factors for himself, whereas the same is not true for the popularization. Nevertheless, the reference to "a random mistake" does presume a knowledge of DNA copying and the fact that errors can be made in copying.

The next paragraph is again straight advice on avoiding cancer and not related at all to the original RA. In fact it is even less related to the original RA than the previous strongly advice-oriented section since the first sentence gives advice on avoiding any breast and ovarian cancer, and indeed other cancers, such as lung cancer. This is general preventive medicine rather than anything specifically to do with the research

which lies behind the article. Even the specific advice for those in high-risk families is not directly related to the Miki et al. article.

The next question again bears no direct relationship to the original RA. The answer is different again, though, in that it is a direct quote from a physician. The reason for this is presumably that here the journalists were advising readers *not* to take the test and it was felt that such advice would be better heeded if it came from a doctor. The authority of the answer is also increased by the doctor's giving a reason for not taking the test – a negative answer would give a false sense of security. Once again, therefore, the article here is following the role of counsellor, in this indirectly via the quote from the expert concerned. It is noteworthy here that the doctor, although an oncologist, is not one of those involved in the discovery of the gene, or even in searching for it. This indicates how general the advice is and how unrelated directly to the actual discovery.

The answer to the last question, the standard one for any discovery in cancer research, whether it will lead to a 'cure for cancer', seems to be answered on the basis not of the original paper, but on interviews with other researchers in the field, or perhaps answers at the press conference. The source is put down at first to "researchers say", while at the end there is a direct quotation from Ray White, a researcher in the field who had led one of the competing teams. Thus the source for the whole paragraph is probably him; at any rate it is not the original RA, which does not discuss future research in anything like this detail.

Interestingly, in terms of the split between the narrative of science and the narrative of society, the article finishes with a separate boxed section. The box is headlined "A visionary who wouldn't give up" and describes the story of the search for *BRCA1* beginning with the work of Mary-Claire King and ending up with the latest discovery of *BRCA1* and even *BRCA2*. This is much more the narrative of science since it traces the advance of the science, though, focussing more on King than Skolnick's team. It is as if the editors, if not the journalists, had taken a conscious decision to focus the story very much on the narrative of society, but realised that some readers might feel

short-changed if virtually no news was given about the discovery. For the latter group of readers, therefore, they separated off the 'scientific' story in a box which stands virtually on its own. With the addition of an introductory announcement of the discovery, it would in fact stand completely on its own as a text, though, as I shall argue below, not as a popularization.

The structure of this story is again rather unusual in that literally the first half of the story is concerned with the work of King and only the second half with that of the teams who actually made the discovery. Again therefore it does not fit the typical structure of a popularization, even one which focuses more on the 'scientific' side than the 'social'. Indeed there are no social moves at all, which shows that this story does not in fact stand alone, but must be seen in the context of the overall story. Since the main story is so overwhelmingly 'social', with the bulk of paragraphs 7 to 14 being Move 10, this subsection can afford to ignore the social completely.

Such a division is a further indication that the distinction between narrative of science and narrative of society made here is a valid one. It seems to lie behind the editor's decision to separate off this section describing the history of the discovery, as it would not fit the kind of story that is being given in the main section. As indicated above, I have not analysed these subsections in terms of their move structure in Chapter 2, as these subsections are not truly separate texts, but on the other hand they are not part of the main text. However, sometimes, as here, we can see that there is a distinct difference in the type of move included in the subsection. It is as if the narrative of science has been excised from the main text and included in this separate section.

However, there may be another reason for this section being set on its own like this. As we have seen in Chapter 2, the main 'scientific' move in the genre is Move 2, Announcement of the new findings. Here on the other hand we have the first part of the text, as well as the heading, focussed on the work of King rather than on the new discovery. This text is anomalous, therefore, even it had been somehow included in the main text, although it is difficult to see how it could have been. It is not really a popularization in the sense we have been looking at but a background piece, giving

the background to the new discovery. The reason for this is obscure: at first sight it does not seem to be based on an interview with King. A photograph of King is used, but this is a file photo from an agency, not taken by a photographer accompanying an interviewer. However, on closer examination it seems that indeed the text is based on a number of interviews. There is a direct quotation from King herself, one not taken from a paper, as it is a reaction to the discovery of *BRCA1*. In addition there are details of expression like "it seemed to King" and "she and a graduate student" that indicate that the information came from an interview. One telling detail, though, indicates that it almost certainly came from an interview or a press conference. There is a reference to an academic conference in Cincinnati where King presented her evidence on the location of a breast cancer gene. This work is much more easily accessible in the form of the published paper (Hall et al., 1990) and would not normally be traced back to this conference. Secondly the reference to "the roar of a World Series game at nearby Riverfront Stadium in the background" is either journalistic licence or the product of personal recollection.

Similarly the reference to the later work mentions details of the finding of the gene that do not appear in the published version. It is said that Futreal, a postdoctoral fellow in Wiseman's lab, suggested a method which Wiseman opposed, but Futreal went ahead anyway. This is the kind of detail of the contingent repertoire (Gilbert & Mulkay, 1984) that never normally gets into print. The reference to Futreal as "Andy" Futreal also suggests personal contact, either in an interview or at a press conference.

It is interesting that the only member of Skolnick's team that is mentioned is Donna Shattuck-Eidens of Myriad Genetics. Indeed the research teams involved are described as being led by Mark Skolnick and Roger Wiseman. The main article has a photograph taken at a press conference with the person nearest camera and the only one identified being Roger Wiseman. This seems to indicate that the source here is Wiseman or Futreal and their part is played up in contrast to Skolnick.

The journalists here then have probably interviewed some of the main players in the discovery, but not *the* main player, Skolnick. It is good material but it is not

something that fits neatly into the main story, so it is used in a separate story. It must be admitted that this reconstruction is speculative, but it might explain why there is such an odd focus on an individual who is not one of those who has made the discovery. If there was a good interview with King then her part would naturally be highlighted somewhat more. As a result we get a section which is much closer to the narrative of science than is usual because of the interview with someone whose input to the discovery lay in the past.

Conclusion: The narrative of society

As the above comparisons show, therefore, there are considerable differences between the popularizations and the RA's. We saw in the previous chapter that in fact the whole generic structure of these articles was different, with a different move structure in the popularizations, including a series of 'social' moves. Moreover these differences in move structure are simply one aspect of a radically different approach to the writing of such popularizations.

The different approach is encapsulated in the phrase I have used, the 'narrative of society'. In using this term, I am following the narratives put forward by Myers (1990), but the term itself calls to mind the fact that the news article is widely talked about by reporters themselves as being a `story'. Although disguised by the inverted pyramid format, in which the most important information comes at the beginning of the article followed by increasingly less important information, journalists are in fact constructing a story for their readers (Keeble, 1994). This construction of the article as narrative, rather than the journalist just reporting the facts is emphasised too by Bird and Dardenne (1988/1997), who stress that the narrative format helps to fix the story reported in the minds of the reader. The use of the term 'narrative' then is meant to emphasise that the reporter is not simply reporting the hard facts but is constructing a tale which relates the scientific facts in a manner which makes them accessible to the reader. Thus the narrative form means that the article really is a 'story' in that it hangs together as a whole and has a beginning, middle and end (Myers, 1990; Toolan, 1988). In contrast, the original RA has a structure which is determined much more by

the accepted pattern of scientific argument, though even here Myers claims a narrative structure (Myers, 1990).

The narrative aspect of these articles can be seen at a glance if we look, for example, at the news article examined in detail above, *Cornering a killer* (Text 10). This article is written as the story of the discovery of the gene, beginning 4 years previously when scientists started to pinpoint the gene. The story then continues with the discovery of the gene highlighted in the article and then the possible test which could result from it and the significance of the test for women with hereditary breast cancer. The article finishes by saying that the story has not yet ended because other breast cancer genes may be found. The reporter even refers at the end to "the breast-cancer story".
A similar sort of pattern is found in one of the other articles studied on *BRCA1*, *Zeroing in on breast cancer* (Text 7). It also, as we have seen, connects up with the previous work by King and then talks about the discovery and possible test. Another aspect of the narrative frame often found in news magazine articles is more clearly seen in this text, however. That is the circular closing device, where the closing section of the story picks up again the example at the beginning used to draw the reader into the story. In this story, it is the case of Charlene Cunningham, the breast cancer victim who was introduced in a Move 1a) in the first paragraph, who is brought back again at the end to round off the story. This format is found not only in this type of science story but is very common generally in news articles of this type in such magazines, and it is clearly a narrative device.

These magazine articles, then, follow a narrative structure which is quite distinct from the scientific argument of the original article. The main difference in one sense, though, that is likely to strike even the casual reader, is one that I have not paid much attention to hitherto, except in the case of Comparison 3. That is the difference in length, in that most of the articles examined were much shorter in their popular versions than their originals. However, this is not criterial in that it would be quite possible, for example, to write a one-page summary of the original RA. This would not, though, produce a popularization in the sense we have been discussing. What is distinctive is not the length but the way the balance of the whole article is altered in

both its macro and micro aspects. At a macro level there is the change in the move structure discussed in the previous chapter and the move to a narrative format highlighted in the previous paragraph. In addition, there is the fact that the vast bulk of the information contained in the original article is omitted in the popularization in a way which cannot be explained just by the reduction in length.

If we look at the main RA we have considered above we can see clearly how and why this happens. Thus after an introductory section the Miki et al. article previously analysed has a structure with the following subheadings:

Identification of a strong *BRCA1* candidate gene

Germline *BRCA1* mutations in 17q-linked kindreds

Congregation of *BRCA1* mutations with *BRCA1* haplotypes and population frequency analysis

Phenotypic expression of *BRCA1* mutations

The role of *BRCA1* in cancer

We can see clearly here how the focus of the article is on the identification of the gene and the mutations involved. This is the main significance of the article. What the authors are doing here is convincing their fellow specialists in the field of cancer genetics, especially the other teams also looking for the gene in question, that they had found the gene. To do so they not only describe the methodology involved, they also specify in exhaustive detail the actual findings. Thus Figure 2 in Miki et al. is made up of a list of predicted amino acid sequences. The legend to the figure also gives the GenBank number where the whole nucleotide sequence is deposited.

So the bulk of the paper is actually made up of a description of their findings themselves, i.e. a description of what the actual gene looks like. To do this in a popularization would be impossible since the description is almost literally meaningless to a layperson. To the average reader of a news magazine the sequence described would be a list of letters which would seem completely random to him. There is no way, therefore, that the popularization can be anything but a

transformation of the original. To make any sort of sense of the RA the journalist must abstract away from the detail and find a level of description embedded in a narrative that impacts the life of the reader.

To do so means more than simply to find the 'human interest' in a story, although this traditional formulation does contain a grain of truth. Human interest stories, in the traditional journalistic sense, normally are concerned with telling the story of a particular individual. The narrative of society, even when it uses the example of a particular individual in Move 1a, is not concerned with this kind of human interest. Rather it uses the individual example to connect the research with a societal level which is understandable and relevant to the reader.

In theory the level that connects to the life of the reader could be one of many. Although the level of the individual nucleotide might be meaningless, there are various other levels that could be used. Thus, as Carey points out in his introduction to a collection of popular science writing (Carey, 1995), the scientific popularizer can aim at explanation or wonderment. But this kind of popularization is still basically concerned with the narrative of nature in Myers's terms. The writer is still only trying to get the reader either to understand nature or be amazed at it. The type of popularization we have here is doing neither of these things. What we have here is the journalist aiming to find that aspect of the science which most nearly articulates some aspect of a concern in the wider society. The issue may be a broad societal concern which may even have political implications, as we saw in the reporting of the Daling et al. paper. Or it may be more humdrum and have an impact which is more low-key. In every case, however, this type of popularization, can, at least potentially, have an actual effect on the lives of readers in a way which other types of popular reporting of science cannot have. The type of popular writing that Carey mentions, or that discussed by Myers under the rubric of the narrative of nature, has an effect at the level of understanding. It brings about a change in the reader's understanding of the natural world, although this change may be not only intellectual, but emotionally tinged as well.

However, the narrative of society type of reporting can bring about not only a change in understanding but also a change in behaviour and thus a change in society itself. Clearly, this is potentially true of any science reporting, in that, for example, a teenager may be inspired to aim for a career in scientific research because of a documentary on television. But the narrative of society may bring about a change which is not directly related to science itself but which instead takes place in the wider society. Thus a reader of the text on '*Warding off breast cancer*' may decide to take up sport regularly on the basis of reading the text and by so doing avoid contracting breast cancer. Or a reader of even the most 'scientific' article on the discovery of *BRCA1*, that by Nash, may be persuaded by the article to be tested for the gene in question.

Thus this type of science reporting is distinct from other kinds of narratives that might be used. A description, for example, of the discovery of a new subatomic particle (Begley with Holmes, 1994) is not likely to follow the narrative of society, as this is not likely to impact on the wider society outside the scientific community except in ways directly relevant to that community in terms of understanding of the phenomenon involved, though even this text starts off with a Move 1. On the other hand reporting of, for example, the discovery of a gene for obesity (Seligmann and Namuth, 1994) follows the same sort of narrative of society as the texts in the corpus.

The claim here then is that there is a specific form of science reporting, hitherto not specifically discussed, which focuses on the impact of the science on the wider society. This type of reporting, found in the corpus under investigation here, is quite distinct in what it reports if we compare it to the original scientific articles which report the science in the first place. It has a different move structure from the original research article and a number of moves, named 'social', which are specific to this type of reporting. This reporting, which I have termed the narrative of society, describes news which will have an impact outside the scientific community and which is potentially relevant to the wider society in that it may bring about change in that society which is broader than an increase in scientific understanding.

The idea that science and society are interrelated, however, has a broader pedigree than the study of the kind of popularization I have been looking at here. Social studies of science have looked at science from a number of angles, but especially influential in recent years has been the approach variously labelled the sociology of scientific knowledge (SSK); science, technology and society; or simply science studies. The following chapter will, therefore, consider whether these more broadly social concerns may shed some light on the linguistic type of analysis described hitherto.

CHAPTER 4

POPULAR SCIENCE AND SOCIETY

We examined in the previous chapter how popular versions of articles on science published in general news magazines differed considerably from the original research articles. These differences were not simply a difference in length, or the use of the inverted pyramid format, as claimed by Rowan (1989). In fact the inverted pyramid format is typical of the hard news newspaper article where the main points of the story are concentrated at the beginning with the rest of the text becoming less and less important as it goes on (Itule & Anderson, 1994). Since the type of article we have been examining tends to start with a personal example rather than going straight to the heart of the story, the inverted pyramid format is one which we can disregard as a candidate for the form of such articles. Similarly, these articles do not start with a summary lead in the style of most hard news (Itule & Anderson, 1994). Rather, as we have seen, the differences between RA and popularization may be ascribed to a difference in the purposes of the writers as a result of the different audiences appealed to. Thus the news magazine articles made a systematic attempt to relate the science they were describing to the wider society of which it was a part.

Social views of discourse

This type of approach I called the 'narrative of society', and in this chapter I would like to try and flesh out what I mean by this term and in what respect the type of writing I have mentioned is, in some sense, `social'. It is widely established that texts have a social dimension, in many different schools of discourse or text analysis. Thus the social dimension of discourse is highly salient in the work of Halliday and his followers (e.g. Eggins & Martin, 1997; Halliday, 1994; Halliday & Hasan, 1989). Another major school of discourse analysis where the social aspects of text come to the fore is that of critical discourse analysis associated with the work of Fairclough (1989, 1992, 1995a, 1995b) and others with the same overall orientation (Fowler, 1996; van Dijk, 1993; Wodak, 1996), and the journal *Discourse and Society*. Van

Dijk's editorial statement in the first issue of *Discourse and Society* encapsulates much that is distinctive of this point of view when he says that the focus is "not limited to ... typically academic problems but is also and explicitly directed at social and political issues" (van Dijk, 1990, p. 10). In another major article, van Dijk sees critical discourse analysis as focusing on "*the role of discourse in the (re)production and challenge of dominance*" (van Dijk, 1993, p. 249). Here we have a determinedly ideological view of language as intimately bound up with social identities and relations, with a typically Marxist dialectical view which sees language as both reflecting and constituting such identities and relationships (Fairclough & Wodak, 1997). A similar view, where discourses are seen as identifying the user as a member of a social group, is put forward by Gee (1990).

One need not, however, accept the viewpoint of critical discourse analysis, with its emphasis on unmasking the role of language in helping to shore up existing capitalist society, to recognise that texts have a social dimension. Thus it has been argued by Mey that we cannot understand text production without considering the text from the wider perspective of society (Mey, 1991). The social nature of texts lies in the fact that they do indeed reflect the discourse community that produces and consumes them, as we saw in Chapter 2. That writing an RA for a particular discourse community is a social act determined by the nature of that community, which then has effects on the writing has been emphasised, for example, by Hyland (1997), Gosden (1995) and Kaplan and Grabe (1991), as well as throughout the work of Bazerman (Bazerman, 1988) and other exponents of the New Rhetoric (Hyon, 1996; Miller, 1984). Thus RA's in a particular research field will reflect the purposes and priorities of the writers and readers of such texts.

Where we are concerned with texts with a wider audience, such as popularizations, though, it is not so easy to discern the discourse community involved. One way of looking at the matter is to see the journalist as mediating between public and private domains (Fairclough, 1995b). But this is, in fact, a rather asocial, atomistic view of the reader which sits rather uneasily with a social view of discourse. Although the reader may be an individual he is a representative of a wider group of readers who

have a social identity of their own distinct from the scientists who produced the text. The task of the journalist, therefore, is to bridge the gap between the discourse community of research scientists, the audience for the original RA, and the wider mass audience who will read the popularization. This change is a social one and will be reflected in the characteristics of the popularization. Thus we have the social dimension of the popularization.

Science news

Science journalists are of course quite aware that they are writing for a different audience than the original research community and that the importance of the research to that community is no longer the main issue. If we examine, for example, *A field guide for science writers* (Blum & Knudson, 1997) the recent "official guide of the National Association of Science Writers" of the USA, we find that journalists are conscious of the need to relate the science to the different audience. Thus Tanne, in explaining how to sell a prospective science story to a magazine editor, says "Finally, you must explain why this story is important for their readers" (Tanne, 1997, p. 21). Studies of 'gatekeeping' in the news media have also shown that the gatekeepers pay especial attention to the interests of their particular audience (Shoemaker, 1991/1997; White, 1950/1997).

In ascertaining what makes science news, Rensberger states his five basic criteria: fascination, size of the natural audience, importance, reliability of the results and timeliness (Rensberger, 1997). Of these, two relate to general newspaper values, in that the last two in the list speak of the need to make your story accurate and get it in on time, which apply to any newspaper story. The other three aspects are more specific to science reporting and they relate specifically to our concept of the necessity of relating to the interests of the wider society. The first, fascination, requires that the reader's interest be aroused. The second relates to "the number of readers who already know that they want to read about the topic" and cancer is even mentioned as an example. To ascertain the importance of a story idea, it is necessary, according to Rensberger to "try to decide whether the event, or finding, or wider knowledge of the

event or finding, is going to make much of a difference in the real world, especially in that of the average newspaper reader" (Rensberger, 1997, p. 11-12). Granted that Rensberger is talking about newspaper science writing here, his views are also applicable to the general news magazines which are under review in the present work.

Although we would not expect science journalists to have a fully explicit theory of popularization, it is, nevertheless, satisfying that the pronouncements of those who actually produce the science reporting are in line with the theory being produced here. This theory was set up on the basis of the texts under discussion, before this field guide was even published, so the standpoint of the reporters just cited stand as independent confirmation of the theory I have put forward, since they are quite consistent with this theory. As we shall see below, the science journalists' view of their work bears little relationship to the standard diffusion model of popularization.

If, then, the model of popularization being proposed does seem to reflect the standpoint of the working science journalist, then perhaps science stories in news magazines simply differ in topic from other stories in the magazines but otherwise reflect general news values. It would be unusual if science stories did not reflect wider news values at all, but this does not mean to say that they can be explained purely in those terms. A fairly standard set of news values is listed by McQuail (1994) as follows. News organisations prefer events that: have a short time span/are sudden, have scale and intensity, are clear and unambiguous, are unexpected, are culturally close to the intended public, have continuity and are already in the news. Some of these values are, of course, contradictory, e.g. for an event to be unexpected and already be in the news.

Generally, however, if we take a classic `scientific discovery' type of news story like the pinpointing of the *BRCA1* gene, we can see that these news values do not unambiguously apply to such a science story. Although the discovery itself might have had a short time span, scientists had been actively searching for the gene for several years. As regards scale and intensity, the story really has not much of either, since the gene is found only among a small group. Science is not at all clear and

unambiguous normally, as most discoveries are subject to further interpretation and dispute. Far from being unexpected, the discovery of *BRCA1* was widely predicted. Culturally, if we remain within contemporary Western culture, the discovery could hardly be further from the average member of the public. The discovery could scarcely be said to be already in the news since it had not been reported beforehand at all.

If we look more closely, though, we can see some relevance of such news values to our analysis. Although these news values do not, on the face of it, seem to apply to the discovery of the *BRCA1* and other scientific discoveries, nevertheless, what can be discerned is an attempt to construct the discovery so that it approaches closer to these values. The focus on the discovery as something sudden, which had led *Science* to lift its embargo on the paper, fits the first criterion. The scale of the discovery is widened by relating it to all breast cancers and its intensity heightened by the stress on the dire consequences of having the gene. The extreme complexity of the original article is focussed and drastically curtailed and the details of the article omitted in order to maximise the clarity. Although quite expected to the research community involved, the discovery was quite unexpected to the general public who did not keep a close watch on this field. Finally the whole aim of the popularization in terms of the model I am putting forward is to make the science less culturally alien to the readership and adjust it to the culture of the readers.

The popularization, therefore, can be interpreted in terms of adjusting a story which on the surface does not rate too highly in terms of news values so that it sits more comfortably with those values. It would be highly unusual if this did not happen and such an accommodation is quite compatible with the model being propounded. However, the popularization cannot be explained simply in such terms, as if we looked purely at the news values what would be predicted would be that the story would not be covered at all rather than that it would be covered but the coverage would bring the story closer to those values. Science news stories, therefore, cannot be explained on the basis of general news values. This conclusion is in line with Dunwoody's argument that we cannot look for news criteria which are universal and

define news consistently independent of the situation (Dunwoody, 1978/97). We need, therefore, to examine some more general theories of popularization of science.

The diffusion/distortion model

At first sight it would not seem too strange or unexpected to claim that popularization of science relates science to the wider society. However, the general approach to popularization, called by Lewenstein in his survey article on the communication of science in the media "the traditional model of science communication" is a simple one of "diffusion" or "dissemination" (Lewenstein, 1994).

In fact it is probably stretching the term rather far to call this a model, since it is nowhere fully worked out as a theoretical system. Rather it seems to be implicit in the type of content analysis approach used by many in journalism and communication in analysing science news. In content analysis a survey of the content of newspapers is conducted in quantitative terms and conclusions on, for example, the amount of science explanation in science stories is given in percentage terms (Long, 1995). A typical recent example of this approach may be seen in Pellechia's analysis of trends in science coverage in three major US dailies in the sixties, seventies and eighties (Pellechia, 1997). This investigated whether reporting of science had increased over the decades studied, whether coverage had become more comprehensive and whether reporting of methodology had improved. Thus this approach focuses on the general content of a corpus of articles in a quantitative way and tends to look at the communication in terms of 'errors' and 'accurate reporting'. For example, Moyer et al. (1995) examined how the popular press reported breast cancer and mammography research and reported findings which "indicate substantial inaccuracy in the translation of health research into popular print" (Moyer et al., 1995, p.157). They were able to categorise the popularizations as containing 10 different types of error, ranging from a misleading title to overgeneralising findings.

Similarly, another investigation of cancer in these terms, by Freimuth et al. (1984) criticised press reporting of cancer for a lack of detailed information, not providing

statistics on the incidence of cancer and not focussing on those aspects of cancer where surveys had found the public "ill informed". Their general approach may be gauged from the fact that they chose to study reporting of cancer because of the "established chain of scientific information flow" with "an organisation designed to facilitate this information flow" (Freimuth et al., 1984, p. 62). In fact the organisation designed to facilitate the information flow was none other than the Office of Cancer Communication at the National Cancer Institute. Here we see clearly the conceptualisation of science reporting as facilitating a smooth flow of correct information from the experts to the general public. Even those who criticise such a simple view of popularization, such as Logan, (1991), nevertheless only argue for bringing a wider variety of opinion into the process. That this is oversimplifying matters can be clearly seen in Bell's work looking at the public perception of climate change and thinning of the ozone layer. He notes that most media discussion of the issue tended to be accurate, but that, nevertheless, public discourse tended towards the extreme (Bell, 1994).

More recently the limitations of content analysis and its lack of theoretical rigour have been criticised by Evans and Hornig Priest (1995) who point out the problems involved in an uncritical reliance on concepts of accuracy and the value judgements implicit in the focus on 'inaccuracies'. Nevertheless the type of model of communication inherent in the content analysis studies lies behind most studies of popularization of science. This is what Hilgartner calls "the dominant view of popularization" - a two-stage model whereby the scientists give a genuine objective description of the facts of nature which is then simplified for the general public (Hilgartner, 1990). This leads to what Gross (1994), following Fahnestock (1986), called a "rhetoric of accommodation" which adjusts the science to the intellectual limitations of the public.

Public Understanding of Science

Another school of thought as regards popularization is from the perspective of what has been called the "public understanding of science" (PUS) (Wynne, 1994). Traditionally knowledge of science by the general public in this framework has been

seen in terms of a "deficit" (Durant et al., 1992; Irwin & Wynne, 1996; Wynne, 1994). Thus the public has been considered to have insufficient knowledge of science, which must be made up by better education, more popularization, greater attention by scientists to getting their work across to the public, and so on. This conception of the public as 'not knowing enough about science' is epitomised in the type of public opinion survey carried out by John Durant and his colleagues (Durant et al., 1989, 1992). In such surveys the general public is quizzed on what is considered general scientific knowledge and their knowledge, or rather lack of it, is then assessed.

From such surveys it is discovered, for example, that over half the respondents think that antibiotics kill viruses as well as bacteria, or almost a third think that the earliest humans lived at the same time as the dinosaurs (Durant et al., 1989, 1992). The result of questionnaires like this, therefore, is that the general public is seen to have considerable ignorance of science. To counterbalance this ignorance, though, the public is often seen as being quite interested in science with high levels of (self-reported) public interest in science (Normile, 1996). This relates to Miller's conception of the public being divided into attentive, interested and uninterested in science, with those in the first two categories making up 20% of the American population in each case (Miller, 1986). Miller, who carried out the NSF Science Indicators surveys in the US, which are a companion to Durant's work in the UK, sees the attentive public as being those who are both interested in and have knowledge of science, whereas the interested public have an interest in, but not knowledge of, science. The task of those concerned with PUS, therefore, is seen as to increase the interest in and knowledge of science among the public.

There are a number of problems with these surveys, as Durant et al. recognise to some extent (Durant et al., 1989). Although there is a fairly good correlation between reported interest in and informedness in such areas as sport, politics and films, this correlation breaks down in the case of science, where the reported level of interest is not matched by a corresponding level of informedness. The authors dismiss such a lack of correlation as being a result merely of respondents giving the expected answer, because respondents who said they were very interested and very well informed

tended to perform better on tests of scientific knowledge. However, this does not preclude respondents giving the kind of answer that was expected of them.

A more fundamental weakness of the questionnaire approach is in their attempt to measure what it means to study something scientifically. Such a question would be found difficult to answer by many philosophers of science, and the answers accepted are, by Durant et al.'s own admission, "quasi-popperian"(Durant et al., 1989). In terms of looking at understanding of science, therefore, this approach is a rather positivist one, whether we consider the process or content questions, with a conception of what are the 'correct' answers, which are known by scientists and not, very often at least, by the general public.

Another aspect of PUS that helps to highlight the conceptualisation of scientists and public involved is the general call for scientists to involve themselves more in popularizing their work. These outreach programmes strive to get scientists to explain their work directly to the general public. For example, the Royal Society's Committee on the Public Understanding of Science runs a National Week of Science, Engineering and Technology every year, where scientists might even be found in a shopping centre talking about their research (Pearson et al., 1997). On the other side of the Atlantic a more ambitious example of an outreach programme may be seen in Baltimore, where scientists working at the Center of Marine Biotechnology at the University of Maryland can be watched at work and explain their work to visitors to the Columbus Center, a newly constructed science museum (Olson, 1997).

These are examples of a way of looking at the popularization of science which tends to see the public as lacking in knowledge, but with a potential interest that can be met by more imagination and effort on the part of scientists in promoting their work.

Critics of this approach, such as Wynne (1994), Irwin and Wynne (1996), Michael (1996), or Cooter and Pumfrey (1994), tend to see it as being one-sided, with the experts on one side and the non-experts on the other. In fact, Cooter and Pumfrey, in their survey of the history of popular science and the popularization of science, are rather doubtful about the whole concept. They see science in terms of two kinds of

science, an elite 'official' science with at the same time also a different kind of unofficial popular science. So on the one side we have the intellectual history of elite science and on the other the social history of popular science. Historians of science have been reluctant to trace how their official science became a part of popular culture, so there has been little emphasis on the popularization of science. They see PUS as having stepped into the breach with a traditional formulation of popular to mean ignorant and passive.

Wynne (1994), Irwin and Wynne (1996) and Michael (1996) criticise the PUS standpoint for generating a view which not only sees the public as ignorant but which idealises both science and the public as unitary wholes, when in fact both science and the public understanding of it are much more complex and problematic. Both need to be seen in terms of the social context involved and when we do so it is clear that the public is not a monolithic ignorant block opposed to the pure truth of science, but both groups represent different social interests and norms.

The PUS model, therefore, tends to focus on the reception of science by the public and to construct the public as being ignorant of science. Altering the angle slightly, some commentators, such as Green (1985) and Hilgartner (1990), have looked at the actual changes in texts involved in going from research science to popular science. They see the traditional view not so much in terms of diffusion but in terms of distortion. In communicating science to non-scientists "the content of the knowledge becomes degraded, so that it is distorted and less true" (Green, 1985, p. 139). That the media distort scientists' discoveries, often in the form of overblown claims, is also suggested by Cohen, looking at media reaction of recent advances in AIDS research (Cohen, 1997). This type of coverage goes much further back, though, as can be seen from Patterson's history of cancer research and the recurrent claims to have found a 'cure for cancer' (Patterson, 1987). In fact, the two views, popularization as diffusion versus popularization as distortion, do not differ very much. The first is the ideal as conceived for successful popularization, while the second is what happens in practice.

Hilgartner then proceeds to what in a sense is an even more radical critique of the popularization of science than the work of Wynne. Whereas Wynne is concerned to reconstruct the public as being not just an ignorant mass, Hilgartner goes further and in a sense attacks the whole concept of popularization. His point is that there are not just two contexts involved, the research article and the distorted popular version. Rather there is a wide variety of contexts ranging from the highly specialised to the highly popular, from informal discussion in the lab to a mass media account. If we could identify the point where the unsullied pure science is created then anything downstream would be popularization, but this is impossible.

Both of these critiques, then, are concerned with deconstructing what had been a generalised simple concept: Wynne critiques the concept of the public as ignorant, Hilgartner popularization as distortion. There is one further part of the equation, though, that we have not yet examined in detail and that is the science itself, and that, of course, has been subject to the most radical critique of all, to which the positions of both Wynne and Hilgartner are related. It is impossible here to do full justice to the range of viewpoints variously labelled the sociology of science or the sociology of scientific knowledge (Woolgar, 1993), or sometimes just science studies, but their effects on this field have been such that it is necessary to try and trace their influence and to take into account the viewpoints involved in any attempt to provide a model of popularization of science.

The Sociology of Scientific Knowledge

Hilgartner's views of the range of contexts of science and the difference even between informal discussion in the lab and the research paper derive ultimately from the classic work of Gilbert and Mulkay (1984). They argue that there can be no one true accurate account of scientists' work because of the inconsistency of viewpoints and contexts. Their work is especially interesting in our present context since they emphasise the varying discourses involved. Although the word 'discourse' is used with almost as many different meanings as the word 'meaning', and it would be foolish to equate the discourse of Gilbert and Mulkay with the concept as used in more purely text-oriented work in linguistics, nevertheless there is sufficient

commonality to pay attention to the viewpoint, based as it is on considerable empirical work in the laboratory. The basic contrast they make in discourse terms is between the completed formal research article and informal discussion in the lab, which they distinguish as constituting two different repertoires, the empiricist and the contingent, respectively. The final published research article, therefore, is constructed as an unproblematic description of the empirical evidence, with all traces of doubt, inconsistency, speculation, personal characteristics etc. erased. But there are no objective criteria by which this account should be compared to the speculative, doubting, personal account of the informal interview, the contingent repertoire.

Thus we have a conception of the research article as only one of a number of possible accounts of the scientific work involved. From the perspective of the present work what is interesting is that there does not seem to be any reason to privilege the research article above that of any other communication. We can see, therefore, why Hilgartner might want to criticise the view of popularization as distortion, since if the research article is not privileged above other accounts then variation from this viewpoint cannot be seen as distortion.

We have here, therefore, a view of science as a discourse construction, and one which can be looked at in discourse terms. Probably the most famous view of science as discourse is that of Latour and Woolgar (1986). They consider the laboratory as a factory for producing papers and scientific instruments as "inscription devices"; laboratory activity is seen as "the organisation of persuasion through literary inscription" (Latour & Woolgar, 1986, p. 88). The work of the laboratory is explicitly characterized in linguistic terms - the transformation of statements from type 1, i.e. speculative assertions, to type 5, accepted facts, a fact being nothing but a statement with no modality and no trace of authorship. Thus facts are constructed rather than discovered and a scientist's aims in his or her work is summed up as follows: "If facts are constructed through operations designed to effect the dropping of modalities which qualify a given statement, and, more importantly, if reality is the consequence rather than the cause of this construction, this means that a scientist's activity is directed, not towards "reality," (sic) but towards these operations on statements." (Latour & Woolgar, 1986, p. 237).

This is a radical view of science which must be of interest to any viewpoint on popularization. Science is seen not as an attempt to uncover an objective reality but as an attempt to have one's statements accepted by others. If what is being produced in scientific research is not an objective description of nature, but a series of persuasive statements, then just what do popularizations consist of? They too cannot be seen, in this framework, as any sort of description of nature, but rather too as attempts at persuasion. The term used by SSK writers to describe science, therefore, is not description, but representation, which suggests a discourse portrayal which is constructed by the analyst, but does not imply anything actually existing outside that construction. In fact popularizations can be seen as not even professed representations of nature, but representations of representations. The implications for any view of popularization based on either a 'diffusion' or a 'distortion' model are clear. Neither view is at all compatible with a view of science which sees science as constructing reality rather than discovering it.

An objection at this point might be that we can safely ignore such a view of science as it is such an extreme viewpoint, which is not held at all by most scientists. The recent 'Science Wars', as they have been called, as well as the earlier polemic from Gross and Levitt, bear out the idea that most researchers working in the natural sciences do not hold such a view (Sokal, 1996a, 1996b; see also Bricmont, 1997; Gottfried & Wilson, 1997; Gross & Levitt, 1994; "Science Wars", 1997). However, this view is commonplace among social scientists involved in the study of science. The purely conventional nature of scientific knowledge is termed a "current orthodoxy" by Barnes and Edge in their book of readings in the sociology of science (Barnes & Edge, 1982, p. 5). Even Stephen Cole, a lonely dissenting voice surviving from the preceding Mertonian era of the sociology of science, also admits the dominance of such a social constructionist viewpoint among most current sociologists of scientific knowledge (Cole, 1992).

It is necessary, therefore, to take the social constructionist viewpoint seriously, as it is the dominant approach in the field which is most closely concerned with the analysis of scientific research. Its focus on science as being constructed socially via language is also one which has major implications for any analysis of scientific texts. This does

not mean that the viewpoint must be accepted at face value. But it must be considered before we can put together any model of popularization of scientific texts.

This social constructionist viewpoint was originally put forward in what was called the strong programme in the sociology of science by David Bloor (1976). In his terms it is admittedly relativist, but according to Bloor this is simply a methodological relativism, in order that all forms of knowledge can be explained in the same way. If the relativism involved is purely methodological, a simple heuristic allowing the sociologist to look at all aspects of a problem in the same way, without adjusting his viewpoint according to whether a theory, for example, is correct or not, as stated in Bloor's second tenet for the strong programme, then there is no real problem. Certainly, this is what the relativism of social construction is sometimes taken to mean. For example, in the exchange between the physicist David Mermin and the sociologists Harry Collins and Trevor Pinch in *Physics Today*, ("Discussion of nature", 1997; Mermin, 1996; "Sociologists, scientist,", 1996) Collins and Pinch refer to relativism as to the rightness or wrongness of scientific research being "a methodological requirement of our work" (Collins & Pinch, 1997, p. 92). The work in question, however, *The Golem: What everyone should know about science*, does lend itself at times to the sort of criticism that Mermin puts forward (Collins & Pinch, 1993). Thus Mermin attacks Collins and Pinch, for example, for saying that "Science works the way it does not because of any absolute constraint from Nature, but because we make our science the way that we do" (Collins & Pinch, 1993, p. 138; Mermin, 1996, p. 11). But just above the quotation that Mermin cites, Collins and Pinch say that "Nature imposes much less of a constraint than we normally imagine" (Collins & Pinch, 1983, p. 138). This, though, suggests that nature does impose some constraints, so it is not an out and out relativist position. The argument between Mermin and Collins and Pinch comes down to the extent to which nature imposes constraints.

In his argument with Mermin, and in his more popular work with Pinch, therefore, Collins is committed only to a methodological relativism. This methodological relativism is again put forward in his own more specialised work, where he argues that sociology of scientific knowledge needs relativism at a methodological level

(Collins, 1992). However, in this latter work he goes on to suggest a stronger form of relativism, relativism as an "output from", rather than "input to", the sociology of scientific knowledge, as he puts it: "while sociology of scientific knowledge does not *prove* relativism it does point inexorably in that direction" (Collins, 1992, p. 185).

Collins, then, does tend towards relativism as a description of science rather than simply a consequence of the strong programme methodology. For their part, although they nowhere say so explicitly, Latour and Woolgar also seem committed to a relativist rather than realist conception of science. They deny that pre-existing objects are discovered by scientists: "(r)ather objects...are constituted through the artful creativity of scientists." (Latour & Woolgar, 1986, p. 129). Woolgar himself is more explicit: "scientific discourse constitutes the character of the object it claims to be merely 'reporting'. The main conclusion is the constitutive point that the organisation of discourse *is* the object." (Woolgar, 1993).

This is a very strong claim about the nature of scientific discourse, that the discourse itself is not only the science but the object described. In other words that, to take an example discussed by Woolgar, pulsars did not exist 'out there' prior to discovery by the scientists concerned, but exist now because of their representation: "the representation gives rise to the object" (Woolgar, 1993, p. 65). As Woolgar notes, this radically inverts the normally perceived relationship between object and representation, whereby representation follows the object. Instead, different social actors with their different representations are conceived as representing different objects.

This view of science as representation is supported from the perspective of neo-Aristotelian rhetorical theory by Alan Gross, who sees the obsolescence of past scientific discoveries as difficult to explain from a realist perspective, whereas if scientific theories are rhetorical inventions then their rapid change and obsolescence is quite understandable (Gross, 1990). Scientific truth is also seen as social in terms that practitioners of SSK would accept as "a consensus concerning the coherence of a range of utterances" (Gross, 1990, p. 204). Despite being framed in terms of a different overall theoretical framework, then, Gross in his conclusions does not differ

radically in his opinions on the discursive nature of science. Although Gross is rather more radical in his espousal of the rhetorical nature of science than other workers in this tradition, there is a wide body of work which demonstrates the rhetorical nature of scientific writing (cf. Harris, 1997 for the most important work in this tradition.)

A view of science as seeking a widespread consensus is quite consistent, however, with points of view which are quite opposed to the radical relativism of Gross or the proponents of SSK. Thus as far back as Ziman (1978/1991) we see a conception of science as aiming towards as wide a consensus as possible by rhetorical means, although Ziman still sees science as "reliable knowledge". A social model of science, therefore, with a belief in negotiation and discourse, does not necessarily entail a relativist model along the lines we have described.

Similarly, it is possible to have a relativist model of science without laying such a great emphasis on discourse, for example in the work of Pickering (1981, 1992), who sees SSK as too reductively concerned with discourse and not sufficiently interested in the complex practice of science. In the field specifically of cancer genetics this emphasis on science as craft and practice has also been put forward by Fujimura (1992, 1996). Another way of looking at the problem is seen in the concept of actor network theory, which does away with the distinction between the natural and the social and treats every actor in the drama equally (Callon, 1986, 1994; Myers, 1996). This attempt to take seriously Bloor's symmetry postulate is interesting, but for our theory of popularization, centred as it is on texts, it is, like the work based on the practice of science, not so central as the discourse point of view.

A social model of popularization, therefore, is not necessarily committed to a relativist viewpoint and attempts have been made to model the public understanding of science in a more socially realistic framework which avoids the deficit view, but at the same time does not go all the way as far as relativism is concerned. Thus Gross in a later work (1994) argues for a contextual model which takes into account both scientific and local knowledge as a replacement for the deficit model. This model had in fact already been prefigured by the work of Wynne and a good example of how the public response to science can be seen in social and cultural terms not as ignorance but as a

different understanding and framing of the issue is the latter's study of the reaction of Cumbrian hill farmers to scientific advice on radiation after Chernobyl (Wynne, 1996). Here he shows that outside scientists' commitment to certainty and control conflicted with the farmers' specific knowledge of the local environment. Each side stated their point of view in terms familiar in their culture - precision, control and standardisation on the part of the scientists, lack of certainty, adaptation and variety on the part of the farmers. The farmers were by no means ignorant of their local circumstances and in many cases pointed out to scientists why a scientific test would not work, as it went against the reality of how sheep acted on their farms.

So one response to the socially dismissive nature of the deficit and diffusion models is to provide a 'thicker' picture of the actual practice of public understanding showing how the public is not in fact ignorant of science but frames science in different terms. This qualitative work is certainly valuable and can provide theoretically interesting descriptions of how particular groups, such as patients, may differ from the mass public in their response to scientific information (Lambert & Rose, 1996). Another approach is to see popularization as involving a different rhetorical strategy. Thus popularizations are sometimes seen as a type of story-telling with a Baconian inductive approach which contrasts with the typical discourse of research science (Curtis, 1994). Although this may be a rhetorical strategy used by some popular science writers in some contexts, nevertheless neither this nor the contextual approach amount to a different model of science popularization and we are still left with the question as to how face the challenge put forward by the SSK discourse model of science to the existing diffusion model of popularization and to any other model that might be proposed.

The narrative of society

The model that I am building up here, the 'narrative of society' model, also differs from the diffusion model in a number of important characteristics. We have seen that the diffusion model sees the task of the popularizer as translating the original work of the scientific researcher into terms that can be understood by the general public while preserving the 'correct science' of the original. It implicitly accepts that the original

research is describing an objective reality which should then be accurately described in simpler terms to those who cannot understand the original because of their lack of knowledge of science. At this point the diffusion model therefore links up with the deficit model of the public understanding of science. Sometimes, unfortunately, this description for the public is not done properly and distortions and inaccuracies intrude into the popularization, giving rise to the distortion model.

Myers, in his narrative of nature/narrative of science model (Myers, 1990, 1994), as we have seen, already implicitly criticises the diffusion model in that he is conceptualising the original RA and the popularization as not attempting the same thing, which the diffusion model assumes they are doing. My model goes along with Myers in this respect and has tried to indicate why it is impossible to conceive of the original RA and the popularization in news magazines as both being simply concerned with the diffusion of the same message. Rather the two are doing two different things. Where the RA is attempting to put forward new scientific claims and get them accepted as new by the community of scientific researchers in the field, the popularization, I have claimed, is trying to find those aspects of the research which will have maximum relevance to the wider society and demonstrate that relevance.

Difference in aims was then correlated with differences in move structure (Chapter 2) and also with a large number of specific differences between original RA and popularization (Chapter 3), all of these differences bringing about a more specifically social orientation. Such differences are completely incompatible with a diffusionist perspective and it is clear from Myers' analysis, my analysis and the discussion of the diffusion model above that it is inadequate. But we must try and measure up the narrative of society not only against the inadequate diffusion model but also against the arguments we have seen put forward by the SSK theorists. SSK, as was noted above, has been applied specifically to scientific research, but its arguments apply in general terms to popular science as well. If science is simply a discursive representation then *a fortiori* popularizations are as well.

At first sight it might seem that the argument can just be accepted *tout court*. Popularizations are clearly representations after all, in the sense that they are

derivative of other texts in a way which original science RA's are not. However, I have tried to show that the popularization is not simply a version which has been made simpler for a different audience who cannot understand the science. It is a different text entirely. However, if we consider the nature of representation as conceived in SSK terms we may begin to see interesting implications for a theory of popularization.

The argument from different repertoires put forward above by Gilbert and Mulkay (1984) would seem to suggest that the RA should not be privileged in any way, and that the popularization must be seen as just as valid a description as the original research article. Just because it is less formal, rigorous and has less empirical precision does not make it any less valid as a text. In itself such an equation of different descriptions is a telling blow against the distortion model, which devalues the popularization as inaccurate in terms of the original, and it cuts the ground from under the feet of a content analysis which relates the errors in the popular accounts of a body of scientific research. The different repertoires approach implies also not only that the less rigorous popularization is not only *ipso facto* no less valuable despite this, but that also there is no way that we can describe it as being a somehow 'inferior' description of the research.

However, the contingent repertoire described by Gilbert and Mulkay is concerned with informal interviews in the laboratory. The genre may be less formal and precise but the subject matter could be seen to be the same as that of the research article. On the other hand the content of the popularization, it might be argued, is seriously lacking compared to the original RA. But this kind of argument does not stand up. As with the popularization, the contingent repertoire of the laboratory is aimed at justifying the research in different terms to a different audience to that of the research article. The differences in style of argument described by Gilbert and Mulkay are put down to the different situations involved, fast-moving conversations in the lab to colleagues who know the field vs. the strict empiricist argument and situation precisely in the context of the research literature of the research paper. Likewise the popularization uses a different repertoire and argues in a different way to the different audience.

We have already moved away, therefore, from a simple diffusion model where there is a flow of information from RA to popularization. Notice the metaphor involved here – a flow involves movement downwards from high to low. The popularization is conceived as being on a lower scientific level from the original research, the scientists are up top, the general public down below. The two repertoires model of scientific discourse, however, puts the different repertoires on an equal level. Hence by extension the two repertoires of RA and popularization can be seen as operating on the same level rather than two different levels.

The view of science as discourse put forward by Latour and Woolgar has even stronger implications for a model of popularization. While Gilbert and Mulkay are still operating with a conception of a background reality and are simply saying that we cannot distinguish between two different discourses as appropriate descriptions of that reality, Latour and Woolgar do away with any conception of a reality which can be more or less accurately described. Thus it does not make sense in these terms to say that the popularization is an inaccurate description of the science. What counts is whether it is a persuasive argument. As the original RA is an attempt to persuade the research community concerned that the statements of the article should be accepted as part of the body of scientific knowledge, so the popularization can be seen in this framework as an attempt to persuade the mass audience of the relevance of the reported research to their interests. In the broadest terms both the RA and the popularization are attempts to convince their respective audiences of the relevance of the information in the text to their concerns, whether it is the sequence of *BRCA1* or the prospect of a test for hereditary breast cancer.

In this perspective it does not matter whether the popularization has deviated from the message of the original RA, since the original RA did not have any objective truth to tell in any case. That the popularization does not contain 99% of the information in the original is not, therefore, a matter of omission or distortion but simply a matter of a different discourse for a different audience. It can be clearly seen how disastrous the implications of this viewpoint are for the diffusion/distortion models. On the other hand, it is not only not disastrous for the narrative of society model, it is just what would be predicted by the model. This model sees the popularization as constituting a

different genre which is aimed at a different audience and has different purposes. Therefore it expects and predicts considerable differences from the original. As we saw above, in chapters 2 and 3, these differences do in fact exist. Here we can see that such differences fit quite neatly with a view of science as discourse representation but that such a view of science is incompatible with the diffusion/distortion model of popularization.

Let us explore a little more fully some further implications of a discourse representation view of popularization. Latour and Woolgar argue that the aim of the authors of an RA is to have their assertions accepted as fact and all modalities, i.e. qualifications and statements of it being less than pure fact, related to that fact dropped by their colleagues "preferably by citing the paper in which it appeared" (Latour & Woolgar, 1986, p. 81). This concept of acceptance by the wider scientific community and the consensual nature of science has echoes, as we have seen, as far back as Ziman (1978/1991). Similarly Bazerman (1988) argues that the rhetoric of science is aimed at having the ideas of the paper accepted by the scientific community and this concept lies too behind the work of Myers (1990, 1991). For his part Sullivan (1996) shows how in putting forward a claim authors will affirm some aspects of the consensus but will challenge other aspects and thus move the consensus forward. A related area where the efforts to gain acceptance for scientific ideas by the research community is in focus is work on the widely recognised practice in scientific papers of hedging. Although there are difference in the models of hedging of Myers (1989), and Hyland (1996a, 1996b) they both agree that hedging is used in order to gain acceptance of the ideas in the paper by the scientific community and convince readers of the validity of new claims in the paper.

It seems to be accepted then that scientists in their papers are putting forward new claims which they wish to be accepted by the research community. What is the corresponding position of the journalist writing an article in a news magazine? The journalist is not putting forward a claim about the natural world. I have tried to show that what she is doing is putting forward a claim about the social relevance of the research reported. Thus when a journalist uses the social moves which I have discussed in the move structure of these articles she is positioning the research in

terms of its impact on the wider non-research community. If she were simply relating the new research, but making it simpler, as the diffusion model implies, there would be no need at all for the social moves. That the social moves in fact exist in all these articles is, however, by now quite clear.

If the aim of the journalist, however, is to have her readers accept that the research reported is not simply of scientific interest but also of interest to them because of its impact on their lives, then we should not blame the journalist for distortions and omissions. As we have seen, such blaming is a common position taken by content analysts and the distortion model of popularization (e.g. Freimuth et al., 1984). And if we look at the Miki et al. article analysed above, or indeed any of the other original RA's analysed, we have seen that the popularization is radically different. But this is to miss the point. Since the journalist is not aiming to get the scientific community to accept new claims about nature but to get the news magazine's readership to accept her claims of societal relevance, it is in fact quite natural that she should ignore most of the scientific argument.

What is being claimed here, though, is not just that the journalist emphasises those aspects of the scientific RA which will be most interesting to her readers. Of course she does this, but that is only what any journalist does in trying to write an interesting story. It is not even that she tells the story in a light which will make it more comprehensible to the reader; again that is only the old journalistic standby of 'find an angle'. This might explain, for example, why a profile of Francis Collins, the head of the Human Genome Project, has a photograph of him sitting on his Honda Nighthawk 750 in motorcycle leathers (Nash, 1994). This is human interest but it is not the narrative of society. Rather the journalist is not interested in the research as such: the aim is not to tell about the research but to talk to the readers about their problems and concerns via the research.

Thus the article on the links between abortion and breast cancer (Text 18) is not concerned, as we have seen, with detailing the research but with addressing the concern of the woman who has had an abortion as to whether her chances of getting cancer are increased. Text 6 on exercise and breast cancer is about telling younger

women that they should exercise more as this has a protective effect against breast cancer. The articles on the discovery of *BRCA1* (Texts 7, 10 and 16) are not in fact on the discovery of *BRCA1*, as we have seen, but on the prospects of a test for hereditary breast cancer.

If we look back at particular articles examined in the previous chapter we can see in what respects the journalists focus on the concerns of the society rather than the science as such. Perhaps the most extreme form of this emphasis is when the journalist in question uses the science in the RA as a way of putting forward a position on a controversial social question. This, being the most overt form of the narrative of society, is relatively uncommon, but we have seen a clear example in the previous chapter. Thus in Text 7, on the link between abortion and breast cancer, we saw how the journalist, Christine Gorman, constructed the main researcher, Janet Daling, as being pro-abortion and thus distorting her data to fit this position. The journalist, unusually, is quite explicit in constructing her story in terms of the abortion debate, so here the societal angle is quite overt, while normally it is rather more in the background. This story, therefore, rather than being about the science of a new discovery, is about the abortion debate, new evidence on the pro-life side, and how a pro-abortionist has attempted to hide the evidence.

The extent to which this story is a construction by the journalist to fit the abortion debate perspective she has taken can be seen by comparing it with the *USN&WR* article, which does not take this perspective. Rubin, the journalist on this story, if anything plays down the relationship with the abortion debate, focussing much more on the personal health implications. This demonstrates the extent to which the *Time* journalist has *chosen* to make the story an abortion story, while the *USN&WR* has just as explicitly chosen not to.

Much more common in this corpus, and more common generally in this type of medical science reporting, is for the journalist to concentrate on the health implications of the science. Since it is biomedical research that is being reported this is always possible. Very often, however, it is basic biological research that is going on and the clinical implications are remote. The model would predict though that

wherever possible the clinical implications would be emphasised, since this relates to the concerns of the reader. In fact there are few things a reader would be concerned more about than his own health.

Thus we would expect that even theoretical basic bioscience, of the type that is reported in the most theoretical journal, will emphasise the actual, or more likely, potential, clinical applications of the science reported. By contrast the diffusion model would predict that if the original RA is about a particular gene, its sequencing, position and mutations, then so will be the popularization. It is evident from our discussion of the reporting of the discovery of *BRCA1* which model's predictions are borne out. As we saw above, there is much more of a tendency to report the prospective genetic test which might become available and the problems that might ensue with the test, than there is to explain the detailed genetics involved. Thus Text 7, *Zeroing in on breast cancer*, has 3 out of a total of 6 paragraphs on the direct medical implications, compared to the half a paragraph at the end of a total of 19 paragraphs in the original source RA. Even the more research-oriented Text 10, *Cornering a killer*, still has two of its six paragraphs on the medical implications, as well as parts of other paragraphs.

One main type of such reporting, therefore, is the focus on the eventual clinical payoff of the research rather than the actual basic science. Even when the payoff is still some way off and the original RA does not play it up at all, we see this kind of pattern. It is understandable why the original does not in fact do this. In the case of the *BRCA1* discovery, the aim of the Miki et al. article is to establish that what they have found is the gene for hereditary breast cancer and have that accepted by their peers, specifically the other laboratories involved in the hunt for the gene. Given the tentative nature of scientific claims (Hyman, 1996a, 1996b; Myers, 1989) there is great danger in going further than the evidence they have and making too strong claims. At that point in 1994 they were nowhere near a full-blown test for hereditary breast cancer. In addition there were the commercial interests involved. Premature discussion of a test would give potential competitors clues and might lead to invalidation of patent rights on which Myriad Genetics depended for their income. As we have seen, therefore, the original paper only hinted in the most general terms about a possible test.

By contrast the whole point of the exercise for those potentially suffering from hereditary breast cancer was the possibility of a test. A test for them could almost literally mean a reprieve from a death sentence, if they tested negative. If the test was positive, at least the clinical position was relatively clearer (Ponder, 1997). So the test was directly relevant to their lives in the way that the original research was not. It is evident therefore why the journalists chose to focus much more on the test – it provided a way in which the concerns of their readership could be articulated.

It might be argued that only a small minority of the readers of any of the magazines could possibly have hereditary breast cancer, afflicting as it did only 5% of all those with breast cancer, while the latter group make up only 10-12% of all women in America. This is clearly the case. But only a small minority of readers are ever directly affected by any story in this sense. What is more important is the construction of the story so that the reader is affected although not a member of the directly affected group. In this case this means that the article stresses that the gene is only carried by a small number and that somatic cancer is not related to this gene. Therefore, the huge number of readers who could potentially have been affected if the gene had been linked to somatic cancer are effectively ruled out as potential sufferers. But this does not mean that they are unaffected by the article. In fact they are affected, quite considerably. If, as researchers had originally expected, *BRCA1* had been linked to somatic cancers as well, this could have meant a huge step forward on the path towards finding a cure for cancer. So the article affects not only those who have the gene, but all those whom a different gene, i.e. a putative gene related to somatic cancers, would have affected.

Such stories, therefore, are constructed so as to appeal to the widest possible audience. The discovery of the *BRCA1* gene is described in glowing terms as a great step forward in the fight against cancer. Certainly, it was an important step in the genetics of cancer, but the clinical effects are limited to the test that is now available for hereditary breast cancer. There is an inherent tendency in such reports, because of the narrative of society, to exaggerate the impact of the research they report on society, a tendency that has been found throughout the history of cancer research (Patterson, 1987). It is routinely condemned by those who attack popularizations as inaccurate,

but it is readily understandable, as its genesis lies in the need inherent in the narrative of society to relate to the interests of the wider society.

Given that these popularizations, then, tend to emphasise the medical aspects which will have the greatest impact on the lives of their readers, is there any further pattern within this general picture? One striking pattern does emerge from the articles we have examined in the previous chapter: there is one subtype of article which strives to reassure the reader that what might seem like bad news is not so bad after all. Both Text 17, *It's not as simple as taking a blood test*, and Text 18, *Linking abortion and breast cancer* fall into this pattern. This is in line with the relentlessly upbeat nature of most reporting of cancer research in America this century, what the historian of cancer, James T. Patterson, calls "The Message of Hope" (Patterson, 1987). With the surge in research from the National Cancer Institute and related institutes after World War II, this led to a flood of 'cure for cancer' stories. Although contemporary reporting is more circumspect, it is still basically positive. Thus Text 18 reassures the reader who has had an abortion that the risk of getting cancer is not greatly enhanced at all. Similarly, the article on a test for ovarian cancer, while having to admit that there is no good test for the condition, and that if not picked up early the prognosis is not good, nevertheless is eager to point out that very few women get the disease.

Even more positive than this is the article which is able to give positive health advice to the reader. Text 6, *Warding off breast cancer*, is a good example of this type of magazine reporting. Here the article transforms the general epidemiological survey into a specific health prescription to take more exercise. This type of article clearly intersects directly with the lives of readers who fit into the group described in the popularization. More generally, the prevalence of Move 10a, Direct advice to reader, shows how important this aspect is.

This model of popularization, therefore, sees the journalist not as primarily trying to give an accurate account of the research, as implied by the diffusion model, but in trying to tell her audience about something which concerns them. Just as the RA is trying to get its audience to accept new claims about the science but must fit those claims into the existing picture, the journalist is trying to get her audience to accept

new claims about their situation and she must fit those claims into the existing situation. Move 1, therefore, for example, gives a picture of the existing situation of a patient. Thus in Text 7, on breast cancer, we have the example of a patient with breast cancer, as we have seen. Text 7 highlights in several respects those aspects of the discovery which will relate to the audience focus. Thus it talks predominantly about breast cancer, much more salient to the audience than ovarian cancer, although the gene is for hereditary breast and ovarian cancer. And, as indicated previously, it focuses much more on the prospective test than the original article.

So what the popularization is doing, in effect, is using the original RA as a jumping off point to discuss concerns of the readership. These are not just any concerns, of course. They must be related in some way to the topic of the research, but the research itself is not the prime concern. The most interesting area which the research raises for the readership is the prospect of a test for hereditary breast cancer and so that is what is focussed on. The details of how the gene was found etc, of vital import to the narrative of science, since that is how the claim to have the gene is substantiated, and thus accepted by the research community, is irrelevant to the magazine readership. It does not intersect at all with their interests or concerns and therefore, however scientifically interesting or important, it is ignored.

The whole series of differences described in the previous chapter therefore are quite explicable if seen in this light. There are these differences because the journalists were not concerned with reporting the original research article but instead with finding the aspect of the research, whether reported in detail or not, which would relate to the concerns of the readership. Rather than the popularization reporting the research of the original, what is happening is that the popularization is using the research to tap an area of concern of the readership.

Thus if we examine the ten types of errors found by Moyer et al. (1995) in their study of reporting of breast cancer and mammography, we can see, that in the light of the above perspective, rather than these being errors, they are predictable consequences of the shift in audience and purpose with the shift from the narrative of science to the narrative of society. We shall examine them, briefly, in turn.

The first error, a "misleading title" is a little misleading in itself. If we mean by misleading not giving a proper idea of the aim of the original research as conceived by the original researchers, then, indeed many popularizations may be misleading. But the aim of the original research is not the same as the popularization, so it is not misleading to give it a different title. In any case, many of the articles in this corpus have extremely explicit and precise titles, e.g. Text 2, *How safe is lumpectomy?* Or Text 11, *Do abortions raise the risk of breast cancer?*, which could not be said to be misleading. In many respects the titles are quite dissimilar from the original research articles, but this cannot be said to be misleading: use of the original titles or something close to them would be, not so much misleading, but rather leading to incomprehension. In short none of the titles in the corpus could be said to be misleading.

The second type of error, a shift in emphasis, we have already discussed in some detail. It is clear that the emphases of the popularizations and the originals are different but this is not an error, but the essence of the populariser's craft. The emphasis on the original RA is on, for example, detailing the sequence of a gene and a mutation, and by doing so convincing the research community of the accuracy of a claim about the status of the gene in question. The popularization is bound to alter this emphasis as the audience is different and the original emphasis would lead to a paper which was both unintelligible to an audience without the scientific background as well as irrelevant.

"Treating speculation as fact" and "omitting qualifications to findings" relate to the missing out of the hedging of the original RA. Clearly, where the original has information which is clearly speculative or qualified in some way and the popularization treats it as hard fact this is inaccurate reporting. Hilgartner notes, for example, that Doll and Peto's explicitly labelled speculations were reported without the hedging in many cases in the articles he studied (Hilgartner, 1990). The corpus under discussion here also shows a lack of hedging. This dropping of hedging, though, is understandable if we see hedging as a function of scientific politeness in Myers' terms (Myers, 1989) aimed at protecting the precise status of the authors' claims before their peers in the research community. Given the different audience

there is no need for the careful politeness involved. This audience is not going to attack the researchers for claiming too much. It is noteworthy, though, that scientists, nevertheless, are unhappy at seeing unhedged claims appearing even in popularizations, since some in the wider research community may read those claims.

As regards the spreading of erroneous information, we have seen that this does indeed sometimes take place, but it is relatively infrequent. In the most obvious case where this happens, in Gorman's misrepresentation of Daling et al.'s paper (Comparison 1), this is not so much erroneous scientific information, as erroneous reporting of the paper itself and was explained above in terms of the reporter's positioning of the paper in the abortion debate. Actual factually inaccurate reporting in terms of giving wrong information is not so common. Rather it is more a case of rounding up figures and giving a more rough approximation of results. Again this is clearly justified in terms of the audience, where precise figures would give no more information to an audience of non-scientists.

Although journalists do omit other important results, as claimed, this is again predictable from our viewpoint. The aim of the popularization is not to give a mirror image picture of the original and the journalist will focus on those aspects of the research which are deemed to be of interest and relevance. Other results, however, important they might seem to the researcher, may be irrelevant to the argument of the journalist writing a story in terms of the narrative of society. Thus the *BRCA1* popularizations did not report the type of mutations involved, because these results, although very important to other researchers involved, would not mean anything to the audience and were irrelevant to the discussion of the test.

The complaint about the omission of important aspects of the research methods is an especially interesting example of a confusion of genres. There is no reason why the popularization should be much concerned about the research method other than to convince the readership that the research is worth paying attention to because competently carried out. The audience for a popularization is not interested in the details of the research method as this does not affect their judgement on the relevance of the research to their interests. The audience for a research article may be interested

in the research methods, as this may affect whether they accept the results, but very often even to them the research method may be quite standard and uncontroversial.

The danger of overgeneralising findings was not much evident in our corpus, where, for example, it was carefully pointed out that hereditary breast cancer affected only a small percentage of the population. There was no instance of the general findings of the research being said to apply to a much wider fraction of the general public than they in fact did. What the journalists did do was to tell the story in such a way that it affected the maximum number of people, but this could be done without overgeneralising the findings.

Inaccuracies due to the obtaining of information from personal communication were again not much in evidence. Where it could be said to happen this would be quite explicable in terms of the different repertoires involved (Gilbert & Mulkay, 1984) when a direct quote from a scientist is likely to omit the hedging found in the research article, for example.

The diffusion/distortion model, therefore, is missing the point in detailing how the popularization does not give an accurate description of the research – it is not trying to. It is easy to show that the popularization misses out aspects of the original RA, so the content analyses have no difficulty in showing such differences. But the popularization is not even trying to give an accurate description of the original research article, never mind the original research. If we bear in mind what has been said above about discourse representation it would be wrong to expect the research article to be about making some sort of objective description of the research. Hence if the original RA is not about such a description, then still less is the popularization. The diffusion model therefore errs twice over in this regard, since it sees the RA as a mirror of nature and the popularization as a mirror of the RA, then it complains when the mirror is distorted. But even if the popularization is not about producing a mirror of the RA, should we not be worried if it gives an inaccurate picture of the RA? Is the distortion model correct in that respect?

However, distortion in such a sense is a misplaced concept. The metaphor of distortion implies that the popularization is giving a warped picture of the original

RA, that it should not do so but should let the reader see the original clearly, like a clear piece of glass. Here we have the old 'holding up a mirror to nature' metaphor of science as an objective description of objective reality. The popularization is being conceptualised as something which should interfere as little as possible with the view of the original RA. However, we have seen that the original RA is concerned with quite different aims than the popularization, so it is ridiculous to expect the popularization to be simply a copy of the original. In fact, to do its job, the popularization *has to* distort the original RA, since the aim of the popularization is to reflect the interests of the wider society.

Nevertheless, it is possible to see what the distortion model is getting at when it says that the popularization should not distort the message of the original RA. If we look at content analyses like Moyer et al. we can see what is meant by distortion: the popularization omits important findings, gives erroneous information, treats speculation as fact etc. If we summarise the kinds of `errors' reported we can see that behind this there lies a conception of a message which is being inaccurately transmitted by the popularization.

What we have here lying behind the diffusion/distortion model, therefore, is a code model of communication which sees messages as being sent by a sender and picked up by a receiver. For the message to be accurate the message must reach the receiver as it was sent by the sender. But messages can be inaccurate, either as they were sent by the receiver or received by the sender. Thus this model is based on a further model of communication, the coding model, and shares the assumptions of objective message and accurate transmission of the message with such a model.

But the code model of communication is not the only possible model and we shall examine in the next chapter an alternative model, which will enable us to give a more interesting picture of the process of communication involved in going from RA to popularization. This is the relevance theory model of Sperber and Wilson (1986/1995), which should allow us to examine the differences between RA and popularization in more varied and complex ways.

CHAPTER 5

RELEVANCE THEORY AND POPULARIZATION OF SCIENCE

In the previous chapter we examined the standard existing model of popularization of science, the diffusion/distortion model, and saw that it was deficient in several respects. One aspect of the model that is worth examining in more detail is the basic assumptions it makes about the nature of communication. As I hope to demonstrate, this is not just an abstract theoretical point, but has important consequences for how science is communicated to the general public. An alternative model of communication will then be proposed and some of its implications for the popularization of science examined.

It is clear from the criticisms of much popularization of science by researchers like Moyer et al. (1995), Freimuth et al. (1984) or Singer (1990) that they attack a lot of such popularization because the popularization does not transmit to the public the 'correct facts' of the science as set out in the original scientific publications. Thus the Singer study, among others, investigates how accurately the popularization reports the research as set out in the original RA. Use of terms like "information flow" (Freimuth et al.) also indicates a model of communication where there is a smooth transmission of the message from source to receiver. Thus the information flows from RA as a source to popularization as receiver and then from popularization as source to the public as receiver. The popularization is seen as an intermediary between the original research and the public transmitting the facts of the research with as little distortion as possible, so that in the ideal popularization the general public is given the same facts as the original research uncovered. This is evident in the criticism of errors and the focus on accuracy on the one hand (Singer) and the concern with areas which have been omitted from the popularization (Freimuth et al.) on the other.

The code model of communication

Now if we look at what such a concept of communication involves it is clear that what is implied is a model of communication where information is transmitted as efficiently as possible so that the receiver picks up the same message as was sent by the producer. The underlying model is the code model of communication classically put forward by Shannon and Weaver, a model based on telecommunications (Rogers, 1994). This widely utilized model sees the process of communication as involving the transmission of a message from a source in the form of a code via a channel to a decoder to its destination where it is decoded. In optimal communication what is decoded at the end is exactly the same message that was encoded at the beginning.

As applied to the popularization of science, the popularization is envisaged as decoding the message which has been encoded in the original, then recoding it so that the message can be understood by the general public. This is what lies behind the standard descriptions of scientific research as being full of jargon etc. In terms of this model, the jargon would be seen as `noise', which makes it difficult to pick up the original signal. The job of the popularizer is to get rid of the noise and allow transmission of the original 'pure' signal so that it can be decoded as transmitted at the other end. Thus the writer of the popularization functions as the decoder translating the message which has been encoded in an esoteric form by the original scientist. What happens is something like an intelligence service decoding the message sent by its spies. The message is received in an unintelligible form, but by the proper decoding procedure the underlying message is decoded and made intelligible. Notice what is being implied by such a model, though. The scientist codes the message in a form which is unintelligible to the average lay person. Therefore the populariser must decode it and re-encode it to make it intelligible. But what is being decoded and re-encoded is the same underlying message, though in a different form. It is this that lies behind the criticisms of popularizations as distorting the original. It is assumed that the objective is to recover the same message as transmitted in the original.

If the aim, therefore, is to convey to the general public the same message as communicated by the scientist, then it is easy to see why there is so much criticism of popular science as being 'distorted'. The distortion model is a claim that the signal that is being transmitted is not being decoded or re-encoded properly. Ideally, there should be no distortion, no noise in the communication process, and the same signal decoded at the end. If this does not happen, the distortion model blames the popularization for errors and omissions. An alternative is to blame the scientist for writing in too specialised and jargon-ridden a manner. In the first case the decoding or re-encoding of the popularization is at fault, in the second the encoding of the scientist. In both cases the actual model of communication is the same, a code model in which the underlying pure message is seen as being transmitted and received via various encoding and decoding modulations and demodulations.

What we have, therefore, is the following model of the communication of popular science (see Fig. 5.1).

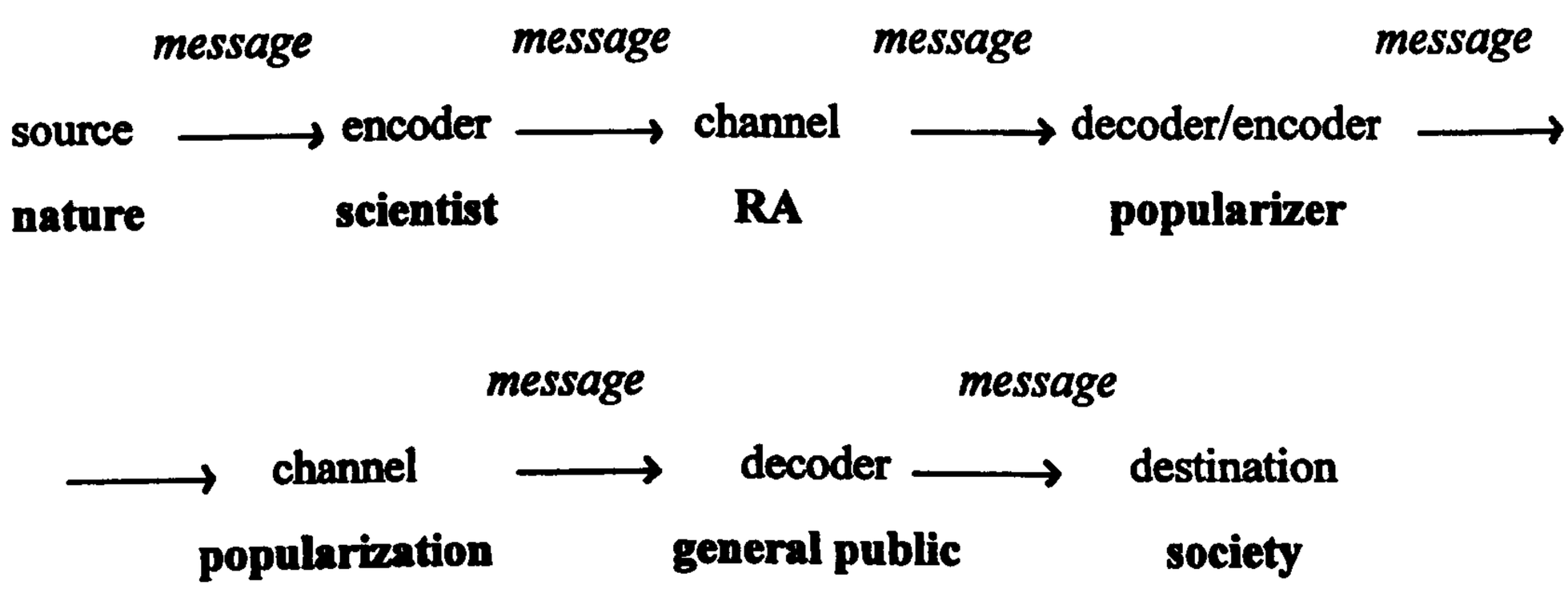

Fig. 5.1 The code model of the communication of popular science

In this model of the communication of science, the scientist is seen as communicating the facts of nature. She describes those facts in the original research article which is then decoded by the popularizer who translates it into a form which can be understood by the general public. The form is different, but the underlying message is the same. This message is then received by the general public. From the general public, the readership of the magazines, it then disseminates more widely into the broader

society. So although the message alters its form in its transmission from scientist via popularizer to general public the underlying message is the same. In fact the same sort of model underlies the scientist's work earlier, since the scientist is seen as uncovering the facts of nature and then transmitting them. So ultimately the transmission is not just from scientist to public, but from nature to scientist to popularizer to general public to society. Not too much need be made of this latter point, however, at this stage. What is important for us in this model is the idea that the popularizer is acting as an intermediary translating the scientific message into a form comprehensible to the laity. But like any translator she is envisaged as transmitting the same message and as keeping faithfully to the original message.

This idea of transmission of an original message, which may be coded in different ways in the original RA and the popularization, explains a lot about the way popularization is perceived in the diffusion/distortion model. Given this way of looking at communication, it is understandable that the popularization should be seen as trying to transmit the same message as the original. And if the popularization is seen as attempting to give the same message, then it is understandable if it is seen as distorting the message when it does not do so.

However, as we have seen above in the narrative of society model, if the popularization is not conceived of as trying to send the same message, then these criticisms do not apply. If the popularization is attempting not to transmit the same information as the original RA but instead to discuss aspects of the research in terms that intersect with the interests of the audience as part of the wider society, then the model of communication outlined is inappropriate as a model of science popularization.

The code model on which this model of communication is based is, nevertheless, widely held. As McQuail, in his standard work on mass communication theory, puts it: "At the core of the dominant paradigm can be found a particular view of communication as a process of *transmission* of a fixed quantity of information – the **message** determined by the sender or source." (McQuail, 1994, p. 49-50). Similarly,

the best-selling introductory textbook on communication, *Human communication*, by Tubbs and Moss, uses a basic model of communication in which a message is sent via a channel with interference distorting the message from sender to receiver (Tubbs, S.L. & Moss, S., 1994). In studies of the media too, code models are standard. Although Stuart Hall's classic *Encoding/decoding* (Hall, 1973/1996) is now somewhat dated, its influence and the code model which is encapsulated in the title still exert a strong hold on media studies, whether in theoretical terms (Krampen, 1997) or in practical analysis of the media (e.g. Fiske, 1987/1996).

If the model put forward here of the popularization of science, that of the narrative of society, is incompatible with such a code model of communication, then it is necessary to put forward an alternative model of communication which could be used to support the proposed model of popularization of science. Although, the code model of communication is probably still the dominant type of model if we consider all those concerned with communication, nevertheless most pragmaticians, following Grice (1975), would argue for an inferential model of language. The central place of inference is common in all those who take their inspiration from Grice, although some have been fairly critical of him, such as Gazdar (1979). Others are more in tune with Grice's work (e.g. Levinson, 1983), and most could be said to be following in Grice's footsteps although critical of certain aspects of his work, such as Leech (1983), Neale (1992), or Recanati (1991). Whatever their particular viewpoint, though, most of them work with what is basically an inferential model of linguistic communication. Probably the most important and fully worked out of such models is that of Sperber and Wilson's (1986/1995) relevance theory (henceforth RT). In what follows we shall examine how relevance theory may be used to support a model of communication which can underpin and give further backing to our model of popularization, illustrated by reference to one of the popularizations we examined earlier, Text 10, *Cornering a killer*.

Relevance theory

Strictly speaking, RT is not a purely inferential theory of communication. In fact it integrates aspects of the code model along with an inferential component. Since language is in some respects at least a code, in the sense that it involves pairing messages with signals, then in talking about the use of language for communication we need to involve a coding component. This is not to say, however, that language can be explained purely in terms of a coding process and it is this interaction of coding and inferential aspects that RT seeks to explain. Since, however, inference is seen as essential in all types of communication, whereas coding is involved only in some, the inferential aspects can be seen to be dominant.

For RT, therefore, communication cannot be seen simply as encoding a message which is decoded by the hearer so that the hearer gains exact access to the same thought as the speaker. This is the basic conception of the code model. In RT terms what happens is that the speaker produces an utterance which is then interpreted by the hearer so that some positive cognitive effect is attained in a context accessible to him at the time (Sperber & Wilson, 1995, p. 265). A positive cognitive effect may be additional information or it may be a reorganisation of existing ideas in a more useful way. In interpreting the utterance the hearer must first find the explicature of the utterance, and in doing so is guided by the literal meanings, for example, of the lexical items. So here a coding model is appropriate, at least in part, although inferential processes are at work even here. Thus the proposition will often have to go through a process of enrichment before a full explicature can be achieved and if this is sufficiently relevant then the interpretation will stop there. But if the cognitive effects achieved by this are insufficient to suggest that this is what the speaker intended to communicate then the hearer will proceed to generate an implicature which will bring about such a benefit.

An example will make this clearer. Let us say that John and Linda are on holiday in Bangkok and are discussing when to leave their hotel to catch their flight home. John makes the following comment: "It will take some time to get to the airport."

This is of course a true statement: it takes some time to do anything. However, it is so trivially true that it could not be the only thing that John wished to communicate, as it does not constitute any sort of new information. Linda will, therefore, need to assume that John wanted to do more than express the literal meaning of these words and she will form a more fully worked out explicature, i.e. 'It will take considerable time to get to Bangkok airport', or similar. In some cases this will be sufficient, but in many cases, as here, the hearer must then go on to infer the implicature intended by the speaker for the proposition to be relevant, here an implicature something like "We should leave in plenty of time."

The process of communication, then, involves the hearer in decoding the encoded meaning of the utterance but also in testing to see if such a meaning could have been what the speaker intended to communicate. Very often it will be the case that the literal explicature will strike the hearer as not being what the speaker intended to communicate and the hearer must infer the implicature intended. Although here I have been speaking about speaker and hearer, in principle the same process applies to writer and reader. What this means, therefore, is that we cannot assume that we can pick out the message intended by the writer by a simple process of decoding the lexis and syntax to reach the underlying message. Nor should we assume that what happens is simply decoding via a two stage process: first we decode the explicature and then we decode the implicature and that is the message intended by the speaker. Accessing the implicatures intended especially is a matter of inference rather than decoding.

How then does the hearer access what the speaker intended to communicate? Sperber and Wilson have a disarmingly simple answer here: "the first interpretation tested and found consistent with the principle of relevance is the only interpretation consistent with the principle of relevance" (Wilson, 1992, p. 177). What this means is that the

first interpretation that occurs to the hearer is one which will bring him a positive cognitive effect for comparatively little processing effort. Is this interpretation what the speaker intended to communicate? The hearer is entitled to conclude that it is, since if the speaker had intended him to access another interpretation she would have tried to ensure that this interpretation would be indicated by the form used to communicate. This follows from the presumption of optimal relevance, i.e. that the ostensive stimulus is relevant enough to be worth processing and that the stimulus is the most relevant one possible given the speaker's state at the time. Thus "if you find an interpretation that is consistent with the principle of relevance and therefore *might* be the right one then either it *is* the right one or there isn't any right one", according to Sperber and Wilson (1987, p. 747). In other words it is possible to infer what the communicator intended.

Notice though that although the hearer may end by inferring the communicative intention of the speaker, this is by no means the automatic process that it is in the coding model. Coding is straightforward and demonstrative: a one-to-one equivalence which is automatic. Inferential cognitive effects are of a different type. More particularly, Sperber and Wilson distinguish between strong and weak implicatures (Sperber & Wilson, 1986/1995; see also Blakemore, 1992, 1993). Thus strong implicatures are those which are strongly constrained by the utterance of the speaker so that the hearer is led towards the one interpretation intended. Weak implicatures, on the other hand, are much more the responsibility of the hearer and a whole chain of these may ensue, each more and more the hearer's responsibility.

This idea of implicatures differing in strength allows for a certain amount of indeterminacy in interpretation, where, if we are concerned with written texts, the reader will take responsibility for the interpretation, which may not be precisely intended by the writer. Now this is clearly the case with 'poetic' or 'literary' texts, where the effects of such texts cannot be clearly delineated. However, it is not just a property of literary texts, though weak implicatures may be more pronounced in this type of text. Thus the final sentence in the *Cornering a killer* text (Text 10) "Finally

we are beginning to crack open the mystery" may be said to produce a range of weak implicatures in the reader, rather than any one basic strong implicature.

Hence communication in RT terms does not involve transmission of a message but bringing about an effect in the hearer or reader. The reader may be able to infer fairly precisely what the communicative intention of the writer was, or the implicatures may be weaker. In either case what the writer does is bring about a cognitive effect in the mind of the reader rather than transmit a message. Thus if Nash in Text 10 writes of a member of a family with a history of breast cancer that "Already, Laurie Skelly is mentally steeling herself for the bad news that could come from such a test" for hereditary breast cancer, she is not simply transmitting a message made up of the proposition encapsulated in this sentence. What she is doing is attempting to bring about a certain cognitive effect or series of cognitive effects in the reader. First of all, the reader does not know who Laurie Skelly is exactly. In addition just how bad is the 'bad news' and what is the bad news? What kind of test is 'such a test'? Such factors indicate that there is more than a simple message being transmitted here.

How the reader goes about interpreting such a sentence is explained in RT terms by saying that the reader processes the new information in the most immediately accessible context. Context in RT does not mean the actual text but is a cognitive term, referring to the mind of the reader. However, in this case the most immediately available context is the information in short-term memory provided in the previous sentence. As Blakemore (1987) points out, in a connected text the smallest and most immediately accessible context will be the one produced by the immediately preceding text segment.

Since RT sees communication as bringing about a cognitive effect rather than transmitting an unchanging message, then there is no justification for evaluating a popularization in terms of its author's success in putting across the same message as the original RA. Given the different cognitive contexts involved, this would not have the same effect at all. At this point a proponent of the standard diffusion model of science popularization would be entitled to complain that what has been claimed here

is that the text of the original RA could not be understood by a member of the magazine audience. This, it might be said, is precisely why popularizations need to take place, because the original RA is too complicated and uses too much jargon to be understood by a mass audience. Once this is done, then the mass audience can understand the same scientific message. However, this is not what happens in practice. The popularization does not attempt to explain the same scientific message, as we have seen. The vast bulk of the original RA has no equivalent in the popularization. The popularizer is not some kind of translator transforming the RA from the language of the scientist to the language of the layman, different language but same message. Most sentences in the RA, in fact, have no equivalent in the popularization at all.

There are a number of reasons for this difference. At one level there is the trivial difference of length, in that the magazine popularizations are much shorter than the *Science* article. But much more important than this is the fact that such 'translation' would be impossible in most cases. The context of the reader of the magazine article is so radically different from that of the reader of the original RA, fellow research scientists in the field, that most sentences could not be 'translated'. Though it is possible for some of any such text, as has been seen above in Chapter 3, it is not true of the bulk of the RA. This is not to claim that it is impossible in principle to explain leading edge scientific research to the layman, merely that this cannot be done by such 'translation'.

Cognitive context and effects

To understand quite why 'translation' is impossible as a general popularization strategy we need to look a little more closely at what RT means by producing a cognitive effect. When an utterance is produced by a speaker the hearer presumes that the speaker is attempting to communicate with him and that it is worth his while to pay attention to this. If he does so, he is likely to make some cognitive gain, for example, to learn a new piece of information which will be of use or interest to him. The extent of this positive cognitive effect can be determined in terms of the idea of

relevance: "An assumption is relevant to an individual at any given time if and only if it has some positive cognitive effect in one or more of the contexts accessible to him at the time." (Sperber & Wilson, 1995, p. 265). The key point here is that new information is relevant if it can have some positive cognitive effect in an immediately accessible context, i.e. cognitive context. The problem is that for most information in an RA there is no immediately accessible context for the average magazine reader where this information could have an effect. To take an example practically at random from the Miki et al. article on the discovery of *BRCA1*, the sentence "Conceptual translation of the cDNA revealed a single long open reading frame with a presumptive initiation codon flanked by sequences resembling the Kozak consensus sequence." (Miki et al., 1994, p. 67) does not intersect with a cognitive context in the average reader. Therefore it produces no positive cognitive effects.

This is not simply because of the jargon involved. The magazine reader may not know, for example, what the 'Kozak consensus sequence' is. But it would not make much difference to him if he had it explained to him by the popularizer. The fact that the *BRCA1* DNA has a sequence which is similar to the Kozak consensus sequence is of interest to a researcher, since it helps to fit the new information into his existing cognitive pattern, but even if the Kozak consensus sequence was explained to the layman he still would not have an overall pattern to fit this information into and so it would have no positive cognitive effects.

The same argument applies to all of the RA texts. Since the aim of these texts is to advance the narrative of science by putting forward new research findings for appraisal by the research community, they are designed to produce positive cognitive effects in that research community. Specifically, of course, they are designed to produce acceptance of these claims by the community. The magazine audience, whose cognitive environment is radically different, will not react to the same information in the same way, as the new information will not intersect with the appropriate cognitive context to produce the desired effects.

At this point a diehard supporter of the diffusion model might want to argue that, of course, it would be more difficult for a reader of the magazine article to understand the scientific argument than a professional scientist, but that, with a bit of effort, it would still be possible. However, RT would claim that any communication will only have an effect if the positive cognitive effect gained is not offset by too great an effort involved to gain that effect. When we process any new incoming information, we are willing to put a certain amount of effort into this processing. If it seems likely that we will gain great cognitive benefit from this processing we will be prepared to expend a greater amount of effort. But the effort will always be commensurate with the likely positive effects. Thus, although it might be possible for a magazine reader eventually to gain a level of understanding that approached that of the scientific researcher, given his general cognitive context, i.e. his existing knowledge, the level of effort involved would be prohibitive.

Given the different cognitive contexts involved in the two audiences and the resulting different cognitive effects involved in a reader of the RA and the popularization, it might seem that what is being put forward here is a claim for the impossibility of popularization of science. But successful popularization does, in fact, take place, so we have to account for this. I would suggest that successful popularization takes place because effective communication is a result of interaction between what is communicated and the audience communicated to. What constitutes successful communication in RT terms is large positive cognitive effects relative to the small effort expended in achieving those effects. Good popularization of science, therefore, involves bringing about such positive cognitive effects in the field of science relative to a proportionately small effort expended.

How is it possible, though, for such communication to be successful given the fact that the cognitive contexts are so different? Where we have different communication situations, as in research science to the community of research scientists and popular science to a mass news magazine audience, there is no *a priori* reason to assume that the same communication can take place in both situations. Since what is communicated – the propositions in the text – and the contexts involved - those of

scientists and laymen – are different, there seems little reason to expect any sort of parallel successful communication in these two different situations.

That two different communicative contexts are involved, however, does not in itself mean that communication cannot take place. To some extent at least, the context for any particular acts of communication will differ in RT terms, since context is defined in cognitive terms. What is communicated is a proposition, this proposition being made up of concepts, stored at conceptual addresses in memory. Each conceptual address has three entries, a logical entry, an encyclopaedic entry and a lexical entry. Whereas each person's logical and lexical entry will normally be the same, the encyclopaedic entry for any concept will vary widely between any two individuals, since this entry includes our general world knowledge about that item. So the fact that context varies between individuals is a general feature of communication, and is not restricted to the communication of science, popular or otherwise.

Given this difference in cognitive context between any two individuals, then, how is it possible for successful communication to take place? RT answers that the speaker will more or less constrain the interpretation of the hearer, in the first place because of the Principle of Relevance, i.e. "Every act of ostensive communication communicates a presumption of its own optimal relevance." (Sperber & Wilson, 1995, p. 260). The hearer, therefore, expects the communication to bring about positive cognitive effects if attended to. Secondly, the context attended to is the smallest and most immediately accessible, that in short-term memory brought about by processing of the previous utterance. Therefore, despite the theoretically potentially massive differences in the wider contexts of any two people communicating, the narrow context in which an utterance is processed is fairly specific and defined, since it has been constrained by the previous discourse (Blakemore, 1988; Blass, 1990).

Nevertheless, despite these constraints, the fact that the encyclopaedic entries are so different in scientist and laymen is going to mean that there will be differences when the lay reader interprets a term in the magazine article compared to how a scientist will interpret it. To take a core term as an example, the encyclopaedic entry for DNA

will be completely different for the average lay reader and the professional researcher in cancer genetics. However, if we consider what happens when a proposition is processed this does not, in essence, matter. The reader processes the proposition expressed in the text to derive a contextual effect. But the reader does not need laboriously to work out the details of his knowledge of DNA, as in searching for the intended contextual effect the reader stops when he reaches an effect which brings a positive cognitive benefit for relatively little effort. In other words if a sufficient effect is obtained by processing DNA as something like "genetic material shaped like a double helix" then communication is achieved. There is no need for the reader to have the same encyclopaedic knowledge of DNA as the researcher has to obtain a sufficient cognitive effect. This is only to be expected, as it is simply an extension of what happens in everyday communication. Thus, if I tell you a story about my brother, and you know only that my brother is a bank manager in Plymouth and is related to me, you may still get the point of the story, despite the fact that my encyclopaedic entry for my brother is vastly richer than yours.

What is important in communication, therefore, in RT terms, is that the cognitive effect intended by the writer is accessed by the reader. It is not necessary that the reader share the background knowledge of the scientist. But if we look more closely at the idea of communication as the intersection of proposition expressed and context in which the communication is processed then further interesting implications for communication of popular science appear. We have seen that the code model implies that the same message is deemed to be communicated regardless of who is doing the encoding and decoding. However, even given this assumption it is admitted by proponents of the code model that popularization cannot use exactly the same message as the original RA, since it would not be popularization in such a case. Thus the original message must be altered. This means that there is a danger that the original message may be lost and we get the critiques of popularization as distorting the scientific message. Unfortunately, if we conceive of communication in terms of the code model such distortion becomes inevitable, since equation of communication with the message means that changing the message changes the communication. The most that can be expected within this model is that as close as possible an equivalent

to the original is found so that distortion is minimised. Thus we have articles like that of Singer (1990) detailing the different kinds of distortion involved.

In the framework of the code model there is no way out of this impasse. If we alter the original RA, as we must, then the new popularization will be a different message. The best that can be hoped for is that the new version, the popularization, will preserve as much as possible of the original with as little distortion as possible of the original message. That this is the ideal can be seen from the emphasis of articles such as those by Singer (1990) and Freimuth et al. (1984). If, on the other hand, we conceive of communication within an RT framework, then the outlook is not nearly so bleak. Since communication is the outcome not of transmission of a message but of the interaction between proposition and context, the interplay of the two allows for a different kind of communication.

If we take, for example, the second sentence in Text 10 by Nash, *Cornering a killer*, then we can see what the difference is. The sentence runs as follows: "Scientists knew that the gene - dubbed *BRCA1* - was somewhere on chromosome 17, but they couldn't pinpoint an exact location or identify the culprit's chemical makeup." The reader trying to interpret this sentence is faced with several points where, compared to the original RA, the information given is much less precise. Thus the original gives the names of at least some of the scientists involved, notes more precisely where the gene was known to be located on chromosome 17, and describes the "chemical makeup" of the gene in much greater detail. But the point is that for the reader of the popularization "somewhere on chromosome 17" is specific enough information. To locate it further as "on 17q21" would not actually provide any further contextual effects for the reader. In fact this seemingly more specific information would cause greater problems for the average reader as it would cause greater processing difficulties, as the reference to one arm of a chromosome as 'q' is not something which can be assumed to be known in the readership concerned. Thus the more exact information of the original would in fact be less relevant to the magazine reader, since greater effort in processing is one way of decreasing relevance. This greater effort would only be worthwhile if offset by correspondingly greater cognitive effects by

knowing the more precise location of the gene. Since the majority of the readership would not be likely to know what 'q' meant here, there would be no compensating greater cognitive effect.

What we have here is a form of what is called in RT "loose talk" (Sperber & Wilson, 1986/1995; see also Blakemore, 1992; Franken, 1997; Wilson & Sperber, 1988). By this term Sperber and Wilson are describing what in Gricean terms is covered by one of the maxims of quantity, i.e. only give as much information as required for the purposes of the conversation concerned. As they point out (Sperber & Wilson, 1986/1995, p. 233), there are many everyday situations where it is not optimally relevant to give the most exact information as the gain in cognitive effect by the more precise information is outweighed by the increased processing effort required.

In many cases in the popularization of science this is just the situation we meet, as the more precise scientific information in the original RA needs a cognitive context to process efficiently which is not shared by the readership of the popularization. Often the expert's conceptualisation of a scientific process is quite different from the non-expert's (Carey, 1986). This is especially true when reading domain-specific (Spilich et al., 1979) or specialist scientific texts (Tardieu et al., 1992). What this means is that it is more efficient communication to omit that part of the information which cannot be processed by the popular audience. To include the extra information in this case, for example, would at best mean that most of the magazine readership would get the same information as referring simply to somewhere on chromosome 17. They would do so via a process of interpreting 17q21 in the context accessible to them, that is 'chromosome 17' and would end up interpreting 17q21 as meaning `somewhere on chromosome 17', but to do so would require extra processing effort. In the worst case they would be so confused by 17q21 that processing would break down.

RT therefore provides a principled reason for use in the popularization of language which is less 'scientifically accurate' than the language in the original RT. It is not the message which is produced which is important in this case but the way that the message is interpreted. There is no point in using language which is scientifically

more precise but which causes too great a processing load for the reader. At best the reader will only understand a less than fully precise meaning of the too accurate phrase in any case. So it is better to spare him the processing effort and steer him to as precise as necessary understanding by use of an appropriately less than fully accurate description. In fact, a fully precise and literal description should be used only if there an especial reason for it (Wilson & Sperber, 1988).

There is, therefore, some justification in RT terms for departing from the message of the original RA in terms of highly specific language. But if we examine this further, we can see the implications of such a type of communication. If we accept that in specific cases like the example of chromosome 17q21 "loose talk" is acceptable because a more precise description would not be relevant, although this is no longer the message of the original, then we are no longer aiming at transmitting the same message as the original. What instead we are concerned with is communicating something which is relevant to the new audience. What applies in this small example applies to the paper as a whole. The original RA's message will not be relevant to the new audience. We shall examine this claim in the light of both extent conditions of relevance, i.e. effect and effort (Sperber & Wilson, 1995, p. 265).

Relevance -- effect and effort

We shall examine the second extent condition first, that of the amount of effort expended by the news magazine audience to process the communication. We have already seen with the example of chromosome 17q21 above that the amount of effort involved in processing this term would be disproportionate for most of the magazine audience. If this is true of this relatively simple technical term, then it is true *a fortiori* for the relatively more exotic technical terms of the original RA, ranging from simpler terms like "frameshift mutation" via "zinc finger motif" to "Smith-Waterman alignment". More than this, however, the whole paper involves such a detailed description of matters which are far outside the everyday experience of the average magazine reader, such as detailed descriptions of predicted amino acid sequences, that it is extremely difficult for the average reader to process the information. The

effort involved is such that it would be impossible for him to process the information with anything like efficiency in terms of interpreting the text in a way which an expert reader would.

It might be objected that this information is found in the original RA, not in the popularization, but that is precisely the point. The popularization makes no attempt even to give a rough idea of most of the information in the original, since the amount of processing involved would mean that most of the audience would give up and nobody would read the journalist's story. Or, more likely, the story would be spiked by the editor before it even reached publication. If, then, most of the original article involves such a great processing effort on the part of the new audience that the journalist does not even attempt to give the information, it is difficult to see how any sort of code model of communicating the same message, even a distorted form of the same message, can have any sort of validity.

The reason why such effort is avoided, however, lies in another factor. It is not simply that the original is 'too difficult' for the mass audience. Readers only process a text at all if there is an expectation that the positive cognitive effects will make it worth their trouble. Even if the readers were to process a version of the original message and understand it after a huge amount of effort, it would not be worth the effort as the cognitive effects produced would be beside the point. The research article, as we saw in earlier chapters, is concerned with the communication of new research results to a narrow audience of fellow researchers. Processing the RA yields rewards in terms of accessing claims which can be fitted into the pre-existing cognitive framework of the researcher in cancer genetics. Even if the magazine reader were to understand the original RA then the cognitive effects would not be great for anyone outside the original research community or related communities (except of course for exceptional cases like the present writer of these words!).

The point is that effect and effort are interrelated in RT so that one depends on the other. Even if it were possible by a mighty effort to understand the text, the cognitive effect in terms of intersection with an existing context would not make it worthwhile

for the magazine reader. And since relevance is a function of both effect and effort if the effect is very small and the effort is very great, then there is no way that the original RA will be relevant.

What this means for the journalist is that, in addition to making the text accessible in terms of effort for the readership, something which has traditionally been seen as the aim of popularization of science, then she has also to make the text accessible in terms of cognitive effect. Given the interrelationship of effect and effort, even if a text is maximally easy to process, unless it brings about significant cognitive gains for the reader there is no point in processing it. Thus if I write the sentence "London is the capital of the United Kingdom", there is absolutely no problem with your processing it - the effort involved is minimal to a fluent English speaker - but the cognitive gain is zero since the information is already known and that specific piece of information does not bring any particular gain at this stage in helping to process any other piece of information. Thus the journalist has to do more than simply make the original RA maximally easy to process. Even if she did this in an optimal way, the information would still not be relevant to the reader as it is concerned with putting forward claims within the framework of advances in cancer research, something relevant to other cancer researchers but not to a general readership.

This is not to say, of course, that advances in cancer research are irrelevant to a mass audience. However, the information in the original RA, the details of the amino acid sequence, the location of the intron sequence, the lod scores of the patients involved etc are of interest only to researchers. As we saw above in Chapter 3, the journalist has to reframe the research in terms of interest to the new audience. In RT terms, she has to make the cognitive effects of processing even a simple text relevant to the readership. To do so she writes the story within a framework which intersects with the cognitive context of the readership. So Nash, for example, in Text 10, writes the story emphasising the possibility of a test for hereditary breast cancer and the possibility of an ultimate understanding of cancer. Both of these intersect with the worries of her readership about breast cancer.

The code model vs relevance theory

Whereas the code model, therefore, sees the process of communication as involving the direct transmission of particular ideas from source to receiver by means of the code, RT sees communication much more as a result of the interaction between the communicators. Efficient encoding and decoding in the code model leads to successful communication. The particular characteristics of the speaker/hearer or writer/reader do not come into play: the code dominates everything. So if writer A encodes message x, then reader B will decode message x. Then if writer C encodes message x reader D will also decode message x. The message remains the same throughout the communication. From the RT point of view, however, there is no underlying message that gets communicated separate from the process of communication. Thus it is impossible for the writer of the RA to write message x for reader B and the writer of the popularization to write the same message x for reader D.

The core difference between the two models lies in the fact that the code model sees the message as a constant given which can be encoded and decoded exactly. Thus the source and receiver are to some extent irrelevant, in the sense that the message transmitted can remain the same regardless of who the sources and receivers are. By contrast in the RT model what is communicated is considered to be the result of the junction of the proposition communicated and the cognitive context of the receiver. Now clearly a researcher in breast cancer research is going to have a different cognitive context from the average reader of a news magazine, so RT would claim that what is communicated by a sentence like the one analysed above (p. 165) on "Conceptual translation of the cDNA..." (Miki et al., 1994, p. 67), and by other such sentences, would be radically different in the two cases. The researcher's context would mean that this sentence would generate the intended positive cognitive effect, while the totally different magazine reader's context would ensure that such cognitive effects could not be generated. Since positive cognitive effect is defined in terms of the union of new proposition and existing cognitive context, even if the new proposition is the same the resulting cognitive effect will be different since the context

is different. What applies to this sentence chosen from among hundreds of similar ones applies to the texts as a whole.

Looking at popularization of science via the framework of relevance theory would seem to help us to characterize the relationship between popularization and original RA in more precise and helpful ways. The description given in the sections above avoids many of the pitfalls of the code model in general and the diffusion/distortion model in particular. It would appear, therefore, that the code model of communication is misleading as a way of describing what goes on in popularization of science. It leads to ways of describing science communication which not only pay no heed to what actually happens in the popularization of science, but which are impossible even as aims. Given the different audiences involved, it is impossible for both audiences to have the same goals in reading a text and any attempt to see the same message involved is doomed to failure. But does this mean, if the aim of the mass audience in reading a text is so different from the original RA audience, and the ease of processing is so much less, that popularization of science as normally conceived is impossible?

If we regard popularization of science as intended to educate the broad public about the nature of science and the details of scientific advances, for reasons such as science as cultural achievement, the need for democratic decision making and the fact that scientific research is publicly supported, to cite three reasons commonly put forward by proponents of the public understanding of science (Durant et al., 1990), then the implications of the view put forward above are not too hopeful. The attentive public, to use Miller's term (Miller, 1986), might be reached, but not anyone outside that 20% of the population. Popularization of science in formats like *New Scientist* or *Scientific American*, as examined by Myers (1990), would be explicable in these terms, but not the kind of popularization we have been examining here. But if we look at the second of Durant et al.'s reasons for communicating popular science to the general public – "science affects everyone's lives and people need to know about it" (Durant et al., 1989, p. 11), then we have a much more hopeful scenario.

Although the general readership of news magazines would not make any great cognitive gains, as we have seen, both because of the effect and effort dimensions, from the message of the original RA's, this does not mean that the magazine articles do not have such effects. If we look at the popularization of science in terms of the second of Durant et al.'s aims stated above, i.e. communication about science that affects people's lives, then such articles can be effective. But it has to be accepted that to achieve their proper effect the desire to communicate the same message as the original RA must be abandoned. These articles have the impact they do because they do not attempt the impossible and try to communicate a message which is irrelevant to the different audience. Instead, as we saw in Chapters 2 and 3, they concentrate on those aspects of popular science which are relevant, in both the general and RT senses, to the audience.

Looked at in RT terms, therefore, the narrative of society is a natural response by the journalist writing popular science for the news magazine of the need to make the science have positive cognitive effects given the different context. This rewriting of the science to make it more relevant to the new audience should not be seen as some sort of distortion of the original science. It is a communication of the science so that it makes sense to the different audience. This perspective on the popularization of science therefore would make the claim that popularization of science is indeed possible and that such popularization will be successful to the extent that the popularization renders science relevant to the new mass audience. However, if the science of the popularization is too high level and elitist, so that the effort involved in accessing it is too great, then the popularization will not be successful. Conversely, if the science described is too remote from the lives of the average reader, then the cognitive effects involved will be too small and the science will equally not be successful.

What this does not imply, though, about the popularization of science for a mass audience, is that it is enough just to simplify the science involved, by cutting out the jargon and the mathematics. This will have an impact on the effort side, so that processing is easier, but unless there are corresponding positive effects on the effect

side communication will not be achieved. This suggests that the traditional approach to the public understanding of science, when what is seen as being necessary is better science education, more scientists communicating with the public etc, is missing the point. Excellent popular science of the type written by popularizers like Stephen Jay Gould (1991, 1993) or Steve Jones (1994, 1996) is not the same kind of thing as the science of the news magazines. Such writers and others like them are professional scientists who are writing about areas of science they are familiar with for the wider public. The minutiae of paleontology written about by Gould, though in a fascinating way, do not intersect with the concerns of the general public. Their readers and the people who kept *A brief history of time* (Hawking, 1988) at the top of the best sellers lists for weeks on end are not the average readers of news magazines. Readers of such authors are interested in science as such, devotees of the narrative of nature in Myers' terms (Myers, 1990). Readers of the news magazines are not necessarily interested in science *per se*, although some of them may be.

For the mass audience popularization of science then means making science relevant by finding those aspects of science which interact with their interests and expressing those aspects in a form which makes the connection between the science and their interests clear. The converse of this is that we cannot expect to write popular science in terms which are at all similar to the science written by professional researchers. Most of this, by putting forward claims about a small segment of a specialist area, has no impact outside the specialist research community. But even science which has potentially a wider impact, like the cancer genetics we have examined, cannot be reported in the same terms. It is therefore necessary to recognise what happens in practice and make our model of the popularization of science reflect what in fact takes place rather than imply, by talking about diffusion or distortion, that popularization of science is simply a reworking, either successful or unsuccessful, of the same message.

The popularization of science, then, must take into account the audience appealed to and make the science relevant to that readership. In the next chapter we shall investigate how, in RT terms, it manages to relate to the wider society by making use of the narrative of society concept described above in Chapter 2.

CHAPTER 6

RELEVANCE THEORY AND THE NARRATIVE OF SOCIETY

Having seen in the last chapter how relevance theory can help us gain insight into the general nature of the popularization of science, we shall examine in this chapter how it can help us frame more precisely the concepts of social and scientific moves integral to the narrative of society model. RT, as we have seen, helps us to explain why we cannot communicate the same message in popular science as the research article does, and therefore implies that we were on the right lines when we talked about relating to the interests of the audience in this model. However, we have not shown, up till now, just how RT demonstrates that the precise model that was put forward in earlier chapters could be justified. In this chapter, therefore, we shall attempt to show that RT provides independent justification for the division between scientific and social moves integral to the narrative of society model.

Interpretive resemblance

If the journalist is trying to bring about new cognitive effects in her audience rather than merely report the research science this has implications for how she writes about the science as detailed in the RA. The reporter does not try to give the same message as the original RA, as we saw above. What she does can be explained in RT by way of the distinction between descriptive and interpretive use of language (Sperber & Wilson, 1986/1995; see also Blakemore 1992, 1993). The first of these, descriptive use, occurs when a proposition is used to represent an actual state of affairs. The second, interpretive use, is when one representation with a propositional form is used to represent another representation with a propositional form. The former occurs when we make a factual statement about the world, while the latter takes place whenever we report another proposition. To make a report of another proposition it is clear that our report of the first proposition must in some way resemble that first statement. The limiting case at one extreme is identity of form, i.e. direct quotation.

But usually interpretive resemblance is a matter of degree and two utterances will interpretively resemble one another to the extent that they are similar.

In what respect similar? One interpretation of interpretive resemblance is put forward by Gutt (1991), who sees two utterances as interpretively resembling one another to the extent that they share explicatures or implicatures. On the other hand, Blakemore (1993), sees the two as sharing logical and contextual implications. If we compare the two formulations that by Blakemore seems preferable, since it is possible for two utterances to be quite similar in their explicatures without sharing implicatures. The reason for this is that a different context will produce different effects relative to these explicatures. Sharing logical and contextual implications makes it possible to share entailments and implicatures without the claim that the explicatures are exactly the same. To see how two utterances can share logical and contextual implications let us look at an example of how it would be possible to report the following utterance.

John: I'll be coming to the party on Saturday.

This could be reported to the host at the party as any of (1) to (3), among many other possibilities.

(1) John said he'd come.

(2) He's coming to the party.

(3) He'll be here.

All of these share implicatures in the context with the original statement which they resemble. In all cases the host would be entitled to draw the conclusion that John would be at the party.

If we consider the communication of popular science from this perspective, then it is obvious that popular science is concerned with interpretive use of language rather than descriptive use. But in this it does not differ in any respect from any reporting, whether of the type given as examples above, or in the writing of any kind of journalism where the opinions of others are reported. Nevertheless it does help to

illuminate how the narrative of society perspective differs from the diffusion/ distortion coding model. Expressed in RT terms, the diffusion/distortion model sees reporting of scientific research as very much concerned with a close interpretive resemblance of the popularization to the original research. Where that resemblance is not close the popularization is accused of distortion. A good piece of popular science reporting is considered to be one where the resemblance is high. Thus if the popularization, for example, omits a description of the research methods then a traditional content analysis approach, e.g. Evans et al. (1990), considers this an omission and the resemblance would be lowered.

In this perspective the closer the popularization to the original then the higher the interpretive resemblance. However, from our perspective, it is possible for a report to differ quite substantially in form from the original and for it still to resemble the original as it generates the same implicatures. For example if we compare the original Miki et al. (1994) description to the interpretation in Text 10, *Cornering a killer*, we find very similar implicatures. The original RA has "Predisposition to cancer is inherited as a dominant genetic trait, whereas the predisposing allele generally behaves as a recessive allele in somatic cells. Thus, a single inherited copy of the mutant allele causes predisposition and, and loss or inactivation of the wild-type allele completes one of the steps in the progression towards malignancy." (Miki et al., 1994, p. 66.) The popularization, on the other hand, has "For when one copy of the gene is inactivated by a mutation and the backup copy is damaged or lost, a breast cell takes a giant step towards malignancy."

The popular version here obviously simplifies the original, which has more, and more detailed, information, including a lot more technical terms, such as "wild-type allele". Nevertheless, as well as generating more precise implicatures because of these additional details, the original RA would still generate the main implicatures as expressed in the popularization.

An example of a different sort will illustrate another type of interpretive resemblance. If we take the sentence in Text 10, "The one section that does remind researchers of

other snippets of DNA they have studied contains a hint that *BRCA1* could spell out the recipe for making a particularly potent protein - one that may serve as a master switch in the nucleus of breast cells, turning other genes on or off", then we will find no one sentence in the original Miki et al. article which this sentence is an interpretation of. Rather this sentence is a summary of various parts of the original RA, a summary being another type of interpretation (Blakemore, 1993). Thus the section that reminds researchers of other DNA is a reference to "a sequence with considerable similarities to zinc finger domains" (Miki et al., 1994, p. 67). This is described in another sentence and there is also a diagram (Fig. 2(B)) which shows the similarities. The rest of the sentence and its description of a master switch is discussed in a different part of the paper where *BRCA1* is characterized as a tumour suppressor gene.

The sentence quoted in the popularization, therefore, is a summary of the more detailed information described in the original RA. Now it is obvious that the more detailed information in the original will bring about more detailed specific implicatures, e.g. about the sequence described. However if we bring together all the information in the original that appears in summary form in the popularization these propositions will also have general implicatures apart from the specific implicatures of each part. To understand this we must remember that the implicature taken to be communicated is the first implicature tested and found compatible with the principle of relevance, i.e. providing large cognitive effects for little effort. The general implicature from all the information on the zinc finger domains, together with the lack of other analogous sequences, would indeed be that there is only one section which reminds researchers of other snippets of DNA. The specific information on the zinc finger domains would allow other researchers to access additional propositions, for example about the extent to which they were similar. However, the information that the gene is not very similar to other genes previously described is common to both the original and the summary in the popularization. Since two utterances are taken to interpretively resemble each other to the extent that they share logical and contextual implications, the two can be said to resemble each other in this way. The original has

more detailed and additional explicatures and implicatures but it entails or implicates those explicatures and implicatures in the popularization.

This allows us to put forward a description of what constitutes the relationship of interpretive resemblance of original RA and popularization. A popularization of a scientific research article may be said to interpretively resemble its source RA to the extent that the explicatures and implicatures in the popularization are a subset of those logically and contextually implicated by the original RA. Thus the original RA will include a lot more information and therefore a far greater number of explicatures and implicatures than the popularization, but those in the popularization should be entailed or implicated by the original RA. In this way we can allow for the fact that the popularization is bound to be a simplification of the original and contain less detailed scientific information than the original, but guard against the distortions that the iffusion/distortion model is so concerned about.

This is not to accept the diffusion/distortion model's emphasis on errors. However in stressing that the RA and popularization are different stories, we should not go so far as to allow the popularization to vary infinitely from the original. There must still remain some connection to the original RA for the news magazine story to be a popularization of the science in the RA. The notion of interpretive resemblance allows us to keep that connection while at the same time emphasising that the news magazine is not telling the same story as the source RA.

However, there is a further problem in saying that the popularization contains a subset of the explicatures and implicatures which are logically and contextually implicated by the RA. Clearly, there are some propositions in the popularization which are not implicated at all by the original RA. To take Text 10 again as an example, the reference to Judi Ellis reacting to the news of the discovery on the car radio obviously does not appear at all in the source Miki et al. article. If we examine what kind of information this is, however, it turns out to be a classic example of Move 1a), Personal example, in other words a social rather than a scientific move, in terms of the classification introduced in Chapter 2. Since the function of social moves is to relate

the science to the interests of the readership, it is not to be expected that they appear in the original RA. The relationship between popularization and original RA therefore cannot be seen purely as the former carrying a subset of the logical and contextual implications of the latter. This set relationship is found only in the scientific not the social moves.

Let us examine the other moves to see whether we have determined a real relationship between original RA and popularization. If we look at the scientific moves we can see that they all involve either a reference to one original RA or other RA's which can also be seen as sources. Thus Move 4, Description of background research, may involve reference to the original RA or perhaps further scientific research articles. In the case of the popularization we have been examining here, Text 10, the background research refers to the work of King and her team, which is both referred to in the main source article and may be a source in its own right. Move 5, Further description of findings, obviously comes from the same Miki et al. original RA, while Move 7, Description of further findings, briefly alludes to other research on *BRCA2*. Significance of findings for future research, Move 9, is found usually in the discussion section of the original RA. It seems, then, that the scientific moves can clearly be traced back to the original RA('s). Therefore the subset relationship we saw between scientific moves of the popularization and the original RA's would seem to be valid.

We can now turn to the social moves to see if the converse is also true. We have already seen that the first move, Relating to the reader, is not found in the original RA. If we examine the others we shall see that they too cannot be found in the original RA's. Thus Move 3, Description of background ideas, although it may seem scientific on the surface, is aimed, as we saw above in Chapter 2, at helping the reader to understand the science rather than describing the new findings as such. Thus its source is not any particular research article but background knowledge which is taken for granted by the research scientist. Move 8, Explanation of findings, similarly might be thought to be scientific, but as its function is to explain the new findings to the reader again it includes information which is too basic to be included in the original RA. The final move, Move 10, Significance of findings for the reader, in its

directly relating the findings to the lives of the readership again is not found in the original. There may be some information in the original RA on which this is based, but this is not the subset relationship that we are investigating here. In fact the Move 10 contains more information than the information in the original RA, so that, if anything the propositions in the original RA are a subset of those in the popularization, rather than vice versa. Thus Text 10 contains a lot more about prospective tests for the presence of *BRCA1* than the Miki et al. original.

There is one social move which has not yet been mentioned, Move 6, Evaluation of findings. This was described above in Chapter 2 as "borderline social" in that it intended to evaluate the findings in some way and this could be done at times by other scientists. Even when this evaluation is done by other scientists being quoted by the journalist rather than the journalist herself, however, there is no actual source in an original RA, so the generalisation we are investigating here still holds.

It appears to be the case, therefore, on first analysis that we have uncovered a clear relationship between the original RA and the popularization in terms of interpretive use and social and scientific moves. However, the examples given up to now have been no more than illustrations. We need to examine a complete popularization right through to see if the relationship holds in detail. Below, therefore, we shall test the hypothesis that scientific moves in the popularization are a subset of the logical and contextual implications of the original RA by examining one popularization we are already familiar with, Text 7, *Zeroing in on breast cancer*.

A comparison of original RA and popularization in terms of interpretive resemblance

Text 7 starts off with a portrait of a family with hereditary breast cancer, the Cunninghams. As we saw above in Chapter 2, this is a typical example of a Move 1a) and, therefore, being a social move, is not found at all in the original RA. The second paragraph, though, introduces the scientific move, Description of background research, Move 4, and we can expect to find an interpretive resemblance to the source

research article. Thus, the original RA has "Intense efforts to isolate the BRCA1 gene have proceeded since it was first mapped to chromosome arm 17q in 1990 (4, 5)" (Miki et al., 1994, p. 66), with references (4) and (5) being the paper by King's team, and a report on the discovery in the *Lancet*, respectively. In contrast, the statement in the popularization is as follows: "Ever since Berkeley, Calif., geneticist Mary-Claire King showed in 1990 that familial breast cancer is often tied to a particular gene, researchers all over the world have been racing to identify it – and, in turn, important clues to a still mysterious disease."

Clearly, the original leaves a lot more unsaid than the popularization. If, though, we remember that implicatures are generated by the interaction of new information in the text with the context in the reader's head, then we can see why the different information is given in the popularization. The research community was well aware of King's work and does not need to be told of it in any detail – the reference is left to a note. The magazine audience on the other hand will not be aware of this and needs to be overtly told about it. The implicatures of both statements, however, would be the same: that since King's work in 1990 there has been a lot of work to find the gene in question. So here we can see that superficially different statements in the original and the popularization can in fact generate the same implicature because of the different contexts involved.

The next sentence, again, is a social move. It is the short sentence "Now this race has ended in triumph." This does not appear in the original RA, but it is in fact a Move 6, which is a social move. In this case it is an evaluation by the journalists themselves of the success of the discovery and could not be expected to be found in the original RA. It may have a source in other journalistic reaction to the news, e.g. Nowak, 1994a, but it could not be said to be found in the original RA.

The following move, however, is a Move 2, Description of main research, and must be related to the main original RA. However, although it is clear that the journalists are reporting the main research findings here, it is interesting to see that the actual source may not have been the original RA. The popularization says that "Last week a

consortium announced...". This leaves open the possibility that what is being reported here is in fact the press conference announcing the findings rather than the RA itself. Although this is probably what in fact is happening, the proposition expressed is, of course, a contextual implication of the original RA. The original talks clearly of identification of *BRCA1*. As regards isolation, this is a contextual implication of the sentence in the RA where the gene is described and is also implicated in the parts giving the description of the sequences.

One further point is worth discussing with reference to this sentence. The popularization mentions simply that the team had discovered and isolated "the gene, dubbed *BRCA1*". Identification of the referent here of "the gene" traces it back to the previous reference to a particular gene often tied to breast cancer. The RA for its part talks about the "**Identification of a strong BRCA1 candidate gene**" (Miki et al., 1994, p. 66, bold in original). What we have here is an example of loose talk. The dropping of the qualification of the gene as merely a strong candidate is justified in terms of the whole paper, where the strength of the evidence is such that it is clear that what has been identified is very likely to be *BRCA1*. Strictly speaking, the identification of a strong candidate for the gene by itself does not implicate the discovery of the gene, but the paper as a whole does implicate such a discovery. What we have here is an example of the different contextual implications produced in different audiences. Description of the gene as merely a "strong candidate BRCA1 gene" is appropriate in terms of the normal hedging of new claims in science (Hyland, 1996a, 1996b; Myers, 1990). It is noteworthy that although this hedging is found in the abstract at the top and in the heading quoted in bold earlier in this paragraph, it is soon dropped in the body of the article. The popularization is quite justified, therefore, in omitting this qualification, since it is put in the original in order to conform to scientific conventions and allows the authors to withdraw their claims later without too much loss of face, if necessary (Myers, 1990). To put such a hedge in the popularization, however, would suggest to the different audience that the discovery was much less confirmed than it in fact was. This is a good instance, therefore, of how the same proposition in RA and popularization may have different contextual

implications and therefore it is justified in making a change in the popularization so as not to mislead the different audience.

This sentence is interesting in terms of the different views of interpretive resemblance held by Gutt and Blakemore, quoted above. We mentioned that Gutt (1991) saw two utterances as interpretively resembling each other to the extent they share explicatures and/or implicatures. In this case, there is no actual explicature in the original RA at all similar to the sentence about the discovery in the popularization. It is an interpretation in the form of a summary by the journalists of the main findings. Being a report, it is a clear instance of interpretive resemblance similar to the examples in Blakemore (1992). But there is no similarity of explicature. And indeed it would be odd if we found any explicature in the original which was similar to the popularization. Blakemore's formulation in terms of logical and contextual implications, however, allows us to capture the kinds of relationship involved.

The next brief reference to King's work again is an obvious implicature of the reference in the original RA to the gene being "first mapped to chromosome arm 17q in 1990". This is followed by a brief summary of a lot of the work on identifying *BRCA1*. The expression "zeroed in on" may not be standard scientific terminology, but it is implicated by the description of the methods of finding the gene in the original. The next sentence, describing what the gene does, has been discussed above in Chapter 3. Apart from the mistake in calling the gene a "tumor suppressant" rather than a suppressor, this is again an example of loose talk in saying that the gene rather than the protein is the tumour suppressor. But such loose talk is also found in the original since it talks about "the hypothesis that *BRCA1* is a tumor suppressor gene" (Miki et al., 1994, p. 66). This is followed by the statement that the mutant gene heightens susceptibility to both breast and ovarian cancer. This is clearly implicated both by the abstract and by the more detailed tables of the kindreds involved. The final sentence of paragraph 2 is another example of Move 6, so is not found in the RA.

The first sentence of the next paragraph, since it talks about the effects of the discovery on the readership, indicates that this is a Move 10, Significance of findings for reader. The rest of the paragraph then expands on this and gives evidence for the generalisation. This paragraph is an interesting test case of the distinction between social and scientific moves. From the first sentence the paragraph would seem be clearly a Move 10, a social move, as we have said. However, the information in the paragraph would, on the face of it, seem to be scientific in nature, which would tend to indicate a scientific rather than a social move. Looked at more closely, however, the data given does not in fact come from the original RA. While the RA has a figure of 5% for hereditary breast cancer as a total of all breast cancers, the popularization has a figure of 5 to 10%. In addition, the source is given as "experts" rather than the Skolnick team. Thus while 5-10% as the proportion of breast cancer patients due to hereditary causes is certainly not an implicature of 5% being due to hereditary causes, we are saved from having to claim this by the fact that the source here is not the RA and that this is not a scientific move. Similarly, the figure of 180,000 cases is not derivable at all from the original RA.

There is a problem, though, with the next sentence, to the effect that, of all hereditary cases, "about half can be linked with *BRCA1*". This is a clear implicature of the statement in the original RA that mutation in *BRCA1* is believed to be responsible for about 45% of cases. This would seem to go against our postulate that no proposition in the social moves of the popularization is an implicature of the original RA. If we look more closely at the statement in Miki et al., however, it can be seen that this figure is not original to this RA, as another source is given. Similarly, the figure of about half is also given by a news article in *Science* discussing the forthcoming paper on the location of *BRCA2* (O'Brien, 1994). This latter article also mentions that the estimate goes back four years to the period following the discovery by King and her group. This means that the proposition that about half of all cases of hereditary cancer are due to mutations in *BRCA1* does not come from the original RA, but is a general background assumption of the field. The same goes for the other data on the frequency of breast cancer at different ages, which also comes from general medical statistics rather than the original RA.

The following paragraph is the same sort of move and again we have information which appears at first sight to come from the original RA. But in fact the prediction that tests for the gene are some way away does not come from the RA, as the only reference to tests is that further tests "may permit accurate genetic screening", with no time frame mentioned (Miki et al., 1994, p.71). The source for the figure of some years off may be Shattuck-Eidens, who is quoted in Nowak, (1994a), as saying that diagnostic tests could be available in 6 months to two years. Since the rest of the paragraph then goes on to discuss in more detail the problems involved with diagnostic tests, this section also could not come from the RA. The same topic is continued in the next paragraph. The only new point here is that there is a direct quote from one member of the Skolnick team, David Goldgar, an epidemiologist, on the problems of providing genetic information to those at risk of hereditary cancer, but again, despite being a member of the consortium, this information does not come from the RA.

It seems, therefore, that we can conclude that although scientific information can appear in a social move, such information does not come from the original RA but rather from more general background sources. Our hypothesis, then, that social moves do not have a relationship of interpretive resemblance to the original RA, remains. It may be the case, though, as here, that social moves have such a relationship by chance, as here. This will occur when both the social move and the RA have an interpretive resemblance to a common source. Therefore, the claim needs to be slightly revised to take such a situation into account. We shall say, then, that social moves do not have a relationship of interpretive resemblance to the original RA, except where such a relationship is shared by both RA and popularization with regard to a common source. The interpretive resemblance, in such a case, is a byproduct of the common interpretive resemblance of the two to the common source.

If we consider this a little further, it is evident that such a specification needs to be put in place. If we ruled out any interpretive resemblance at all between social moves of the popularization and the RA, this would mean that any general information in the social moves and in the RA would be impossible. Thus a statement in a social move

to the effect that cancer is a serious threat to the reader's health could be seen to be implicated by a statement in the RA that x Americans die of y cancer each year. More particularly, it is probably better not to specify that the interpretive resemblance be due to a common source. In many cases it would be very difficult to prove that the resemblance was to a particular source when general scientific information was being discussed. A way of framing the claim therefore that would avoid such problems would be to say that there is no relationship of interpretive resemblance between RA and social moves of the popularization unless the explicatures or implicatures in the popularization can be seen also to be logical or contextual implications of another source. This means that such a relationship is ruled out where the sole source is the RA.

The last paragraph contains a different scientific move, Move 7, Description of further findings. The further findings referred to are the location of *BRCA2*. This, like the earlier announcement of *BRCA1*, could refer to a press conference, but, nevertheless, is inferrable from the Miki et al. paper, which states that "A second locus, *BRCA2*, recently mapped to chromosome arm 13q (6), appears to account for a proportion of early-onset breast cancer roughly equal to that resulting from *BRCA1*" (Miki et al., 1994, p. 66). The second part of the sentence reporting the discovery of the location of another gene is not so easily traced to any part of the original RA. However, the statement that "though it could take years to isolate the second gene, *BRCA2*, the new race is on" is an implicature that can easily drawn from two propositions in the original RA. These propositions would be, first, that there was a race to find *BRCA1* after it was first localised, and second, that *BRCA2* had been newly localised. The union of these two propositions easily yields the contextual implicature that a race to discover *BRCA2* will also take place. This constitutes a Move 10, Significance for future research, so it is a scientific move where we would expect such an implicature to be traceable back to the original RA. Finally, the popularization ends with a Move 9, where we have a social move relating back to the Move 1 at the beginning of the article.

Examination of Text 7, therefore, appears to bear out our hypothesis on the relationship of interpretive resemblance between RA and popularization. However, a check of the other texts does reveal occasional problems, which can, though, be explained by circumstances peculiar to the texts concerned. If we take the sentence in Text 10, "But a foolproof test will take time – perhaps years – to develop, because *BRCA1* turns out to be an unusually long complex gene that can be crippled by many different mutations.", then we will find no one sentence in the original Miki et al. article which this sentence is an interpretation of. Rather this sentence is a summary of various parts of the original RA, as well as general background knowledge, a summary being another type of interpretation (Blakemore, 1993). There is no real equivalent of the first part of the sentence quoted, other than a mention of future genetic screening at the end of the Miki et al. article, but such knowledge that tests were some time in the future would be already part of the context of researchers in the field. But since this sentence is part of a Move 10, a social move, we would not expect that it could be traced back to the RA, so there is no problem with this part.

The statement that *BRCA1* is long and complex and crippled by many different mutations, however, brings together information in the original RA on its length and complexity. Thus its length is given as "distributed over roughly 100 kb of genomic DNA" (Miki et al., 1994, p. 67). The complexity is revealed in statements about 22 coding exons and "a complex pattern of alternative splicing" (Miki et al., 1994, p. 67). The many different mutations summarises the list of 5 mutations of different types given, with more expected to be found. (In fact well over 200 different sequence variations were later found, (Kahn, 1996)).

The problem with this is that the last part of this sentence, therefore, is squarely based on the original RA. But because this is part of a social move, it should not be related to the RA in this way. It is also the type of information, relating to the characteristics of the gene isolated, which could not have come from general background knowledge of the research field other than the original RA source. However, if we look at the placing of this information we can see that it is a subordinate clause right at the end of the paragraph. Its discourse function is to link to the next paragraph which is a

scientific move, Move 5, Further description of findings. Thus, although being part of a paragraph which is a social move it acts, in fact, to link forward to a scientific move, which explains its relationship to the RA.

Another, more serious, challenge to the hypothesis of social moves not being linked to the RA is found in Text 17, *It's not as simple as taking a blood test*. Paragraphs 8 and 9 of this text were coded as Move 10, Advice to reader. However, the information here which is given as advice is taken directly from the RA. This, though, can be explained by the fact that Text 17 is rather anomalous in our corpus, in that it is the report of two panels giving direct medical guidance on screening for cancer. Therefore, it is not directly reporting original biomedical research as such. This explains why it is possible for the popularization to give advice which is taken directly from the original RA. Unlike the other RA's this particular one does give direct medical recommendations. The anomaly with regard to social moves, therefore, can be explained by the difference in the type of text.

There is one case, though, where there is definitely reference to the original RA in a social move. This is in a move, though, which is defined above, in Chapter 2, as "borderline social", Move 6. More accurately, Move 6, Evaluation of findings, may be said to be giving a social reaction to the science of the RA. In such a situation, it must sometimes refer to the RA. A good example of this is Text 11, *Do abortions raise the risk of breast cancer?*. We saw above that this text included a critique by the journalist of the scientist's research, the journalist accusing her of hiding findings which conflicted with her pro-choice standpoint on abortion (paragraph 4). In doing so the journalist necessarily makes specific reference to the original paper. However, the journalist here is not criticising the science as such, she is rather criticising the researcher's reporting of the science. Thus the journalist is not actually reporting the science, she is reporting the reporting of the science. Hence, despite appearances, this Move 6 does not in fact contradict our hypothesis that the social moves are not implicated by the source RA, as the source does not implicate this critique that they were hiding information. One sentence is indeed implicated, the sentence beginning

"She and her colleagues reported...", but the move as a whole is not implicated as it is a critique of the reporting rather than a description of the science.

In other texts, such as Text 2, *How safe is lumpectomy?*, the evaluation is taken from interviews with other specialists, this being the situation also with Text 5, *To test or not to test*. The case is similar with Text 1, *Beyond vitamins*, where a comment evaluating the research by a scientist is a prelude to describing the significance to the reader. In Text 8, *Breast cancer: A diagnosis of deceit*, one source of evaluation is another scientist, while another evaluation is a general statement by the journalist not specifically based on any one source. Sometimes the evaluations are simply ascribed to "other scientists" or "critics", as in Text 13, *Breast cancer's deadly masquerade*. In Text 19, discussing the effects of tobacco smoking, we have evaluations of the research from both sides of the issue, the tobacco industry and anti-smoking advocates.

Another social move which is related to the science of the RA might also be expected perhaps to dent the hypothesis, i.e. Move 8, Explanation of findings, where the journalist explains the findings of the RA. However, we find, for example in Move 8 in Text 11, that the explanation of the findings bears little resemblance to the original RA. The reason for this is that the RA can rely on the knowledge of the readership, whereas the popularization must spell out in detail what is not mentioned at all in the RA. In Text 11, too, we have the added point that, in addition to the RA, an interview with the researcher was another source of information for the journalist in question. The situation is similar for Text 18, *Linking abortion and breast cancer*, on the same topic. In other texts, such as Text 2, *How safe is lumpectomy?*, the explanation is taken from interviews with other specialists, as is the case with Text 8. In Text 6, *Warding off breast cancer*, on the other hand, we have a specific reference to past studies. A general explanation in terms of standard textbook genetics is given in Text 12, *Stopping cancer in its tracks*, where the explanation could also be seen as Desccription of background ideas, so standard is it.

Therefore, there may be some minor exceptions to our hypothesis on the relationship between original RA and popularization in terms of interpretive resemblance, but these can be ascribed to special factors. Borderline text, which is not fully part of that section of text, but links to neighbouring sections, may have characteristics of these neighbouring sections, as here. The function of these sections of text is to link one move to the next. Or texts which are not mainstream biomedical research reports may also differ. One part of a move may be implicated, as in Text 11. Generally, though, the hypothesis seems to hold. This is interesting, as the social moves were set up originally without reference to the concept of interpretive resemblance. The fact, then, that our division into social moves and scientific moves is supported in RT terms is further additional independent confirmation of this analysis.

It seems, then, that investigation of the idea of interpretive use has led us to an important insight, namely that the relationship between popularization and original RA can be characterized in such terms. More precisely, it has allowed us to differentiate more exactly between the two types of move set up independently in Chapter 2, and has enabled us to give a more precise description of the difference between social and scientific moves set out there. In addition we have been able to describe the relationship between popularization and original RA in more precise terms, as follows:

1. The explicatures and implicatures contained in the scientific moves of the popularization are a subset of those logically or contextually implicated by the original RA(s).

2. The explicatures and implicatures of the social moves of the popularization are not a subset of those logically or contextually implicated by the original RA(s), except where these are also logically or contextually implicated also by another text.

Examination of the relationship of interpretive resemblance between original RA and popularization, therefore, has allowed us to characterize more exactly the relationship between the two. We have been able to specify a precise connection between them in

terms of the categories of scientific and social moves put forward earlier. As these move categories were established prior to, and independently of, any examination of the corpus in terms of relevance theory and within the framework of a different discourse/pragmatic model, that of genre analysis, the fact that this aspect of the model is borne out by a totally different theory acts as powerful support for the model of scientific popularization put forward here.

In addition, as we have seen above, relevance theory also supports the more general claims of our model of the popularization of science that the popular science text is not just a simple version of the original research article. Instead it bears out the proposition put forward here that popular science involves articulating aspects of the interests and concerns of the mass audience rather than explaining research science.

Relevance theory, therefore, has allowed us to support the model put forward earlier and to highlight it in more subtle ways as a model of communication. However, relevance theory has up till now not been used to explain such a text model. The arguments put forward above suggest that it can be very successful in this regard. Nevertheless, because of the different way I have used the theory here, there are some aspects of RT which need to be further examined to demonstrate that they are compatible with the model of popularization proposed here.

CHAPTER 7

RELEVANCE THEORY AND TEXT

In the last two chapters it was shown how relevance theory could be used to support the model of popularization put forward here. As a general model of how communication is achieved it lent itself very well to the view of popularization described, in contrast to the code model inherent in the diffusion/distortion model. In addition, a specific aspect of the theory, the idea of interpretive resemblance, was put forward as a way of making more precise the distinction between social and scientific moves argued for in earlier chapters.

Nonetheless, despite this close agreement between RT and the model of popularization of science proposed, there are aspects of RT that need to be investigated further if we are to make a strong argument for an RT-based, or even RT-compatible, model of communication. RT makes very precise and particular claims about how communication is achieved and is a very general model indeed . It claims that its model of ostensive-inferential communication applies to all human communication without exception, although linguistic communication also involves coded communication. Not only this, but it also applies to all types of human communication, cutting across distinctions like public or private, large or small scale, written or spoken, literary or non-literary (Sperber & Wilson, 1986/1995).

For this reason, some critics, such as Levinson (1989), have concluded that RT is overambitious in its claims. Others, like Bach and Harnish (1987), have objected to the lumping together of linguistic and non-linguistic communication, while still others claim that "(c)ommunication cannot be restricted to what people intend to communicate" (Mey & Talbot, 1988). Other more specific criticisms of particular claims in RT abound (see for example the Open Peer Commentary on Relevance, 1987), but it is sufficient for present purposes to note that there has been a common criticism of Sperber and Wilson for their definition of what constitutes communication and then for saying that their model explains it. .

Part of the problem may lie in the fact that, as Sperber and Wilson (1997) admit, much of the work on RT has been carried out on the basis of isolated invented examples rather than authentic data of any length. Sperber and Wilson nevertheless argue that this does not invalidate their claims (Sperber & Wilson, 1997) and within the broader generative framework with which their model is compatible, although it is, of course, not part of the same theory, such use of intuitive examples is quite acceptable. We cannot, therefore, attack Sperber and Wilson for doing what most contemporary linguists in fact do. However, it is one thing for a syntactician discussing a sentence to produce examples from her own linguistic competence; it is another thing entirely for a theory of communication to be based on such isolated examples. Indeed, Sperber and Wilson admit that use of such invented examples tends to mean that they are interpreted with more emphasis on the effort side of the effect/effort divide because the context of use is artificial (Sperber & Wilson, 1986/95).

This is not to say that RT has not been applied to larger stretches of non-invented data. Thus both Blass (1990, 1993) and Gutt (1991) have used RT to investigate extended stretches of text. Nevertheless, it remains true to say that the bulk of work in RT has been on the basis of such isolated invented examples as are typically found in most of the major works in RT (e.g. Sperber & Wilson, 1986/95; see also Blakemore 1987; Carston, 1988). Similarly, although billed as a theory of communication rather than a theory of conversation a la Grice (although the latter's theory is applied more widely, of course, as well), much of the argument has been on the basis of small scale (often invented) conversation (Sperber & Wilson, 1986/1995; see also Blakemore 1987, 1992; Jucker, 1993a, 1993b). A corollary of this is that a lot of the data analysed has been spoken rather than written text. Even the major corpus study to date, that of Blass (1990), is on the basis of spoken data.

It would seem important, therefore, also to test RT against a corpus of written authentic data, of a specific text type, to see whether it can also apply, as claimed, to this type of data. For our argument here, since RT has been used to back up our model of a type of written communication, it is important that RT does in fact function effectively as a model of written mass communication as well as spoken

private communication, as claimed. Hence, we shall examine whether RT can explain also this type of communication, as evidenced by the data in our corpus.

Written vs spoken communication

Outside the RT community it is widely felt that there is in fact quite a significant difference between spoken and written texts. Thus Akinasso, (1982), in surveying the differences found by researchers up till then, listed 10 main differences in syntactic and semantic structure, so that written texts have a more complex syntax with a greater use of subordination rather than coordination, greater use of text organisation devices, elimination of false starts etc. In addition, it is clear that a major difference between written and spoken text is that the former is characteristically more permanent and less dependent on the physical presence of the sender and receiver of the message. Similarly, Biber summarises the differences between the two according to previous researchers as being that writing is more structurally complex, more explicit, more decontextualized, less personally involved and more detailed and abstract, contains more new information and is more deliberately organised and planned (Biber, 1988). There is a problem here in that when comparing written and spoken texts we may not be comparing like with like, as texts may vary along other dimensions, such as formality, thus complicating the written/spoken comparison (Akinasso, 1982; Beaman, 1984). Nevertheless, even when a similar text type is compared, as in Tannen's work in narratives, differences are found between written and spoken forms (Tannen, 1984), such as the degree of explicitness (cf. Mazzie, 1987). Other researchers, e.g. Gumperz et al (1984), have emphasised the inherent differences between typical written and oral modes, such that writers typically do not have immediate feedback on the success of their communication from an interlocutor. Similarly, Scinto (1986) argued that the oral form is unmarked for dialogue, while the written form is unmarked for monologue.

Work more specifically on writing, such as the classic research of Hayes and Flower (1986), has also suggested that writing is planned, goal directed, hierarchically

organised and subject to revision. This is supported, for example, in the recent model of writing as communicative language use put forward by Grabe and Kaplan (1996). Other descriptions of academic types of writing, such as those by Crismore and Farnsworth (1990), Halliday and Martin (1993), or Salager-Meyer (1994) among many others, have described particular features of different types of academic writing. It seems to be fairly well accepted, then, that writing is distinct from speaking in many ways other than the obvious differences of physical channel. Although written texts may differ from spoken in terms of such factors as the syntactic complexity, this is not likely to make any difference at all in RT terms. However, other differences found are likely to have more of an impact on communication. Perhaps one of the most consistently agreed on types of difference are those related to the fact that written texts are normally more planned and are consequently more structured than spoken texts (Biber, 1988; Brown & Yule, 1983). The kinds of rhetorical structure seen in the scientific research article, with its classic structure of Introduction-Methods-Results-Discussion (Swales, 1990), is a clear example of this kind of structure.

RT and discourse analysis

This kind of text structure has not been intensively investigated by relevance theorists, but some work on discourse has been carried out, principally by Blass (1990, 1993) and Blakemore (1987, 1988). Blass criticises most discourse analysts for being overly concerned with the idea of coherence, which she dismisses as "merely a superficial symptom of something deeper", with that something deeper being a concern with context in the RT, cognitive, sense (Blass, 1990, p. 19). To see whether such a criticism is justified we need to examine what in fact coherence is considered to be by discourse analysts. Cohesion and coherence are widely recognised features of texts, so are they in fact as superficial as Blass claims?

The basis of Blass's criticism is that coherence is concerned purely with aspects of the text and does not concern itself with the really important factor, the cognitive aspects of relationship to context (Blass, 1993). She claims, therefore, that "discourse analysis is nothing else *but* tracing the hearer's part in understanding utterances"

(Blass, 1990, p. 11). To pay attention to linguistically encoded aspects of the text is not enough and contextual and inferential factors need to be investigated also (Blass, 1993). Similarly, Blakemore sees coherence as a relationship between linguistic units, whereas relevance is a relationship between propositions (Blakemore, 1987).

To read Blass and Blakemore, therefore, one gets the impression that all work on coherence prior to RT was resolutely anti-cognitive, concerned purely with the surface relations of the text. It may indeed be the case that some linguists, like Halliday and Hasan for example, place great emphasis on the surface properties of texts, but they were concerned with cohesion not coherence and theirs is by no means the only approach to discourse analysis (Halliday & Hasan, 1976). A number of different approaches to coherence are possible, with van de Velde (1984) listing a total of six, ranging from the linguistic through to the neurological. Thus, if we look at classic works on discourse analysis such as Charolles (1983), it is clear that coherence is defined in terms of perception of global intention, which is very much a cognitive point of view. Cohesive signals are seen as having no value unless perceived as such by the receiver of the text (Charolles, 1983). Explicitly cognitive theories of discourse organisation have also been put forward by many, perhaps the most influential being that by van Dijk and Kintsch (Kintsch & van Dijk, 1978; van Dijk & Kintsch, 1983). Even such mainstream representatives of the continental text linguistics tradition, which places much more emphasis on the formal properties of texts, as Petöfi and Sözer, state that one and the same text can be seen as "explicitly coherent, latently coherent or even incoherent depending on different interpretation processes and/or different interpreters" (Petöfi & Sözer, 1988, p. 454).

Despite the attacks on previous approaches to discourse analysis by Blass, therefore, it is by no means the case that cognitive approaches to discourse analysis were conspicuous by their absence. Perhaps, then, some of the concerns of discourse analysis as conceived outside the RT tradition might not be so incompatible with the RT perspective as might seem to be the case at first sight. Blass notes that to restrict attention to hierarchically structured discourse may be misleading, as it might result in our taking formal properties of such texts as factors necessary for textuality (Blass,

1990). On the other hand we should not commit the opposite fallacy and think that relative lack of structure in unplanned texts means that texts have no hierarchical structuring.

That texts have hierarchical structure in a cognitive sense as well is also widely appreciated outside RT: "(i)t is generally accepted that discourse comprehension entails the construction of a multilevel mental representation of text" (Fincher-Kiefer, 1993, p. 99). The van Dijk and Kintsch model is, of course, explicitly hierarchical, building up a series of different levels of text comprehension, the crucial distinction being between the textbase, or model of the text itself, and the situation model, which includes the incorporation of the context, in RT terms (van Dijk & Kintsch, 1983). Because different readers have different situation models, and different goals in reading a text, they also build up different macrostructures in reading a text.

Similarly, Britton and Black (1985) also argue that text comprehension necessarily involves the building up of higher-level knowledge structures, as otherwise the processing of any text would very quickly become impossible to manage. A similar distinction between higher- and lower-level information is made, for example, by Schnotz (1983), among others. Specifically looking at comprehension of scientific texts, Tardieu et al (1992) found various levels of representation, which differed between experts and non-experts. More generally, different discourse analysts have argued for different overall text-organising templates. Thus Meyer, for example, has only five very general relationships: collection, causation, response, comparison and description (Meyer, 1985). Hoey has even fewer: situation, problem, solution and evaluation (Hoey, 1983). Other analysts, such as Mann and Thompson (1988), hesitate to circumscribe the number of such relationships, but concur in seeing texts as having such general organisational characteristics.

Outside RT, then, there is a general recognition that planned written texts have a hierarchical organisation and indeed this distinction between planned and unplanned texts is one that is recognised by Blakemore. She sees the distinction as being that in unplanned texts, such as casual conversation, the role of the speaker is simply to make

each utterance relevant, whereas in planned texts the aim is to maximise relevance over the discourse as a whole (Blakemore, 1988). The implication here, therefore, is that in such texts not every proposition may be maximally relevant on its own, but may be doing the job of contributing to the overall relevance of the whole text. This is a somewhat problematic distinction, as the question then arises as to how we are to distinguish between a proposition that is meant to achieve relevance in its own right and one that is merely contributing to the overall relevance of the text. Nevertheless, it seems clear that here we have grounds within RT for regarding texts as having hierarchy, in the sense that not every proposition is to be interpreted merely as being relevant on its own but as contributing to the relevance of a higher level unit.

In addition to any overall structure that texts may have, though, the normal process of interpreting a discourse proceeds by interpreting each utterance in the context established by the preceding utterance. There is thus a gradually changing background against which new information is processed (Blakemore, 1987). This captures the way in which the hearer's or reader's understanding of the discourse gradually unfolds as the discourse proceeds.

The problem we are faced with though is how to reconcile the picture of a gradually unfolding discourse where every utterance is processed in a context produced by the immediately preceding utterance, or, more exactly, the context produced by the interaction of the immediately preceding utterance and the hearer's existing assumptions, and a hierarchical discourse where relevance is maximised over the discourse as a whole. In fact the problem is putting together the online processing as a reader reads a text, in which the processing is in the smallest and most accessible context produced in large part by the processing of the previous proposition, and the overall relevance of the text as a whole. That this is indeed a serious problem can be seen from Giora's claim that the very act of choosing an inappropriate initial context may lead to an incoherent discourse, leading her to argue that RT cannot be a theory of discourse coherence (Giora, 1997).

How one utterance may not be relevant on its own, but merely contribute to the relevance of the whole has been suggested by Unger (1993). He points out that a

speaker may, for example, preface a statement about an object by checking first of all that the object talked about is manifest to the hearer, e.g. "Do you see that house?" (Unger, 1993). Despite this, however, Unger does not believe that texts have a hierarchy at all. If we use the concept of optimal relevance, he claims, "no appeal to hierarchical discourse structure is necessary" (Unger, 1996, p. 428). Nevertheless, he does admit the existence of paragraphs, but explains paragraph breaks by seeing them as helping the reader in his search for relevance by guiding him in this search to access information other than the last utterance (Unger, 1996). It would seem, then, that Unger is recognising that paragraphs have a discourse function after all, although he explains this function in terms of the search for relevance.

In spite of the dismissal of discourse hierarchies, therefore, it appears that sometimes the reader is entitled to search for other propositions which might be more relevant in addition to the most recently processed one. Hence it does seem to be possible within the RT framework to imagine that the reader does at times make use of information suggested by the text which is not that made accessible by the most recent proposition alone.

However, we do seem to have a tension here in that Unger does explicitly claim that "in a relevance theoretic framework there is no reason to assume discourse segments in the sense of formal entities or units" (Unger, 1996, p. 418). On the other hand he does talk about the relevance of paragraph breaks in signalling to the reader the necessity to access a larger context. This seems to be making a rather unlikely distinction between paragraph breaks and paragraphs. The assumption of paragraph breaks entails the existence of paragraphs, which are a larger discourse segment of the type he argues against. What he seems to be saying though, with this way of arguing, is that it is the break between paragraphs which functions to help the reader with processing the discourse rather than the paragraphs themselves. Paragraphs are a result of the paragraph breaks rather than units in themselves. However it is put, nevertheless it remains the case that at some point the writer decides to produce a new paragraph, or terminate the previous one, and thus introduces a subsection of text.

Another way of looking at Unger's argument is to notice that he says that there is no need to assume *formal* entities or units. At one level this could be seen merely as an acknowledgement of the widely accepted position in discourse analysis of the impossibility of writing a formal grammar above the sentence level (van Dijk, 1997). What Unger seems to mean, though, is that there are no actual units of discourse above the sentence and that "paragraphs or other discourse segments have no role as primitives" (Unger, 1996, p. 431). Perhaps, therefore, we should take the burden of the argument of Blass, Blakemore and Unger to be that in RT any unit of discourse needs to be explained in RT terms, as Unger explains the paragraph in terms of reducing the reader's processing effort by signalling to him that he needs to access a wider context than the last utterance. Similarly, Blass does not so much deny the existence of the topic of a text as reinterpret it in terms of giving "access to the encyclopaedic entry which plays a relatively central role in comprehension" (Blass, 1990, p. 76; Matsui, 1993).

Given then the arguments in RT for maximising relevance over the text as a whole and the interpretation of standard discourse terms like coherence, paragraph and topic within an RT framework, we can take the position, not that texts have no structure, but that this structure needs to be interpreted in different terms from those posited in traditional discourse analysis. What I intend to do, therefore, in the rest of this chapter is to examine some of the claims I have made for the structure of popular science texts in terms of genre, as well as some other aspects of their text structure, and interpret those within an RT framework.

Text structure

Outside of RT it is widely acknowledged that texts have some sort of structure or organisation. That texts are not just a random non-linear collection of sentences is accepted by everyone, although just what makes a text a text is more widely disputed. On the one side we have discourse analysts like Halliday and Hasan, who in their classic work on discourse argued for the primacy of cohesion and surface features of text in constituting textuality (Halliday & Hasan, 1976, 1989). On the other are those

like Charolles, who see the text much more as a whole in terms of the intention of writer and reader (Charolles, 1983). Writers like Hoey see texts in terms of overall organisation, so that individual texts are held together by links between different parts of the text (Hoey, 1991). Others working particularly in the field of scientific texts are happy to accept the idea of text structure (Swales, 1990; see also Dudley-Evans, 1994, and others cited in Chapter 2, for example). The distinction between the two positions is concerned with whether texts have particular elements without which a text is not a properly constituted text, i.e. the structural position associated with genre analysis, or the position associated, for example, with Hoey (1991), that texts are held together by the different parts acting together, but there is no element which is obligatory.

Two sets of interrelated distinctions seem to be in play here. The two poles are surface pattern vs. cognitive interpretation and structure vs. organisation. Some analysts lay stress on the actual surface pattern of texts while others emphasise the need for cognitive factors. This distinction is sometimes, e.g. by Blass, (1990), expressed in reserving the term `text' for the actual linguistic formal object and 'discourse' for both linguistic and non-linguistic aspects. The other opposition lies between those who think that any text has organisation and is held together by this organisation, but need not necessarily have any specific formal elements, and others who argue that texts, often of a particular type, necessarily have certain structural components. Most RT discourse analysts would seem to be of the cognitive and organisation persuasion, as we have seen in looking at Blass, Blakemore and Unger above. This is a fairly common conjunction, but need not necessarily take place. Thus Hoey is an advocate of organisation rather than structure, yet stresses the surface pattern of texts (Hoey, 1983, 1991). The emphasis on structure and non-cognitive factors would be seen in most work on genre analysis. It is true that Bhatia (1993) argues that genre analysis is also cognitive, but he does not argue that genre analysis is a mainly cognitive orientation, merely that cognitive factors should be allowed for. Most other genre analysts, like Swales (1990), see genre analysis as social rather than cognitive in orientation (Bloor, 1998). What I would like to argue for is the fourth

possibility, structure and cognitive analysis (shown as AW below). The possibilities are summarised below in Figure 7.1.

	Structure	**Organisation**
Pattern	Genre analysts	Hoey
Cognitive	AW	RT

Figure 7.1 Countervailing tendencies in discourse analysis

This is not an argument that all discourse analysts would necessarily make these distinctions, that all could be fitted neatly into these categories, or even that the labels used are the most appropriate. Rather it is a way of encapsulating for heuristic purposes those similarities and differences which highlight the situation we are analysing. Thus it is clear that genre analysis and RT are in opposite quadrants, seemingly implacably opposed. It is the recognition of this opposition that makes this chapter necessary. Although I have endeavoured to show that RT in many ways supports the model of popularization of science I proposed above, nevertheless, it is clear that RT makes some assumptions about discourse which, on the surface at least, seem to be opposed to the model proposed. Therefore it is necessary to demonstrate that, despite this, RT is fully compatible with the model. To do so it will be shown that genre arguments can be interpreted in an RT framework.

One reason for attempting to demonstrate that RT is compatible with the genre framework is that genre analysis has been so successful in describing the type of texts looked at here, i.e. scientific texts. A large amount of work on EST, as evidenced by most issues of the journal *English for Specific Purposes*, is expressed within this framework. More broadly, the idea that scientific texts have a certain structure is a standard feature of scientific life expressed in the 'Notes to Contributors' of scientific

journals and demonstrated in the section headings of papers in most scientific journals.

If the text structure analysis of scientific texts, or genre analysis, is so widely accepted within the EST community working with such texts why is it that RT has been so insistent in denying the need to look at elements of text structure? The short answer to that is, clearly, that hitherto RT has been, as noted above, concerned mostly with non-authentic, invented examples and not with real texts. Even where, as with Blass, corpus data has been investigated, the corpus has been usually spoken data. In addition, the type of questions in discourse analysis that RT has examined up till now have been questions concerned with particular discourse particles like 'after all', or similar (Blakemore, 1987; Blass, 1990). That RT has grown in linguistic terms out of neo-Gricean pragmatics has meant that it has been preoccupied with questions which are more germane within that tradition than in the broader field of discourse analysis (e.g. Carston, 1988, 1993). This is not to say that RT is only concerned with such questions. Campbell (1992) has argued that RT can be used to investigate rhetoric, in the neo-Aristotelian sense, but his paper is largely programmatic. More to the point, as shown by the Postface to Sperber and Wilson, 1995, it is being used to investigate a wide variety of topics. Nevertheless its focus has been on narrower aspects of pragmatics rather than on applied discourse analysis of particular text types.

If then scientific texts and other more formal texts have not really been investigated by RT, then it may very well be the case, not that these texts have no hierarchical structure, but simply that these have not yet been examined within this framework. We should remember that Blass claimed, not that hierarchically structured texts did not exist, only that they should not be taken as typical exemplars (Blass, 1990). Similarly, the distinction made by Blakemore between planned and unplanned texts makes sense only if the former are seen to have some sort of organisational or structural difference from the latter (Blakemore, 1988).

Moves and RT

The idea of move structure put forward in genre analysis differs from the analysis of discourse particles by Blakemore or paragraph breaks by Unger in one immediately apparent respect (Blakemore, 1987; Unger, 1996). Whereas phrases like 'after all' of the type investigated by Blakemore, or paragraph breaks, are surface features of the text evident to any reader, moves in genre analysis are analytical constructs of the discourse analyst. This is not to say that they have no reality apart from their construction by the discourse analyst, or that they are not based on surface features of text, but it does mean that moves as such are not there on the surface of the text for the naïve reader to pick out. This is a somewhat paradoxical position, in that RT is at pains to stress that linguistic clues alone are never sufficient to identify a proposition and that inferential processes must be taken into account (Blass, 1993); yet there has been a great concentration on such clues by Blass and Blakemore (Blakemore, 1987; Blass, 1990). Nevertheless, before trying to determine just how moves have their effect it is worth while looking at what sort of effect has been examined by Blakemore and Blass.

The kind of features of texts investigated by Blakemore, 1987, are primarily discourse connectives such as 'so' or 'after all'. The function of such discourse connectives is taken as being to constrain the hearer's search for contextual effects by limiting the space in which the relevance of the utterance is taken to have an effect. In this way, the hearer's processing costs are reduced. For example, 'so' is used to point out a "specific contextual implication of a remark" (Blakemore, 1987, p. 86), so that the relevance of the utterance is more easily and quickly accessed by the hearer. Broadly, therefore, such discourse connectives constrain the semantic space in which the search for relevance takes place and thus have their effect on the effort side of the search for relevance by reducing the hearer's effort. Thus Blass sees such connectives in Sissala as constituting semantic constraints on relevance, contributing to reducing the hearer's processing load (Blass, 1990). Similarly, Unger regards paragraph breaks as constraining the search for relevance and reducing the reader's effort (Unger, 1996). This approach was broadened out by Wilson and Sperber (1993), who, following

Blakemore (1987), distinguish between conceptual semantics and procedural semantics. The common thread running through these arguments, therefore, is the idea of constraining the hearer's search for relevance. Let us turn now and look at how moves might fit into this picture.

If RT sees various discourse particles as helping the reader interpret the propositions in the text might moves not act in somewhat the same way? First of all, it must be considered how the reader goes about identifying the moves. In fact, there are clear linguistic clues to the moves which help the reader identify them. Thus Swales (1990) gives numerous instances of the kind of linguistic exponents of the Moves he established in his CARS model. If we look at a few examples it becomes reasonably clear. Thus he provides several examples of exponents of Move 1, Step 1. These are obviously examples of centrality claims, but none of them actually says so explicitly. The expressions used are phrases like "Recently, there has been wide interest in..." or "The theory that ... has led to the hope that", to take two examples at random (Swales, 1990, p. 144).

Now if we consider the relationship of the step, Claiming centrality, to the exponents of that step, the question that naturally arises is whether steps/moves are explicatures or implicatures. On the one hand explicatures, however they might be enriched, are closely based on the language of the expression which they rest on. Implicatures, in contrast, need have no linguistic relationship with the expression which gives rise to the implicature. It seems apparent that the step, Claiming centrality, cannot be said to be closely based on the precise language of the text. This means, therefore, that steps or moves are not explicatures of the text.

Could they then be looked at as implicatures? If we consider that implicatures are assumptions that follow the interpretation of an utterance by a hearer who is presuming that what is said is relevant, then it would seem reasonable to interpret a claim of centrality as an implicature of expressions like "Recently there has been wide interest in..." when found near the beginning of an RA. That the writer is intending the reader to interpret the sentence of which the quotation is part as a claim that the

area she is investigating is important seems a perfectly rational interpretation consistent with the Principle of Relevance.

The same idea can be applied to the moves of the popular science genre. Each move is expressing an implicature which is indicating how the reader should interpret the propositions contained in the move. Thus Move 1a) introduces an example of the type of case discussed in the text, so the implicature here is one of exemplification, indicating to the reader that the Move is to be taken as a Personal example. More generally Move 1, Relating to the reader, can be taken to mean that the whole move is to be understood as relating the science in the text to the reader.

How is this implicature arrived at by the reader? If we consider Move 1a), this often starts the text with a description of a total stranger who is quite unknown to the reader. This could not be seen as relevant by the reader unless he considers that this person is to be taken as an example of the problem discussed in the text. The other moves can be explained in a similar way. Thus, Move 6, for example, Evaluation of findings, is understood by the reader to be an instruction to interpret the move as an evaluation. The rest of the moves can be interpreted along the same lines, so that Move 2a), for example, is to be interpreted as announcing the main research findings.

Although the moves are implicatures of the texts which contain them, there are, nevertheless, linguistic clues to the moves which help the reader identify them. As we saw, Swales (1990) gives numerous examples of the kind of linguistic exponents of the moves he established in his CARS model. In the popular science texts studied here, the moves are not so clearly marked by linguistic means, but there are still, nevertheless, linguistic indications in the text. Thus Move 1a) is signalled by the use of a personal name of a member of the public. Move 2a) is shown by clues like the naming of one or more of the researchers involved, an indication of recent time by means of a time adverbial or similar, and so on. Examples of how to identify the moves were given above in Chapter 2, so there is no need to repeat these here, but in general it can be said that often linguistic clues are there, but usually context is necessary to identify the move in question.

Moves and text processing

It is suggested, then, that the moves can be seen to be implicatures of those parts of the text which are exponents of these moves. But it remains to be answered how these moves have their effect and how they help the reader to process the text. Let us, therefore, examine the picture in more detail in order to trace how this is done. If we consider first of all probably the most well established type of move, that of Swales' CARS model of RA introductions, we can see immediately how the moves function to constrain the processing of the reader (Swales, 1981, 1990). For example, when an article starts by referring to a particular area, the reader understands not only the proposition conveyed by the particular sentence in question, but also, because of the move structure, that this is the general area which will be investigated in the article. If anomalies are mentioned, or areas which have not yet been investigated, the reader knows that this is not just out of casual interest, but that the writer is creating a niche for her research. Mention of an article is taken to be part of the review of the literature, as this is part of the move structure of such types of article introductions. So the moves constrain the way the reader interprets the proposition in the text. The information in the text is interpreted in terms of the move it is a part of.

None of this is conveyed simply by the propositions of the article itself. Thus if an article starts "The increasing interest in high-angle-of-attack aerodynamics has heightened the need for computational tools suitable to predict the flowfield and the aerodynamics in this regime" (quoted in Swales, 1990, p. 143) or similar, this tells the reader only the facts included. However, the reader interprets it also to mean that the article will be about this general area. It might be argued that of course the reader interprets it in this way, because it is the first sentence of the text and the first sentence of any text will be interpreted to be about the main subject matter of that text. But this is far from being the case. Does a novel continue the topic introduced in the first sentence? Or is the *Canterbury Tales* about April? Not exactly. We do not interpret the first sentence to mean that the rest of the text will be about the same topic because of our knowledge of the conventions of the genre. The same thing happens with research articles in science, but with them we interpret the first sentences as

introducing Move 1, Establishing a territory. In other words, being Move 1, we interpret the information within that move in terms of that move.

If such texts create an effect in the reader beyond that brought about by the explicatures embodied in the texts, this is only to be expected in RT terms. By recognising the genre concerned and the particular move of that genre the reader is constrained to interpret the information in a particular way. Thus reading the introduction to the RA by Miki et al., the first paragraph introduces the topic of breast and ovarian cancer, with the next paragraph specifying genetic factors, so this is recognised as the general research area involved. The text then goes on to the topic of one gene, *BRCA1*, and the efforts to find this gene, so this is identified as the niche. Then the niche is occupied by reference to localisation of the gene.

It is only by knowledge of the genre and the moves of the genre that such interpretation can take place. As the typical reader of the genre has internalised such information, he is helped greatly in understanding the text by the fact that the RA follows the conventions of the genre. If each researcher wrote in her own style it would make it much more difficult to gain the required effects. But the moves of the genre are not overtly labelled in the text. The nearest that any move comes to being overtly labelled is Move 1, Step 3, Reviewing items of previous research, which is indicated by bracketed numbers. Otherwise, the rest of the moves are implicit rather than explicit in the text. Therefore, they are implicatures of the text rather than being directly expressed in the text.

It should be pointed out that this kind of processing of the moves is over and above the actual information given in the particular moves. Thus the reader gains contextual effects from the propositions in the text. But, as has been shown, in addition to the actual propositions encoded in the words of the text, the reader also implicitly recognises these propositions as embodiments of particular moves. Thus the reader accesses the proposition conveyed by the text and recognises the move it expresses. Hence, the fact that the first sentence of the Miki et al. text "Breast cancer is one of the most common and important diseases affecting women" functions as a centrality

claim, Move 1, Step 1, does not affect in any way the truth or falsity of the proposition. The moves act functionally in the text; they do not state facts about the world.

What happens, therefore, is similar to what Blakemore shows discourse connectives to be doing in that moves also constrain the semantic space in which the search for relevance is sought. In the case of moves, the contextual effects produced by the propositions in the text itself are constrained by having to be interpreted in terms of the move of which that proposition is a part. Perhaps, though, `constrain' is the wrong term here, in that this suggests reduced effects. In fact, by interpreting the proposition in terms of it being an exponent of a move, the reader gains increased cognitive effects. Thus the statement just quoted at the beginning of the Miki et al. text, "Breast cancer is one of the most common and important diseases affecting women" in itself does not produce many contextual effects in the audience of breast cancer researchers reading the text. Since this fact is known to them and any proposition only has an effect to the extent that it interacts with the cognitive context of the reader to create new effects, this proposition, in itself, has no new cognitive effects. This use of information which is already known to the reader functions to bring it to the attention of the reader and acts as a reminder (Sperber & Wilson, 1986/1995). By itself, therefore, this statement does not bring any great contextual effect. The contextual effect is achieved by its positioning at the beginning of the text, where it functions as a centrality claim. The contextual effect is achieved in this case primarily because of its functioning as a step in a move.

Moves of the type identified by Swales in the RA, then, seem to help the reader's processing of the text by indicating the kind of information involved. But there is a difference to some extent with the moves in a popular science text. Whereas the scientific researcher could be said to be familiar with the conventions of the science RA, especially in a journal he reads regularly, the same, it might be argued, cannot be said of the reader of a popular science article. Certainly, it is true that the conventions are more regular and more strictly followed in an RA, because of the gatekeeping effect of the editorial policy of the journal in question. Nevertheless, even in a journal

the reader does not consciously recognise the moves in question. That the moves of the popularization are not recognised overtly as such by the reader, then, is not an argument against such moves.

Let us examine, then, how the moves of the popular science article might act to produce greater relevance for the reader. As an example (to reduce the processing effort of the reader!) we can look at an article we are already familiar with, Text 7, *Zeroing in on breast cancer*. This article, like a number of others we have analysed, starts off with a Move 1a). Now the reason for this in journalistic terms is that it functions as a hook to grab the interest of the reader, but in what way can it be said to help the processing effort of the reader? The paragraph discusses the example of a particular family, the Cunninghams, who have hereditary breast cancer. At first sight, this might not seem to be relevant to the reader at all, since the individuals mentioned are not known to the reader. Since the individuals introduced at the very beginning of the text are unknown to the reader, the reader would fail to see the relevance if he misunderstood the significance of the individuals portrayed. But just because the reader is familiar with the popular move of using typical members of the public as examples of the topic discussed, it makes it much easier to process the information in this paragraph.

To appreciate this, we can imagine the difficulty a reader who came from a culture where such texts were not found would have when faced by this text. There would be great difficulty in identifying the protagonists involved at first. But the conventions of the genre for those familiar with them mean that these particular individuals should simply be taken as typical examples of many others like them affected by the discovery highlighted in the story. In fact if the conventions of the genre were not understood, there would be great difficulty in attaching any contextual effect to most of the first paragraph. The reason for this is, simply, that in itself the fact that Charlene Cunningham was 25 when she discovered a lump in her breast almost four years before the article was written has very few contextual effects for the vast majority of readers who do not know the individual in question. Unless the individual and family in question are taken as examples then the relevance of their situation is

minimal to the audience. Thus it is only by recognising, albeit subconsciously, the move as being one where a Personal example is given that the paragraph becomes at all relevant. So what the move does is establish the relevance of the paragraph by showing how the paragraph can be interpreted in a way that makes it relevant. The contextual effects of the information are greatly increased once the information is seen as constituting a Personal example. Recognition of this opens up the effects for the reader. Without such a recognition, the processing costs are greatly increased and the cognitive effects similarly diminished.

We can interpret further moves in the same way. Thus Move 3, Description of background ideas, shows how the information given is to be interpreted. It is saying why the information in these sentences is important and why it is worth processing. In itself the information given in such a move has reduced relevance and only when it is understood as background necessary for understanding the main research finding can it be seen to be sufficiently relevant. If we take an example of Move 3, from Text 5, we see there that in order to appreciate the problems of deciding on intervention or not in the case of prostate cancer, it is necessary for the reader to know, first of all, what the prostate is, the fact that it typically develops a benign swelling in most men by age 65, and that, although in men over 50 typically 30-40% have cancerous cells in the prostate, very few die from it. Without such information, the reader cannot process properly the key discussion of the article on whether or not to have treatment for prostate cancer and, if so, what treatment. Understood as background information necessary to interpret other more important information, these propositions are more easily processed.

Understanding the moves therefore helps put the information in the moves in a context that makes more sense. But moves also act to put the information in the text as a whole into perspective. Thus the remaining social moves, Moves 6, 8 and 10, help the reader to put the information given in the article into context, in the general sense as well as the RT sense, as they give an indication of how seriously to take the research findings described in the magazine article. Move 8 explains anything which is difficult for the reader to understand about the findings and thus helps him to accept

them. Moves 6 and 10, on the other hand, indicate how important the research is. The first of these, Move 6, helps the reader to evaluate the research by giving the views of either the journalist herself or other scientists on the research. Move 10, of course, relates it directly to the readership.

It should be remembered that this kind of processing of the moves is over and above the actual information given in the particular moves. Thus the reader gains contextual effects from the propositions in the text, in addition to the influence on processing of the moves themselves. What the move structure does is help in the processing of the conceptual information in the text. Thus, for example, Move 6 evaluates the research and the actual conceptual evaluation is processed by the reader. But in addition the reader processes the move as an evaluation.

All of these social moves, therefore, help the reader to place the information given in a more helpful framework, thus reducing the processing effort and gaining an appropriate cognitive context in which to process the information and achieve maximum relevance. Moreover, this simplicity of processing because of the move is in addition to any conceptual information given explicitly in the text itself. The move structure acts in addition to the conceptual information processed in the propositions themselves. What is being claimed here, therefore, is that the move structure of the social moves channels the information in the article so that it is more easily processed by the reader and the processing cost is therefore reduced. Because the social moves are aimed at the general readership and are intended to help the average reader not particularly interested in science to see the relevance of the research reported to his own life, what these moves do is present the information so that it appears more a part of the reader's interests and concerns.

The move structure also helps to separate out the scientific information which is being presented. The actual discovery and scientific information related to that discovery comes in these moves and by their being presented as scientific moves the reader is having the information packaged in a helpful way. This is the new information which needs to be processed and which will have or will not have the desired contextual

effects in the reader. The job of the social moves is then to make the information presented in the scientific moves easier to process and thus to increase the contextual effects.

Information in the scientific moves is made more relevant by being made easier to process because the reader is helped to see their relevance by the social moves. The social moves in other words act to increase the contextual effects of the information in the text by making the information in the text more easily processed by showing the information is relevant, in the everyday sense, to the reader: that is the scientific information is made easier to process by being shown to be relevant. However, this does not apply particularly to the social moves themselves, which are not difficult to process. What the social moves do is directly increase the contextual effects of the information in those moves by demonstrating their relevance to the reader. In the social moves themselves the information has been made directly relevant to the reader. This can be seen especially in Move 10, Significance of findings to the reader.

If the moves help the reader to process the information in the text by directing him to the appropriate cognitive context in which to process the moves, this can be clearly seen to increase the relevance of the information in the text. However, it might be seen to suggest that the reader has to consciously recognise the move concerned in order to gain the required effects. But this is not necessarily the case. First of all, it sets up too strong a dichotomy between conscious and unconscious recognition. Before Swales' work in the late seventies and early eighties (Swales, 1981) the moves of the introduction to a research article were not consciously recognised as such. Nevertheless, both writers and readers of the RA in fact produced and understood them. So it is not necessary to actually fully recognise the moves and steps in the way that the discourse analyst does for one to be aware of them. The researcher reading the introduction to an RA, therefore, can recognise that the writer is, for example, making a counter-claim without having to label it as Move 2, Step 1A.

In a similar way, the reader of the popular science magazine article can recognise that Move 1a) is a Personal example without labelling it as such in his head. Therefore,

the claim is not being made that the reader of the magazine article overtly recognises the moves as such, merely that the contextual relationships involved are accessed in interpreting the article. Thus the claim is that the reader interprets the information in a Move 1a) as implicating a Personal example and by recovering the implicature is helped to process the proposition involved in a more appropriate context. More generally, it can be stated that the reader in interpreting a popular science magazine article, by recovering the moves in the form of implicatures, is helped to interpret the propositions in the text in the appropriate cognitive context and by so doing reduce his processing effort and increase the contextual effects.

Implicatures in the RA and the popularization

However, there does seem to remain a difference between the RA and the popularization in the extent to which the implicature has an effect. If we go beyond the article introductions discussed above and consider the RA as a whole, there is very often an overt labelling of such sections as Methods or Results. Clearly, therefore, the concept of the RA being divided into such sections is much more likely to be recognised as a normal feature of the text type by the reader of an RA. This is increased by the fact that readers of an RA are very often writers of an RA as well and used to the construction of such articles. It could also be argued that it is a lot more important to a researcher to be able to recognise, for example, a new claim that is being put forward in his field than it is for the reader of a magazine article to recognise background ideas.

All of this suggests that it is very important for the reader of an RA to recognise the implicatures involved. For this reason, the move structure of an RA is much tighter than that of the popularization. In terms of the implicatures involved these are very often strong implicatures. The idea of strong and weak implicatures is related to the extent to which the speaker constrains the interpretation of the hearer (Sperber & Wilson, 1986/95; see also Blakemore, 1992). Thus the stronger the implicature the more the speaker takes responsibility for the implicature the hearer recovers. In RA's, the implicature that, for example, the first move is Establishing the territory, is very

strong, not only because of the markers of the move in the text, but also because of the ubiquity of the move in that position in RA's. Similarly, Move 1, Step 3, Reviewing the literature, is very strongly implicated by the numbering system employed. Only someone totally unfamiliar with the conventions of the literature would fail to recover the implicature.

Because of the importance of the implicatures involved to the proper processing of the text, therefore, such implicatures in the RA are very strongly communicated. In popularizations, on the other hand, the communication is not so strong and the implicatures are weaker. It is not vital to most readers that they understand every implicature. If a reader misconstrues background ideas as background research instead, it will make processing a little more difficult, but it will not make it impossible to understand the text. Thus implicatures are weaker in the popularization and as a result the move structure is not so tight. As we have seen, many moves are not found in all texts and the order of moves is highly variable. This is only to be expected when the implicatures are relatively weak. On the contrary, to find that the communication is as strong in a popularization as in a research article would be a highly unlikely outcome. That Nwogu (1991, 1997) does find that popularizations are almost as strong in their communication as RA's is enough in itself to make us doubt the accuracy of his model, as we have seen above on p. 56.

What seems to happen, therefore, is that the strength of communication of the implicatures in a text will vary according to the extent to which the recovery of such implicatures helps in processing the text. This is a finding quite in tune with the idea of strong and weak implicatures. Because the recovery of the moves in an RA helps greatly in processing the text the moves in such a text are more strongly implicated. Conversely, the fact that moves are less strongly implicated in the popularization means that the recovery of the implicatures, although helpful, is not so vital. More precisely, we can see that the identification of some moves is more important than others. Thus, as we saw above, the identification of Move 1a) as a Personal example helps the reader greatly in processing the text. This move, therefore, is fairly strongly implicated in the text by the fact that an actual name is used. Similarly, Move 2,

which is vital to the understanding of the text, is made easily recoverable by being marked with various signals, such as the names of researchers, a specification of what was discovered, noting of the time period involved, etc. The reader is helped in this way to pull out this particular discovery from the text and to note it as especially important.

A factor related to the strength of the implicatures in the text is the connection between the explicatures and the implicatures involved. It is a basic concept in RT that what happens when the hearer attempts to work out what proposition is being communicated is that "the first interpretation tested and found consistent with the principle of relevance is the only interpretation consistent with the principle of relevance" (Sperber and Wilson, 1987, p. 744). In other words the reader does not need to go through a vast number of possible interpretations to find the correct one. But it also means that the intended interpretation may not simply be the one expressed by the explicature of the proposition. If this does not yield sufficient contextual effects then the reader will continue searching for an interpretation that will bring such effects. This gives a way of working out how the implicatures which constitute the moves are arrived at.

The reader of the RA, where the moves were very strongly implicated, arrives at such implicatures because of the fact that the explicatures of the propositions he is reading in the RA will not, in many cases, bring sufficient contextual effects on their own. Thus, as we have seen, when the first sentence of the Miki et al. text is read, giving the proposition that breast cancer is one of the most common and important diseases affecting women, this in itself is not going to trigger sufficient contextual effects. By the Principle of Relevance, therefore, this could not possibly be the intended interpretation. The reader will, therefore, search for another interpretation, and will find it in the implicature that this is the area that the RA is going to discuss. In a similar way, he will realise that reference to the efforts to isolate the gene since it was first mapped to chromosome arm 17q in 1990 indicates that the article is part of this search and a continuation in the tradition. The breast cancer researcher, of course, knows very well that intense efforts have been under way since that date to find the

gene – indeed he might very well have been part of those efforts. Telling him this, therefore, brings no contextual effects in itself, but the implicature that this RA can be seen in the context of this work does bring its effects. In addition, there are further, weaker, implicatures, that these efforts have finally borne fruit. These implicatures are not strong at this stage but they will be followed up later in the article.

The explicatures of the RA, then, do not bring sufficient contextual effects to the reader of the RA introduction and he is forced to access other interpretations in order to make sense of these propositions. Interpretation in terms of the conventions of the genre brings the appropriate contextual effects and he can be assured that these are the effects intended by the writer.

Something of the same general pattern happens with the popularization. But there are differences in the way the interpretation is arrived at. The reader of the RA is forced to access further implicatures in order to arrive at a satisfactory interpretation because the explicature gives insufficient contextual effects due to its being already known. In contrast, the reader of the popularization is forced to access the implicature because the explicature fails to provide sufficient effects because of the unknown information contained therein. Thus the reader of Text 7, faced with the proposition that Charlene Cunningham was 27 when she first discovered a lump in her breast, will not fail to achieve sufficient contextual effects because this is already known information. The failure to achieve the appropriate effects here is for the opposite reason – this information fails to intersect with the appropriate cognitive context to yield the effect. Whereas the RA reader already has the information and thus gets no effect, the popularization reader cannot find the relevance of the information because he has insufficient information. The reaction is the same, however: the search for an implicature that will generate sufficient effects. In the case of the popularization the interpretation arrived at is that this is a Personal example.

Thus whereas the interpretation of the RA is deficient on the effect side, in that not much effort is needed but insufficient effects are found, the interpretation of the popularization fails on the effort side. Too great an effort is needed to make the fact

that someone called Charlene Cunningham found a lump on her breast four years ago in itself yield cognitive effects. Overall, however, the effect on both readers is the same, they go on from the immediate explicature to find another interpretation more consistent with the Principle of Relevance.

Although in this situation the RA fails on the effect side rather than the effort side of the equation, we cannot say that this is always the case. It may be the case in areas of the RA other than the introduction that the explicature does not yield so few effects. Thus a claim for a new result will not have so little impact on the effect side, for example. In addition, it must be remembered that effect and effort are closely interrelated and it is somewhat artificial to separate them in many cases. Nevertheless, it might very well be the case that the implicatures of RA's have their relevance more in increasing the effect rather than decreasing effort. Even in the case of the announcement of a new result the impact will be on the effect side and the implicature that this is a new claim for a result is that the effect will be even greater. The processing effort is not decreased much.

With the popularization, it is more likely that the consequence of the implicature will be to reduce the processing effort required of the reader rather than to increase the effect. We have seen this to be the case with Move 1a). If we consider the other moves, it seems likely also that the implicatures might have a tendency to reduce the reader's effort rather than increase the effects. Thus if a reader recovers the implicature that a section of text is a Move 5, rather than a Move 7, i.e. Further description of findings, rather than Description of further findings, this helps him understand the text more easily by helping him to relate it more efficiently to another section of text, rather than directly increasing the contextual effects. On the other hand, recognition of a part of the text as Move 10, Significance of findings for the reader, would directly increase the contextual effects rather than decrease the effort.

Therefore, there does not seem to be an absolute distinction between the implicatures in the RA and the popularization, the former increasing the effect, the latter decreasing the effort. Given the interrelationship between effect and effort this would be unlikely

in any case. Nonetheless, there does seem to be a definite tendency in this direction. Further research on other moves in the RA would be needed to establish whether this is in fact the case. However, this might be expected from the audiences concerned in that the RA is aimed at an expert audience while the popularization is trying to make a complicated and difficult topic relevant to a mass audience.

Relevance over the whole text and the unit of relevance

This way of looking at move structure also helps in solving a problem that was noticed but not directly addressed above, that is the idea put forward by Blakemore that a planned text maximises relevance over the discourse as a whole (Blakemore, 1988). The problem is how to distinguish between a proposition that achieves sufficient relevance on its own account and one which is relevant only in terms of the discourse as a whole. Alternatively, every proposition could be said to be both maximally relevant on its own account and maximising relevance over the discourse as a whole. In such a case, however, the distinction is fairly meaningless, as it would imply only that a proposition made sense in terms of its relationship to the immediately preceding section of text and in terms of the whole text, which is a truism of any well constructed text.

It seems more likely that Blakemore here is putting forward the concept that some parts of a discourse will not have much relevance on their own. It is clear that this applies to the first sentence of a novel, which, on its own may have very few, if any, contextual effects. It may even be difficult to determine a fully truth-conditional proposition as it may be impossible to determine the referents. We may take it, therefore, that texts may contain elements which do not have their full effect in themselves but instead act more in terms of the overall text.

This would seem to be the case with various moves that we have identified in popular science texts. Thus it is clear that an introductory Move 1a, a Personal example of the type that we saw in Text 7 with the Cunningham family, for instance, does not have its full effect on its own. The aim of this move is to help the reader relate more

readily to the information contained in the scientific moves by showing how they impact on the family named, thus increasing the relevance of the rest of the information in the text. Similarly, Move 3, Description of background ideas, can be seen to have its effect in making the science of the scientific moves easier to process. Some moves, therefore, seem to be of lesser relevance in themselves, and act more to increase the relevance of other, more important moves, in the text.

Which are the most important moves can be seen from whether they are regularly found or not. Thus Move 3 is not found in many texts, and neither are Moves 6 or 8. These moves, then, are clearly subsidiary and act to increase the relevance of other moves in the text. But other moves are obviously central in that they occur in practically every text. Moves 1 and 2 are moves of this type, the central social and scientific move respectively. Both scientific and social moves are necessary for texts of this type of popular science. Without the scientific moves there would be nothing for the social moves to communicate, while without the social moves the scientific moves would not be relevant. However, the main aim of such texts is not to report the scientific information as such, but to relate it to the concerns of the audience. In this respect, therefore, the social moves are paramount. Although, both types of move are absolutely necessary for this type of text and no text in the corpus has moves of only one type (with the exception of Text 15, where, as noted above, the social moves are separated out in a box), nevertheless, it seems that social moves are more relevant than scientific. The reason for this is that they are inherently easier to process than the scientific moves because of the nature of the information contained in them, on the one hand. On the other, some of the social moves, especially Move 10, Significance of findings for the reader, have important contextual effects for the reader.

Relevance theory, therefore, gives us a way of determining the relative importance of different parts of the text in terms of their respective relevance. Looked at in this perspective, it seems that we can argue that the social moves are more relevant than the scientific. Their ease of processing is greater and their contextual effects may also be greater. Thus RT is not only compatible with our model in this respect, but acts to

reinforce it. Maximising relevance over the whole text can be seen to be in operation in our corpus.

A further question related to the maximisation of relevance over the whole text is the problem of what is the unit of relevance. Once again this is related to the fact that RT was established on the basis of spoken, conversational data and the unit chosen is more appropriate to such data. Thus most of the examples in Sperber and Wilson, (1986/1995), are single sentence utterances conveying a single proposition. The basic unit seems to be taken to be the utterance, although this is not defined. The textbook version of RT, Blakemore (1992), is called *Understanding utterances*. The central place of the utterance is also seen from the fact that the utterance is seen in terms of its being an interpretation of a thought. An utterance of course conveys a proposition and it is from this that the hearer derives the contextual assumptions intended by the speaker.

Thus it seems that the basic unit is indeed taken to be the utterance. An utterance in its turn is taken to interpretively resemble a thought. This notion of an utterance as an interpretation of a thought has been used by Unger to constitute a definition of an utterance, as follows: "an utterance is a linguistic behaviour whose explicatures or sequence of explicatures is used to interpretively resemble one thought which the speaker intends to communicate" (Unger, 1993, p. 12). On the basis of Kurdish data Unger then argues that what on the face of it might seem to be several utterances are in fact one because they interpretively resemble one thought. Thus the following is taken to intepretively resemble one thought and thus be one utterance:

> "therefore they joined together, the leaders of the village
> talked to one another – at that time Zakho was a village –
> they sat together and said: 'we will build a bridge
> to help us so that we can bring goods to the other side
> that our sheep and goats may go over it, and that people
> want to go to the Behdinan area or to Kurdistan on the
> other side (may go over it)'" (Unger, 1993, p. 10).

This is said to interpretively resemble the thought "'the people of Zakho must make effort (sic) to build a bridge'" (Unger, 1993, p. 10). Interpretive resemblance is said by Unger to constitute having "some contextual and/or analytical implications in common" (Unger, 1993, p. 10). In this he is following the standard RT idea that an utterance shares the logical and contextual implications of the thought it is expressing (Blakemore, 1992; Sperber & Wilson, 1986/1995). The problem is that the utterance contains considerably more than just some contextual and analytical implications in common with the thought. The utterance as quoted contains considerable information which is neither entailed nor implicated by the thought it is said to resemble. Thus the utterance conveys the propositions that Zakho is a village, that the leaders of the village discussed the bridge, that the bridge led to the Behdinan area etc. None of this information is contained in the thought the utterance is supposed to resemble. What is considered to be a thought might be an appropriate summary of what is considered to be the utterance, but thoughts can hardly be considered as summaries of utterances.

The problems inherent in this analysis can be seen from the fact that in his later work (Unger, 1996), Unger describes the data quoted above as constituting more than one utterance, so he seems to have given up the analysis of the earlier paper. Such an attempt to define an utterance in terms of relationship to a thought, therefore, cannot be seen as the end of the matter. In addition, we have to reconcile the idea that the unit of relevance is coterminous with the utterance with the claim by Blakemore, discussed above, that relevance can be maximised over the discourse as a whole. In addition, Blakemore has argued that in reformulations, the first segment, which is then reformulated, be seen as providing assumptions which facilitate the understanding of the second segment (Blakemore, 1993). It seems clear, therefore, that not every segment of a discourse can be seen to have the same degree of relevance. Therefore the unit of relevance is still unclear.

If we consider this problem within the framework of genre analysis, it seems evident from a genre point of view that texts of a certain genre must be seen, if not in all cases, at least in a substantial majority of cases, to be constituted by particular moves. Thus an introduction to a research article would not be a proper introduction if it did

not contain the 3 moves found in Swales' CARS model. In the model of popularization proposed here, the popular science text of the type examined would not be a complete text if there were no social moves. Or if there were no Move 2, Description of main research, the text would not be of this type. From a genre point of view, therefore, it is clear that texts can be defective because a certain type of information typically found in those texts is missing. It must be remembered here, as discussed above in Chapter 2, that genres typically have moves which are not found in all texts and that, as Swales points out, the inclusion of moves is never a categorical matter as it is with syntactic elements (Swales, 1990). Nevertheless, within genre analysis it is normal to posit certain moves as being characteristic of a certain text type and those texts which seem to belong to such a type but which lack a move of such type as being at the least atypical, if not defective.

For a text to be complete, therefore, in genre terms it normally would contain certain moves. The reason for this is that each move often only makes sense in conjunction with another move, or other moves. Thus Swales' Move 3, Occupying the niche, is dependent on a prior Move 2, Establishing the niche, for example. In my model, a text would typically not be complete without a Move 9, or Move 10, which establish the significance of the findings for future research or the reader, respectively. In an extreme case, of course, a move does not only not make sense except with other moves, but the converse of this holds. All, or at least many, other moves do not make sense if a particularly important move is lacking. In our corpus, none of the other moves would make sense if Move 2 were lacking.

The perspective here, therefore, is of each particular part of the text contributing to the structure of the text as a whole. Each move contributes to the overall structure of the text as part of the genre. Thus, by itself, each move is incomplete. It only gains status as a move by being part of a text to which other moves also contribute. Each move to a certain extent is defined by being in relation to the other moves, as we saw above with Swales' moves 2 and 3. In RT terms, therefore, each move cannot be completely relevant on its own, but only as part of a whole discourse. The corollary of this and the logical implication of maximisation of relevance over the whole text is

that the unit of relevance for planned discourse is the text as a whole. This applies first and foremost to written text, but also applies to the kind of planned discourse which may be spoken, but which functions as a whole discourse in the same way as a written text. An example would be a conference paper. Thus when a speaker is cut off by a strict chairperson because she has run out of time, her paper fails to have its full contextual effects and is less than fully relevant.

This would not be the case for unplanned discourses like casual conversation, as Blakemore says. Since casual conversation can be broken off at any time and the topic can jump around, it does not have the same overall relevance as planned discourse. In such conversation, an utterance will only have relevance in the context provided by the immediately preceding utterance, but even here the links with the preceding utterance may be weakened by topic jumps and thus the relevance lessened (Jucker, 1993b).

Planned discourse, typically that found in written texts, will, however, attempt to maximise relevance over the whole text. This does not mean that particular propositions in the text will not be found relevant by the reader as he reads. What it does mean is that the full relevance of any part of the text cannot be properly appreciated outside of the context of the whole text. Both of these facets are necessary. If a reader does not find the discourse sufficiently relevant as he reads, he is likely to just stop reading and never complete the text. Conversely, a text which is read to the end and then found not to make sense as a text will be felt to be irrelevant, or less than fully relevant, by the reader, despite the fact that particular information in the text may be found to be relevant.

We can consider what it would mean if relevance was seen only to inhere in individual utterances. This would mean that each individual utterance could be relevant but that the whole discourse has no status at all. It might be argued that if the discourse did not hang together as a whole then individual utterances would not cohere and therefore would not be relevant. But unplanned discourse, as in casual conversation, shows how each utterance can be relevant in terms of the previous

utterance, but the whole discourse goes nowhere in particular, in that there is no overall pattern. In fact the extent of topic shift is almost a defining feature of casual conversation (e.g. Sacks, 1992). This lack of an overall pattern is not noticed by participants in casual conversation because they do not expect relevance to build over the whole discourse. However, if this happens in a planned discourse, then the reader is not satisfied and considers the discourse to be incoherent as a whole, despite the local coherence. In other words the text has not maximised relevance over the whole text.

Thus it makes sense both within genre and relevance theory to see the whole text as a unit. In genre theory this is because moves only make sense along with other moves and in the context of the text as a whole. RT, however, can see the text also as a unit of relevance, since a text has a relevance over and above the information in particular parts of the text. This is quite consistent with previous work on RT. What has been done here, though, is to draw out more fully an implication of the idea of maximising relevance over the text as a whole.

In the last several sections, therefore, I have endeavoured to show that aspects of the text associated with genre, particularly those connected with moves and move structure, can be seen to be compatible with RT. Although RT has been reluctant to take into account discourse units larger than the utterance, it can be seen that the viewpoint can be integrated with a more traditional view of discourse analysis which considers texts to have a textual structure and that discourse units do exist and help to hold together a text.

Despite, then, on the face of it, considerable differences of approach between RT and genre analysis, we can see that in fact the two are not in fact so incompatible as they might at first appear. Going further, it has been argued that RT has been able to bring out aspects of genre theory which were hidden because of the non-cognitive nature of that theory as usually put forward. In the following, concluding, chapter, therefore, we shall endeavour to draw out further a number of ways in which RT and genre

analysis could profitably interact, as well as some implications for future research of the model and approach suggested here.

CHAPTER 8

CONCLUSION: SOME IMPLICATIONS FOR THE POPULARIZATION OF SCIENCE, RELEVANCE THEORY AND GENRE ANALYSIS

In this concluding chapter, I would like to draw together the multiple threads that have been woven through this work and see what implications the model proposed herein might have in some of the areas investigated. The fields looked at here and the methods of analysis involved, in many cases, have not been applied in this way before, so there are many aspects raised by the model put forward here that could not be adequately covered in the space available. This is not a drawback, however, since it means that the model is sufficiently powerful that it suggests other interesting areas of investigation and provides insights into how such investigation might proceed. To start with I shall make some suggestions on how the model might have further implications for the popularization of science. In the following section I suggest some aspects of relevance theory that might profitably be looked at. Finally, the advantages of the model put forward for genre analysis and as an approach to discourse analysis generally are highlighted.

The popularization of science

The main emphasis of this thesis has been on the development of a new model of the popularization of science and, therefore, it is appropriate that we should examine the implications of this model for the communication of science. The standard diffusion/distortion model, it has been shown, lacks any justification in terms of the actual practice of popular science journalism. Reporters on mass market news magazines are not trying in their writing to spread the science of the research article to a wider audience. As we have seen, the writers of research articles and the writers of news magazine popularizations have different aims and popularization is not simply a matter of research article simplified for a wider audience.

Recognition of the fact that news magazines are not in the business of reporting scientific facts wholesale should lead to a cessation of attacks on news journalists for not doing something they are not attempting to do. Once it is clear that news magazines are concerned with articulating the concerns of the society vis-à-vis new research this important task can be evaluated in its own right. Given the part that science and scientific research plays in modern developed societies it is important that the wider public be aware of developments in this research. To this extent the Public Understanding of Science advocates are correct. But the job of the journalist is to extract from the vast field of scientific research not what is important to the scientist, but what is important to the wider society. Sometimes the two coincide, as in the BSE scare, for example, where there was a huge impact on societies all over the world (Jasanoff, 1997; Wood, 1996), and the discoverer of the agent that causes diseases of this type was awarded the Nobel Prize. Dolly the cloned sheep is another recent example that springs to mind (Wilmut et al., 1997; see also Kahn, 1997), cloning being an area which is both scientifically very important and has potentially vast implications for society. On the other hand, sometimes what is very important scientifically has little immediate impact on society. An example here would be the discovery of Buckminsterfullerene, C60, which also earned its discoverers a Nobel Prize, but which has yet to impact on the public.

Thus scientific importance to the researcher and importance to society are not the same thing and the journalist must distinguish between the two. The realisation that popularization of science involves intersecting with society's interests should allow a more clear-headed debate on what such popularization involves. As it is, with popularization seen as the diffusion of scientific results to a populace not as interested in them as they should be by journalists who distort the science, much discussion of the subject misses the point. When popularization is seen in terms of the narrative of society model put forward here, then the model is more in line with the actual work of journalists. Discussion of what they do can then proceed on a more secure foundation.

The model put forward here is, of course, on the basis of a fairly small corpus. However, informal checking against other texts has confirmed that these also conform to the model. The fact that the corpus included every text on the topic in three different magazines over a one-year period would tend to indicate that the conclusions are valid, at least for the type of texts examined. This type of text is biomedical but since medical popularizations are seen by the public as the paradigmatic type of popular science (Durant et al., 1992) and they predominate in the news magazines of the type we are discussing, this is not a great drawback. The question naturally arises as to whether other, non-medical, popular news magazine texts follow the same generic structure. Informally, it would seem likely that news of a similar type to that discussed in this thesis, i.e. news which has an impact on the public, would follow the same pattern. Checking of this model against a wider corpus of non-medical texts would be a suitable follow-up to the work done here. As it stands, the model applies to biomedical texts, but since these texts are taken as paradigmatic examples of popular science, the model may apply more widely.

The addition of a narrative of society model to the narratives of science and narrative of nature put forward by Myers (1990) means that each type of text has its own way of describing science. Given the different purposes of the three types of text, RA's, specialist popularizations of the *New Scientist/Scientific American* type, and news magazine popularizations of the type investigated here, this is hardly surprising. Myers (personal communication) has suggested that there might very well be other narrative types. This could be the case, but it seems likely that because the type examined in this work is very common and because the model of popularization put forward has such general applicability, the narrative of society would be the standard default type in mass news magazine popularization.

The way forward from here, therefore, would be to test the model against a wider corpus of non-medical texts. More broadly, however, the use of a model of communication that is not code-based but more cognitively RT-based allows us to get away from the emphasis on whether the same information is conveyed in the original research and the popularization. Once we have left behind the worry that facts in the

original RA are not found in the popularization, then we are freer to look at popularization in terms of the process of communication, rather than the facts of what is communicated. This opens up the study of science communication so that it is much more amenable to analysis from a variety of standpoints. For the linguist this is very helpful, as we can now look at how communication is achieved rather than be stuck with a catalogue of factual omissions and errors. Science communication can therefore become the domain of the linguist as well as the sociologist and the journalism teacher, allowing us to throw new light on a field dominated for too long by rather unhelpful models of linguistic communication, to the extent that language was investigated at all.

In the longer term it may even be possible for the linguist to help scientists, philosophers and sociologists of science with one of their perennial problems: the clash between a realist and relativist view of science. As we saw in Chapter 4, current orthodoxy in the sociology of scientific knowledge leans towards a relativist view of scientific knowledge, whereas the practising scientist is almost always a fairly uncompromising realist (cf. the furore over the Science Wars, "Science wars", 1997; Sokal, 1996a, 1996b). If we look at the communication of science in an RT framework the problem begins to look more soluble. Since what is communicated is understood by the receiver in terms of his own cognitive environment, to that extent his understanding is relative. However, it does not, therefore, follow that the science communicated does not describe a real world. We are not committed to saying that because the understanding is relative what is described is not real.

Further discussion of this longstanding bone of contention would take us far beyond the topic of this thesis. It does show, though, that focus on science communication within the kind of framework put forward here could have interesting implications for more than the popularization of science. Attention to how science is communicated is very important not only to scientists but, because of the position of science in our society, to many outside the scientific research community. I have tried to show in this work that analysis of popularization of science from a standpoint which takes into account work in genre analysis and relevance theory can yield interesting insights into

how this communication is achieved. Science communication can thus become a more productive field of research for a broadly linguistic analysis. In addition it seems likely that such an approach if taken further might also shed some new light on other types of communication both within and beyond science.

Having examined the more specific aspects of the model proposed in terms of its being a model of the popularization of science and the effect this might have on the study of the communication of science, let us turn now to the wider implications for discourse analysis of the type of model put forward. We shall have a look in the following section at the implications of the approach taken here to future work in relevance theory.

Relevance theory

As was noted above, relevance theory is an extremely broad theory, claiming as it does to explain, if not *all* communication, at least the vast bulk of it and all communication of the type that a linguist might be interested in. If it is claimed to apply to the vast bulk of communication, then it would be expected that it be used for the analysis of a wide range of types of language and texts. As noted in the Postface to the second edition of '*Relevance*' (Sperber & Wilson, 1995), RT has been applied to such areas as literature and psychology. However, the bulk of the work has remained fairly narrowly focussed on such matters as the explicature/implicature distinction, metaphor and irony and discourse connectives, all fairly small scale matters of fairly narrow pragmatic concern. If RT were simply a narrowly pragmatic theory this would be acceptable, but in a theory which claims to describe practically all of communication, the fact that most work is of such narrow focus is rather worrying. In looking at RT the impression is of rather wild swings between very broad abstract generalities applying to all communication (e.g. Sperber & Wilson, 1986/1995) to very precise discussion of particular phrases which are not syntactically central (e.g. Blakemore, 1987).

This leaves the vast bulk of language use out of the equation completely. Although, of course, those working in pragmatics need to investigate the type of questions that, for example, Blakemore (1987), or Carston (1993), do, there is a need, I submit, to examine other broader areas of language as well. Most language use falls into the broad intermediate areas between discourse particles and overarching claims about the very nature of communication. Relevance theory needs to start examining some more typical intermediate areas of language use. This has been done in some areas, e.g. Gutt (1991), or Jucker (1993b), on translation and conversation, respectively, but even these two fields are vast in themselves. Investigation of particular types of use of language like the popular science magazine articles investigated here, would begin to pinpoint areas where existing claims could be tested and new hypotheses raised.

In particular, there is a need to examine written language as well as spoken. Whatever might be argued about the primacy of spoken language following Saussure (cf. Scinto, 1986), the fact remains that written genres pervade modern society and fulfil vital roles. It is important, therefore, that RT be used to examine not only casual conversation but written texts. Again a start has been made in this work, but there are many other different types of written text which it would be worthwhile to investigate besides literary works. Until extensive empirical work is carried out on written texts, the claim that RT applies to all communication remains a purely theoretical claim deduced from first principles rather than empirically grounded and solidly based on evidence. An analogy here would be work in generative grammar on the basis of English which produced universal claims which are said to apply to all human languages. These claims are then naturally tested against other languages, especially to languages that differ maximally in their syntactic structure from English.

Thus claims about human communication based on casual conversation need to be tested against other types of human communication which differ greatly from casual conversation, such as scientific texts. Work on as large a variety as possible of written texts of different types would help greatly in testing RT. Not only would it help test RT's general claims about the nature of communication, it might yield other insights and more specific claims about particular types of communication. Written

texts of a particular type have of course been examined in this thesis and it has been shown how they can shed interesting light on aspects of RT. The principal aim of the chapters on RT, however, has been to show that RT is compatible with the genre analysis model of popularization put forward here; it has not been to propose changes to RT itself.

RT could be more fully worked out, however, by looking in more detail at real discourse examples, rather than the inauthentic two or three line invented examples typical of most of the argument hitherto. Examination of such authentic discourse immediately throws up problems which helps cast light on aspects of RT. We have already seen this in Chapter 6 with the discussion of the unit of relevance. In addition examination of problems within a different framework, such as genre analysis here, provides an impetus to RT to explain aspects of discourse with which it may not have been faced hitherto. We have seen in the previous chapter how RT is able to explain in its own terms a unit like the move which has been posited in another framework. This adds strength to RT as a theory, since it is important for any theory not only that it is able to explain aspects of the world which are immediately accessible to that model, but that the theory explains other aspects which may seem at first glance not so amenable to investigation. I would argue that RT cannot simply ignore theoretical categories like genre which have grown up outside RT itself. What is much more productive is to do what has been suggested here and try to explain those categories in RT terms. It would seem likely, therefore, that examination of particular text types with an authentic corpus of the type used here would provide a way in which RT could be enriched as a theory and made more in line with the diversity of real-world communication.

Having seen how relevance theory might benefit from an examination of a wider array of authentic discourse types of the sort analysed here, let us turn our attention in the final section to another area we have examined in this thesis. In this section we shall turn the tables and examine how genre analysis might benefit from a more cognitive approach.

Genre analysis

As noted above in Chapter 2, genre analysis has been notably successful in the analysis of various text types, ranging from scientific research articles to business correspondence. It has been widely applied in the teaching of ESP and, in the Hallidayan variety, is used throughout the Australian educational system in the teaching of English. Given this success, then, one might argue that there is no need to tamper with the approach. Nevertheless, despite its strengths, genre analysis as currently constituted does suffer from several weaknesses. A genre is defined by Swales (1990) and Bhatia (1993) in terms of communicative purpose of the writer. Although this would seem to indicate a concern with a cognitive process approach, there has been little attention paid hitherto to how an example of a genre is understood by the reader. Analysis of the moves of the genre and discussion of how these moves are marked are certainly necessary and valuable. But they do not suffice as a theory.

The reason for this is that if we are concerned with the communicative purpose of a genre we are *ipso facto* concerned with how such communication is achieved. This implies a theory of communication, which up to now has been lacking. To the extent that any overall theory of communication is envisaged, we are implicitly faced with the standard code model of communication. But a theory of text using purpose as a main criterion would function much more effectively with a theory of communication which could examine precisely what the communicative purpose of a text was. RT is able to do so because of its definition of communication in terms of relevance.

Another advantage of RT in the analysis of genres, as we saw above, is its ability to characterise the various moves more precisely. These can be defined in terms of the implicatures involved. Although only a start has been made in this direction in the present work, it would seem a useful approach which is generalisable to other genres as well.

Moreover, RT is able to offer very precise methods of discourse analysis which could be of great use in genre analysis. Specification of how an explicature is attained in terms of

assigning reference, enrichment and disambiguation, as well as the concept of a full proposition which can be assigned a truth status, can lead to more exact analysis of discourse. When we look at a text in such terms, the extent to which many texts are not fully explicit becomes very clear, and this gain in clarity of analysis is of great benefit to the discourse analyst.

An instance of how this works in practice will demonstrate how helpful this can be. If we take as an example our familiar text, Text 10, *Cornering a killer*, we can see how, from an RT point of view, this headline is not in fact a full proposition that can be evaluated as true or not. It must be enriched and disambiguated before any full understanding of the headline can take place. Thus who is doing the cornering? There is no subject to the verb, and no tense either, since the verb is non-finite. The killer itself has no referent. Hence it is impossible to assign any truth condition to the headline as it stands. It is only when the subheading below the main headline is read that any sense can be made of the main headline. Reading this, we can infer that the killer is a gene that causes some breast cancer and the reference to researchers indicates who may have done the cornering. But the process of disambiguation that is gone through when the reader identifies the killer as the breast cancer gene helps to intensify the effect of the phrase as the reader will have to separate out the real referent from other potential referents but in doing so is reminded of the seriousness of the effect. Similarly, the search for the missing subject for the verb is likely to activate subconsciously other people who might corner a killer, like policemen or soldiers, and thus increase the status of the scientists concerned.

This short analysis of just one headline gives some indication of how RT can be used to make analysis of texts more powerful and subtle. Similar analysis can be applied throughout a text and the extent of the ambiguities and subtleties inherent in the popularizations become evident. This has not been done in this work as the focus was not on such analysis, but it can be very useful in demonstrating how a text achieves its effects.

More generally, an RT approach to genre analysis can help guard against a purely formal approach to genres, looking just at the moves and their exponents, as criticised by Fairclough (1995a, 1995b). This more cognitive approach can help the analyst focus on how the genres have their effect in achieving communication. In particular, it can help to bridge the gap often somewhat artificially set up between product and process approaches, making genre analysis more concerned with the process of how communication is achieved rather than just the end product, the genre.

An emphasis on just how communication is achieved via a more cognitive RT analysis could thus aid in shifting genre analysis forward from a stress on simply cataloguing genres and moves. It is possible, by combining a strict move analysis of the type done in this thesis, and an RT approach, to gain the benefits of the precision of the emphasis on moves with the greater depth and subtlety of RT in its examination of just how communication is achieved. RT can be applied to genres that have already been analysed within a genre analysis framework, to help define how the moves have their affect. It can also be applied to putative new genres to see whether the application of RT could help in defining the genre.

One area in particular where the combination of genre analysis and RT might bear fruit is in the investigation of mixed genres. Following Fairclough (1992), genre analysts such as Bhatia (1997) have begun to apply the idea of mixed genres. However, given the definition of genre in terms of communicative purpose, it will be a problematic issue to define when a mixture of genres exists and when a new genre can be said to exist. Thus although Fairclough (1992, 1993) shows convincingly that academic descriptions of university courses and degrees have been colonised by market language, the question still arises as to how to characterize such types of writing. More flexible methods than traditional genre analysis may help define more clearly when a new genre has come into existence.

The approach to genre analysis outlined here, therefore, shows a way forward which might help to bring closer together the diverse approaches to genre analysis described in Chapter 2 above. That the traditional Swalesian approach to genre analysis, despite

its impressive achievements and influence (cf. Swales, 1990, p.140), might be in need of a new impetus, is indicated by a number of factors. Thus Swales himself has indicated that his earlier approach may have been too product-oriented and that a more process approach may be needed (Swales, 1996). Similarly, Bhatia's recent turn towards an emphasis on genre-mixtures (Bhatia, 1997), can be seen as the result of a model which is rather too vague in its definition of communicative purpose. The model proposed here combines a specific precise notion of move and genre with a method of analysing genre which helps to show how communication is achieved. Thus it combines the strengths of both the product and process approaches to genre analysis.

In addition, the model provides the social dimension of genre which is demanded by the proponents of New Rhetoric and is assumed by the Hallidayan approach. The relation of the genre to the audience gives this social dimension, also argued for by the proponents of critical discourse analysis. That approaches to pragmatics tend to fall into a speaker-oriented social approach or a hearer-oriented cognitive approach has been emphasised by Thomas (1995) and is a valid critique. One way round this dichotomy is provided by Thomas's interactive model (Thomas, 1995) but this is applicable primarily to spoken discourse. My model attempts to integrate the social and the cognitive by a different route. It recognises the social importance of the audience for the text but looks at the process of communication from a cognitive point of view.

The model proposed here then has elements of both product and process, social and cognitive, models. In trying to integrate these opposed viewpoints it points the way towards a more general theory of discourse which might go beyond the dichotomies traditionally set up in such a manner. That discourse analysis can be seen from each of these angles is clear, but that is what they are, views from different angles. A text is at one and the same time a finished product there on the page and something which is interpreted in different ways by different readers. Discourse is both social in origin, in the sense that it is constituted by wider social forces, and at the same time cognitive, as the processes of production and interpretation are irreducibly cognitive

in nature. It is possible, of course, to emphasise one aspect or other of these dichotomies, but models of discourse analysis which see discourse as purely product or purely process, simply social or simply cognitive, are no more than a part of the picture. Discourse cannot be reduced to something which is either purely social or purely cognitive. The model of discourse suggested in this work is an attempt to widen the focus and examine what a more integrated model might look like.

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