Surveying Social Life

Papers in Honor of Herbert H. Hyman

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FIRST EDITION

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20 Dietary Cholesterol and Heart Disease: The Construction of a Medical "Fact"

The mass media play a critical role in transmitting health-risk information from knowledge producers to consumers. But do those reporting on health risks present an accurate picture of the state of scientific knowledge on these risks? More specifically, if there are biases and distortions of scientific information, what is the character of these problems in reporting? And what properties of the institutions of science and the mass media help us understand the types and sources of bias and distortion? To answer such questions we would need an extensive data base of health-risk studies reported in the various media.[1] Such a set of data does not exist. Here I will present material on one case—the reporting of health risks purported to be associated with dietary cholesterol. The basic analytic problem of this paper can be stated quite simply: How is a questionable claim to truth, or medical "fact," transformed into an unquestionable one? What role do health scientists and the mass media play in this process?[2]

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[1] For a discussion of the distinction between the frontiers of research and the "core" of knowledge, see Cole and Cole 1973; Cole 1983.

[2] The paper represents one effort in a larger program of research focusing on the sociology of risk assessment. I have examined material for other cases, and although all of the cases examined to date lead to similar conclusions, we must be cautious about

The focus is on reporting by scientists and reporters of recent studies on the relationship between cholesterol and coronary heart disease. The first section describes the recent results from a major, long-term study of cholesterol and notes what types of information were not transferred from the medical research community to the public via the media. The second section examines the recent history of the controversy over the health risks associated with dietary cholesterol and describes the development of a "scientific fact" that cholesterol intake is a major cause of heart disease and of the normative conclusion that it should be uniformly minimized in American diets. A final section discusses some possible sources of explanation for the distortion of health risks in the media.

The Cholesterol Case

In January 1984 the *Journal of the American Medical Association* (*JAMA* 251, no. 3) published the results of a recently completed ten-year, $150 million study of the link between lowering cholesterol levels and a reduction in heart attack deaths. Prior to this publication, several scientists from the Lipid Research Clinics (LRC) team who conducted the study on 3,806 subjects gave the media an extensive briefing on their research methods and the study's results (Brensike et al. 1982, 1984; Levy et al. 1984; Lipid Research Clinics Program, 1984*a*, 1984*b*).

Almost all the major newspapers and magazines in the United States carried reports on the scientists' findings, and the principal investigators were interviewed extensively on radio and television. The *Los Angeles Times* ran a typical headline: "Cholesterol Decisively Linked to Heart Attacks." The *Times* reported: "The average participant who received the [cholesterol-reducing] drug for between seven and 10 years had 19% lower risk of having a heart attack and was 24% less apt to die of a heart attack than those who received only the placebo."

Plainly a breakthrough for medical science. Now doctors could recommend reducing cholesterol intake and feel confident that this would reduce risks to their patients. And those of us who rarely see a physician could make our own decisions about cutting down on eggs and beef. But we should consider first what major newspapers and network television news programs failed to discuss about this major medical study.

To this end, I took the 1984 *JAMA* publications of the lipid research group's major findings as a point of departure. I examined all news

generalizations from a few cases before more extensive work on larger samples is completed.

stories, columns, editorials, and opinion pieces appearing in the *New York Times*, the *Wall Street Journal*, the *Washington Post*, *Newsweek*, *Time*, and a less systematically collected set of additional reports by the wire services and television news programs.[3] I also traced back to 1980 the news treatment of the relationship between cholesterol and coronary heart disease and death, and traced it forward since January 1984. Portions of the scientific and scholarly literature (particularly the clinical trial and epidemiological research) and assessments of the risks of dietary cholesterol by the National Research Council, the research arm of the National Academy of Sciences, were examined.[4]

Here, then, are seven features of the research that were given little or no space in the press reports following the 1984 Lipid Research Clinics' publications:

1. Because the media reported the data as "% reduction in risk," the reader was not made aware of the difference in the number of coronary heart deaths (CHD) in the two groups studied. The control group's 1,900 members were matched with the 1,906 in the experimental group for a variety of factors but were not given cholestyramine, a cholesterol-reducing drug given to the experimental group. Over the ten-year period of the study, there were thirty-eight CHDs in the control group and thirty in the experimental group. In short, there was a difference of eight deaths in an experiment involving 3,806 people, or a death rate of 2 percent in the first group and 1.6 percent in the second. Of course, if we take the difference between the two—that is, 0.4 of 1 percent—and state it as a ratio of .4 to 1.6, we obtain, as was reported in the scientists' abstract, a 24 percent reduction in risk. Thus, a major conclusion of the study on the harmful nature of cholesterol rests on this 0.4 of 1 percent.[5]

2. Although the scientists reported this difference to be significant at the .05 level for a one-sided test of their hypothesis, they neglected to report in the *JAMA* article that chi-square and other tests of significance do not show any significant difference between the experimental and control groups.

[3] In all, over 100 cholesterol news stories were reviewed between 1980 and 1984. Twenty-two were located in the *Washington Post*, forty-six in the *New York Times*, ten in the *Wall Street Journal*; nineteen were from CBS Morning and Evening News, five from ABC News, and more than twenty-five from various news magazines and news services. These stories varied, of course, in length, depth, and type—from straight news reports to opinion columns. References to "the media" and "the press" throughout this paper are restricted to these data sources.

[4] David Gerwin, an undergraduate student at Columbia College, helped collect these materials.

[5] Epidemiologists are quick to point out that very small fractions can represent large numbers of cases when extrapolated to the entire population. But this is so, of course, only if the small differences are to be treated as a real "signal" rather than "noise."

3. Most reportage of the study failed to state that the overall mortality rate in the two groups was not significantly different.[6] In short, the control group members were no less likely than those in the experimental group to live through the ten-year period (3.7 versus 3.6 percent), a fact reported in the *JAMA* article but rarely picked up by the press.

4. Virtually no news stories examined the gastrointestinal side effects of the treatment, which, though ostensibly not severe, did occur more often in the experimental group. Indeed, the data on gallbladder disease (one potential consequence from using lipid-reducing drugs) show insignificant effects, but if we apply the method of ratio of increased risk adopted by the scientists in presenting the data on coronary disease, it appears that the cholestyramine group had an increased risk of 46.1 percent over the placebo group of having "operations involving the gallbladder" (36 versus 25) (Lipid Research Clinics Program 1984*a*, p. 357).

5. Although the *JAMA* article gave plenty of information about the criteria used to select subjects for the study, the press gave little or none. For instance, few reports cited the fact that the study concentrated on those in the top 5 percent of the population in terms of their cholesterol level—hardly a representative sample of the United States population or of those apt to read about the results.

6. There was no discussion in more than 100 news stories and analysis columns or in the *JAMA* papers of varying results among the score of lipid research clinics. In fact, there was substantial variability, making the pooling of the data for all of the clinics a somewhat questionable method of analysis.

7. The news and health column stories did not deal with a set of other questions that, although not considered in the *JAMA* article, are relevant to readers attempting to make decisions about their cholesterol intake. For example, what is the relationship between dietary intake of cholesterol, on the one hand, and cholesterol plasma levels on the other? What scientific problems are there in extrapolating from dietary intake of cholesterol and cholesterol plasma levels? How do the rates of death within specific age groups from all causes in this study differ from those in the population of American men during the same period of time? How do women, children, blacks, and whites react to the treatment?

Not having any training in medicine, I am not suggesting, of course,

[6]This conclusion, as well as others drawn throughout this paper, is limited to the journals and media that I examined, which were noted above (n. 3). Thus, when I speak of the media with regard to the cholesterol case, I am limiting myself to these sources in the media.

that any given level of cholesterol consumption is appropriate for all or any of us. I am saying that the conclusions about the risks of cholesterol intake were raised by some to a level of scientific certainty ("definitive," according to the principal investigators and quoted in the press) prematurely. And I am suggesting that this type of distortion might lead in turn to significant changes in the public's dietary habits—especially after it has been bombarded with "news" of the risks and newly spawned advertising campaigns about the dangers of cholesterol.

From my limited evidence, it appears that reporters of health risks working for leading newspapers, newsmagazines, and television network news programs have difficulty in incorporating scientific disagreement and uncertainty into their stories. In fact, among leading experts in the field of cholesterol research, there is substantial scientific disagreement about the value of changing dietary habits and about cholesterol-reducing drugs as means of influencing rates of coronary heart disease. None of the alternative views from the one calling for dramatic dietary change was given prominent airing in the news articles following the 1984 LRC publications.

Take the views of Edward H. Ahrens, Jr., Frederick Henry Leonhardt Professor of Rockefeller University, who has been working in the "murky business of lipids" since the 1940s, as an example. He has studied how cholesterol metabolism is regulated in different types of people. Cholesterol is necessary of course in normal body functioning: It is a good thing. The question is, Do we have "too much of a good thing"? As Ahrens puts it:

> Blood levels of cholesterol are useful indicators of people at risk . . . but the important question to ask about such a person, once identified, is *why* the level is elevated; is the body making or absorbing too much cholesterol or not excreting enough? Most treatment is aimed simply at lowering blood lipid levels, which is not necessarily beneficial. [Rockefeller University Research Profiles 1984, p. 4]

Considering the ten-year cholesterol study from this angle of vision, Ahrens makes the point that it is "not scientifically sound to extrapolate drug results to advice on diet for people in whom synthesis-absorption feedback regulation works efficiently" (p. 6). In a recent paper in the *Lancet*, Ahrens questions some overly broad extrapolations from the results of clinical trial and epidemiological studies of cholesterol. The past evidence appears, in fact, weak:

> Over the past 25 years this hypothesis [that CHD rates will drop if plasma cholesterol levels are reduced] has been put to the test in more than 20 trials which attempted to lower plasma cholesterol levels by dietary manipulations

or by the administration of plasma-cholesterol-lowering drugs. Only the Lipid Research Clinic's coronary prevention trial (LRC–CPPT) . . . produced evidence for benefit that was any more than suggestive. [Ahrens 1985, p. 1085]

Extrapolations by scientists from limitedly supported hypotheses to proof is disquieting to Ahrens, not only because these extrapolations can mislead reporters who include them in their stories but also because major agencies in the scientific community, for example, the National Institutes for Health (NIH), are reinforcing these leaps. Ahrens makes several additional points.

He questions whether the results from research on cholesterol can be applied to all subgroups in the population.

I cannot accept the recommendation that the prudent diet be adopted by everyone over the age of 2 years. That viewpoint was rationalized on two grounds: (1) the generalization by the CPPT authors that any 1% reduction in plasma cholesterol level will lead to a 2% reduction in CHD incidence in all segments of the population; and (2) the public health view that dietary interventions are more feasible if adopted by a whole family than singly by a high-risk member of a family. . . . I know of no evidence that the prudent diet will prevent the development of arterial atheroma at any age: the hypothesis is reasonable but not proven. [Ahrens 1985, p. 1086]

Perhaps most tellingly, Ahrens is also worried

that drastic changes in fat consumption, both in quality and quantity, which have been observed to change cell-membrane structure, may have undesirable effects on an individual's immunologic responsiveness and susceptibility to other diseases. [Rockefeller University Research Profiles 1984, p. 6]

He would prefer the health community to focus its attention on

the twenty percent or so of the population at the top end of the blood cholesterol level scale who need vigorous, *individualized* testing and treatment. It is not a popular theme song these days . . . but more and more experts are learning the tune. [P. 6]

This cautionary position, which would limit extensive treatment to the people at "real" risk, is shared by Michael S. Brown and Joseph L. Goldstein, who recently received the Nobel prize for their work on LDL (low-density lipoprotein) receptors. These receptors bind particles that carry cholesterol and remove them from the body's circulatory system. Brown and Goldstein take a far more cautious stance toward sharp dietary changes than do the Lipid Research Clinics researchers:

If the LDL-receptor hypothesis is correct, the human receptor system is designed to function in the presence of an exceedingly low LDL level. The kind of diet necessary to maintain such a level would be markedly different from

the customary diet in Western industrial countries (and much more stringent than moderate low-cholesterol diets of the kind recommended by the American Heart Association). It would call for the total elimination of dairy products as well as eggs, and for a severely limited intake of meat and other sources of saturated fats. [Brown and Goldstein 1984, p. 65]

Then, the authors make the critical point about the consequences of drastic changes in our eating habits:

We believe such an extreme dietary change is not warranted for the entire population. There are several reasons. First, such a radical change in diet would have severe economic and social consequences. Second, it might well expose the population to other diseases now prevented by a moderate intake of fats. Third, experience shows most Americans will not adhere voluntarily to an extreme low-fat diet. Fourth, and most compelling, people vary genetically. Among those who consume the current high-fat diet of Western industrial societies, only 50 percent will die of atherosclerosis; the other 50 percent are resistant to the disease. Some individuals resist atherosclerosis because their LDL level does not rise dangerously even though they consume a high-fat diet. [Pp. 65–66]

Elsewhere, Goldstein and Brown specifically note that the positive benefits of lowering blood cholesterol remain unproven.

In some subjects with high serum cholesterol levels . . . , atherosclerosis to some degree always develops with time. In other people, cholesterol accumulates even when their blood cholesterol level is within the normal range; their arteries seem to be sensitized by unknown factors. It can be argued that such individuals would benefit from a lowering of their blood cholesterol levels, *but that has not yet been demonstrated scientifically.* [Goldstein and Brown 1985, p. 46; emphasis added]

Putting aside the questions of economic and social cost–benefit analysis associated with dietary change, the fact is there exists substantial scientific disagreement about its consequences among highly reputable scientists. Ahrens, as well as Brown and Goldstein, views the scientific problems and related policy recommendation for cholesterol intake in different terms from the authors of the ten-year cholesterol study. The former group allows that dietary recommendations may be in order, but they emphasize that these should be highly individualized, dependent upon detailed knowledge of individual histories and other risk factors. The lipids research group was far more willing to generalize its results to the entire U.S. population, suggesting in several interviews that significant dietary changes should begin for all children beyond the age of two. The central point here is not who is right or wrong but that the reportage neglected these alternative perspectives and failed to give the reader any idea that there

might be disagreement within the scientific community over the meaning and implications of the results of the ten-year study.

Today we find almost no discussion in the news media of the scientific disagreement over the hypothesized relationships between cholesterol intake, plasma lowering of cholesterol, and atherosclerosis. The harmful effects of cholesterol in the diet are not only taken as a given but, based largely on the Lipid Research Clinics findings, are given the following felicitous functional form: For every 1 percent reduction in cholesterol there is a 2 percent reduction in the risk of coronary heart deaths. This has become a scientific "fact"—at least in news stories and probably in the minds of many concerned Americans who attend to this news. Yet, as I have suggested, such consensus simply does not yet exist in the scientific community. In the following section, I take up the question of how scientific disagreement about the effects of dietary intake of cholesterol was transformed in the press into apparent consensus, how it became a "fact."

From Possibility to Probability to Fact: The Cholesterol Case in Detail

Elevating important but inconclusive scientific results to the level of "definitive fact" is often facilitated by certification through institutional authority. For example, the National Institutes of Health recently have formed "consensus committees," on which a panel of "experts" sit and "weigh" evidence, attempt to reach a "consensus," and publish a set of conclusions and recommendations. Such a committee was formed to review cholesterol data.

Thorny problems can arise in selecting members of consensus committees, and these choices can affect what the media receive as "best evidence." The cholesterol consensus conference is a good example of this problem. Among the members of the planning committee who set up the NIH consensus panel were Dr. Basil M. Rifkind, the associate deputy director for atherogenesis lipid metabolism, Atherogenesis Branch of the NIH's National Heart, Lung, and Blood Institute. And it included Dr. Kenneth Lippel from the same NIH unit, as well as Dr. Charles Gluck of the Lipid Research Clinic in Cincinnati. Each of these three is undoubtedly an expert in this field. But they also happen to have been deeply involved in the ten-year cholesterol study. In the end, the essential question is, Were we really obtaining "peer review" from the most knowledgeable experts on cholesterol research?

If this consensus conference was composed of unbiased experts, it surely did not appear so to M. F. Oliver of the Cardiovascular Research Unit of the University of Edinburgh, who concluded:

> Clearly, the aims of both the consensus development conferences were to develop a consensus view and, not surprisingly, the final statements prepared at the end of each 2½ day meeting were biased. How could they have been otherwise? Those who initiated the idea were either naive or determined to use the forum for special pleading, or both. The panel of jurists . . . was selected to include experts who would, predictably, say that . . . all levels of blood cholesterol in the United States are too high and should be lowered. [Oliver 1985, p. 1088]

Regardless of their makeup, when NIH consensus panels talk, the media listen. And the *New York Times* on December 13, 1984, ran a front-page story on the conclusions reached. The National Institutes of Health, as a leading scientific authority, are now supporting the "fact" that there is a direct causal link between cholesterol and CHD. And few caveats enter into the news story. A paragraph in the *Times* story states: "The most recent study showed that reducing cholesterol levels in the blood could prevent deaths from heart disease, with every 1 percent reduction in cholesterol lowering the coronary risk by 2 percent." No mention any longer of the subpopulation on which this questionable result is based; no discussion any longer of the statistical or other problems we have already noted. This ratio of 1 : 2 is gaining "cognitive independence" from the original study.

The progression of scientific evidence from the status of possibility to probability to fact occurs, I suggest, with some frequency in the media. This would hardly be significant if the progression were coupled with notable improvement in the quality of scientific evidence, but, in the case we are dealing with, it is not. By tracing briefly the historical journey of the standing of cholesterol vis-à-vis coronary heart disease, we can develop a picture of how the values and cultural ideology of scientists, scientific organizations, and members of the media influence the public's perception of scientific "facts."

An abbreviated sketch of the 1980 to 1985 media presentation of the link between cholesterol and coronary heart deaths, triggered by new epidemiological and clinical trial studies, will demonstrate how the construction of this fact is accomplished.

Phase One: Possibility

Toward Healthful Diets—In 1980 the National Research Council (NRC) published a report of the Food and Nutrition Board, *Toward Healthful Diets* (National Academy of Sciences 1980). The board evaluated existing scientific evidence on aspects of diet and obesity, cardiovascular disease, hypertension, cancer, and diabetes mellitus. The board took a cautious position on the link between cholesterol and atherosclerosis.

The causes of atherosclerosis are unknown. . . . A number of risk factors for cardiovascular disease have been identified from epidemiological studies. . . . Risk factors are those factors found to be statistically associated with an increased incidence of disease. They cannot, without independent evidence, be considered to be causative agents of the disease. . . . Diet modification as recommended for the prevention of atherosclerosis is based upon the assumption, not yet adequately tested, that reduction of high serum cholesterol levels . . . will reduce the probability of cardiovascular disease. [Pp. 8–9]

Now consider a truncated presentation of the critical reception of this work by the media.

1. May 28, 1980: Jane E. Brody of the *New York Times* writes a front-page story with the following lead: "In a sharp departure from recent dietary recommendations, the Food and Nutrition Board of the National Research Council said yesterday that it found no reason for the average healthy American to restrict consumption of cholesterol. . . . For human beings . . . the link between cholesterol and fat in the diet and heart disease is largely circumstantial."

2. On the same day, however, Lawrence K. Altman of the *Times* writes an article on an inside page entitled: "Report about Cholesterol Draws Agreement and Dissent." He presents a scenario of disagreement within the scientific community. There is the spokesman for the American Heart Association: "We stand firmly behind our dietary evidence to the American public—eating a maximum of 300 milligrams of cholesterol and 30 to 35 percent of calories from fat . . . These recommendations represent the work of hundreds of experts who have sifted carefully through the available scientific evidence." With a contrasting view is Dr. Norman Spitz, professor at the NYU Medical School and "a recognized expert on nutrition": "it turns out that our bodies manufacture much of our cholesterol and that the effect of diet is 'relatively small.' . . . Clinical studies that tried to lower cholesterol directly by dietary and drug means were not successful in lowering the death rates."

3. On May 29, we have Susan Okie, writing in the *Washington Post:* "A scientific panel's finding that healthy Americans need not lower the amount of fat and cholesterol in their diet was welcomed by milk, meat and egg producers yesterday but caused some chagrin at the American Heart Association." Later she quotes the rising chorus of dissenters: "Dr. William Kannel . . . head of the Framingham study that first produced evidence on the relation of life-style to heart disease, said the latest report was inconsistent in its recommendations. . . . 'It makes me wonder . . . at their objectivity.'" Michael Jacobson, director of the Center for Science in the Public Interest is quoted: "It stinks. It reads as if it was written by the meat, dairy, and egg industries."

4. May 29, 1980: Jane Brody of the *New York Times* writes a story headlined "Dispute on Americans' Diets." The story details the scientific disagreement on the effects of cholesterol.

5. June 1, 1980: the *New York Times* has this front-page headline, "Experts Assail Report Declaring Curb on Cholesterol Isn't Needed." The counterattack on the National Academy of Sciences (NAS) begins in earnest. The following points are made. (*a*) *The NAS is attacked for its lack of representatives on the board of different perspectives.* Robert Levy, director of the National Heart, Lung, and Blood Institute of the NIH is quoted: "It's true that not all the facts are in. . . . But to recommend doing nothing in the meantime is inappropriate . . . Americans should hedge their bets and seek a diet lower in saturated fats and cholesterol, at least until more evidence is available." (*b*) *The Nutrition Board members had ties to the food industry.* (*c*) *The board's recommendations were inconsistent.*

6. June 1, 1980: the *New York Times'* "Week in Review" runs a story, "Sunny Side Up for Cholesterol," in which it briefly summarizes the NAS report.

7. On the same day, Jane Brody, under the headline, "When Scientists Disagree, Cholesterol Is in Fat City," notes:

Lacking ironclad proof that changing one's diet can prevent heart disease in otherwise healthy persons, the board recommended no restrictions in cholesterol intake. . . . the board's advice is contrary to that offered by at least 18 organizations concerned with nutrition and health.

Thus far, the presentation of information on cholesterol reflects the existing disagreement within the scientific and medical community. There is a possibility that large cholesterol intake is harmful, but it is neither proven nor beyond scientific dispute. But it is also plain that there is a growing interest in the press, at least in some quarters, in debunking the NAS findings altogether. For example, on Monday, June 2, 1980, a *Washington Post* editorial entitled "Cholesterol Does Count" states: "'Toward Healthful Diets' . . . not only has increased public confusion over proper diet. It has also soiled the reputation both of the board and of the academy for rendering careful scientific advice" (p. A-18). The *Post* seems to be asking that a subject that, *in fact,* generates real scientific disagreement and some confusion be simplified and made easily digestible for the American public, even if the reports are not true to the controversy.

The *New York Times* follows with an editorial on June 3, entitled "A Confusing Diet of Fact." It admonishes the academy:

The National Academy of Sciences is supposed to be an authoritative, impartial source of scientific advice to both the public and government—a Supreme

Court of Science. But its latest report on health diets is so one-sided that it makes a dubious guide to nutritional policies. . . . The Academy should be espousing more than a single view.

And in fact subsequent articles do reflect existing disagreement. Consider just a few additional headlines: "A Few Kind Words for Cholesterol" (*Time*, June 9, 1980, p. 51); "How Bad Is Cholesterol?" (*Newsweek*, June 9, 1980, p. 111), which presents the conflicting opinions of various authorities; "Lower Fat Diet Affirmed Despite Recent Findings" (*Washington Post*, June 10), which is based upon testimony at the National Academy by Dr. William Castelli, "medical director of the famed Framingham, Mass. study of life-style and disease." On June 11, 1980, Mimi Sheraton, a food and dining specialist for the *Times*, writes under the headline, "Conflicting Nutrition Advice Bewilders U.S. Consumers." A variety of points of view are expressed, including Dr. Michael De Bakey's: "A good 60 percent of the people who have arteriosclerosis do not have elevated serum cholesterol levels . . . why then are we basing our assumptions on the 40 percent minority?"

The *Washington Post*, June 12, 1980, headline for a story by Victor Cohn: "Panel Quits to Protest Advice on Cholesterol." The *New York Times*, June 18, 1980, headline for a column by Jane E. Brody: "Hidden Fat: The Hazards." The *New York Times*, June 20, 1980, "Scientists Clash on Academy's Cholesterol Advice," reports that "scientists . . . agreed that there was no scientific correlation between lowering of cholesterol and a reduction in coronary disease, they disagreed on whether Americans should have dietary advice on cholesterol." Suffice it to say that in June of 1980 the public was presented with a spate of news stories emphasizing the scientific disagreement over the effects of cholesterol.

Several points emerge from the plethora of stories. The NRC–NAS report's findings questioned the "received wisdom" about cholesterol and heart disease. Consequently, the media present a sense of a lively, and often confusing, debate over what constitutes scientific fact on this issue. They explicitly call for the presentation of diverse points of view and take the academy to task for its supposedly unbalanced presentation of this scientific conflict. But, as we shall see, when subsequent scientific studies present findings that appear to support the prevailing received wisdom, the press fails to present any similar discussion of conflicting points of view. And the fundamental point is that no critical experiments or results of scientific work were done in the intervening years to warrant a shift in position from a possible to a probable linkage between cholesterol and coronary heart deaths.

The Framingham Study—The controversial academy report was not the only cholesterol and atherosclerosis news in 1980. In that year,

the results of the Framingham study, a massive twenty-four-year epidemiological effort "to determine the risk factors for coronary heart disease and other atherosclerotic disorders" (the results of which had been published in more than 150 scientific papers between 1950 and 1978), were pulled together and summarized in a book by Thomas Royle Dawber (1980), one of the study's principal investigators. The Framingham study followed 5,127 men and women between ages thirty and fifty-nine. Initial examinations of subjects began in the spring of 1950. Individuals who were free of coronary heart disease at the initial physical examination constituted the population to be followed (pp. 20–21).[7]

On the effects of blood cholesterol levels, Dawber states: "Observations from the Framingham study over 24 years clearly indicate that serum cholesterol plays a role in the incidence of coronary heart disease. . . . The average annual rate during the entire 24 years for men with cholesterol levels 260 mg % or more was twice that of those with levels below 200" (1980, pp. 129–30).

Now consider these results in greater detail. For women, the findings are wholly inconclusive. "Analysis of the 24-year risk in women . . . shows [that], in both the youngest and oldest decades examined, no significant differences in incidence of coronary heart disease on the basis of cholesterol levels were observed" (Dawber 1980, p. 130).[8] Even in the middle-aged group there is no difference in incidence of coronary heart disease for every cholesterol category except for the highest cholesterol group, representing a relatively small proportion of the population (see Dawber 1980, fig. 8.4, p. 133). For most of the population of women there is no relation between cholesterol and heart disease.

Even for men the relationship is tenuous. Of course, men in all age groups are far more apt than women to develop heart disease. But for men with cholesterol levels in the lowest group, the average annual rate of coronary heart disease was 7.4 per 1,000; 8.4 in the next group; 12.7 in the midgroup; 10.1 in the next-to-highest; and 14.6 in the highest. The incidence of heart disease is about double between the extremes, but the relationship is less marked when comparing the other groups—for example 1.7 persons per thousand between the second-lowest and next-to-highest cholesterol groups. These rates are simple bivariate distributions. Multivariate analysis describing the independent effect of cholesterol on coronary heart disease and mortality by cholesterol levels is not presented. Dawber never indicates the

[7] The various methodological problems of the study, such as dropout rates and representativeness of the sample, require more elaborate discussion that can be found in Cole 1986.

[8] The book omits discussion of mortality rates and discusses morbidity only.

percentage of variance explained by dietary cholesterol. He does suggest that diet had little to do with coronary heart disease in the Framingham population:

> The expectation that the intrapopulation differences in cholesterol level could be attributed to individual dietary habits has not been fulfilled. The variability of blood cholesterol values within the Framingham population appears to reflect inherent or constitutional traits rather than differences in life habits. . . . Differences in dietary intake that could affect the blood cholesterol level did not account for the intrapopulation differences nor for the effect of cholesterol level on the *relative* risk within this population. [Dawber 1980, pp. 138–39; emphasis in original]

The relatively small differences in risk associated with varying cholesterol levels in men; the virtual absence of any relationship among women; and that differences in cholesterol were unrelated to diet have been almost totally ignored in the media's presentation of the Framingham study. The message of Framingham has been that the intake of cholesterol in diet produces significantly increased risk of heart disease and death.

Phase II: From Possibility to Probability, 1981–1984

Between 1981 and the Lipid Research Clinics trial results in 1984, the media transform the possible linkage into a highly probable one. This progression is again associated with the publication of results of scientific research.

The Western Electric Study—In January 1981, *The New England Journal of Medicine* publishes a paper, "Diet, Serum Cholesterol, and Death from Coronary Heart Disease: The Western Electric Study." Based on a twenty-year follow-up physical examination of 1,900 middle-aged men, this study investigated "the associations of dietary saturated fatty acids, polyunsaturated fatty acids, and cholesterol with serum cholesterol level and risk of death from coronary heart disease (CHD)" (Shekelle et al. 1981, pp. 65–70). There are design problems with this study. For example, there was only one follow-up over a twenty-year period and no monitoring of changing patterns of health, diet, and life-style variables in the intervening years. Women, children, members of varying occupations and social classes are not studied at all, or are underrepresented. But more importantly, the percentage difference in coronary heart deaths between the lowest and highest third of the 1,900 men in terms of saturated fats was 10.9 versus 11.8 percent and in terms of dietary cholesterol, 10.9 versus 13.6 percent. In short, these small substantive differences mean that cholesterol explained little variance in death rates, although the authors choose to interpret

the effects very generously indeed. "The correlations between dietary variables and serum cholesterol concentration in our study were small" (p. 69). But the media that I examined treat the results otherwise.

Time, January 19, 1981: "Cholesterol: The Stigma Is Back—New Report Reaffirms the Link to Heart Disease." The story states: "The main finding: those who had consumed large amounts of cholesterol and saturated fat suffered upwards of a third more deaths from heart disease than those who consumed relatively small amounts." Although this is a misleading summary, *Time* acknowledges that the debate over cholesterol will probably continue. A *Newsweek* article contends that the Western Electric study "demonstrated a specific link between individual eating habits and fatal heart attacks" (*Newsweek*, January 19, 1981, p. 74).

The "MRFIT" Study— In September 1982, the *Journal of the American Medical Association* (*JAMA*) published the results of a randomized primary prevention trial to examine the effects of changes in hypertension, cigarette smoking, and dietary advice for lowering blood cholesterol on mortality from coronary heart disease. Almost 13,000 high-risk men aged thirty-five to fifty-seven were chosen from 361,000 initially screened and randomly assigned to "special intervention" groups, who received counseling to alter existing habits, and a "usual care" group, which represented a control. To become a participant, a person had to be at "increased risk," that is, in the upper 15 percent of a risk distribution based upon cigarette smoking, serum cholesterol, and blood pressure (based upon data from the Framingham study). This Multiple Risk Factor Intervention Trial (MRFIT) encountered methodological problems, as had the previous major epidemiological and clinical trial studies. For one, the experimental and control groups differed little on a set of risk factors, even after attempted interventions. Members of both groups had lowered their smoking levels and their intake of cholesterol during the ten years of study.

The results were dramatic—but at variance with the wisdom being placed in print and on the air by the media. For example, the mortality rate from coronaries in the special intervention (SI) and usual care (UC) groups were almost identical, and the difference in overall mortality obtained went in the opposite direction from what had been predicted. The death rate was actually 2.1 percent higher in the special intervention group. The effects of serum cholesterol on CHD deaths were nonexistent or minimal. In short, cholesterol levels had no significant effect on the death rates, even considering statistical interactions among the three risk factors.

This was a major, $115 million, ten-year study reporting a "negative finding" on cholesterol. MRFIT did not go unnoticed by major news

organizations. As far back as 1980, there was keen interest in the potential of the MRFIT study.

Several months before the MRFIT results are published, Jane Brody discusses a Norwegian study of about 1,200 healthy men between forty and forty-nine years old with high cholesterol levels. Under the headline, "Life-Saving Benefits of Low-Cholesterol Diet Affirmed in Rigorous Study," Brody begins her story:

> A major, well-designed study has shown more persuasively than any previous experiment that eating less fats and cholesterol can reduce the chances of suffering a heart attack or of dying suddenly from heart disease. . . . Though their blood pressure was normal, their cholesterol levels were considered high—from 290 to 380 milligrams of cholesterol per 100 milliliters of blood—and 80 percent of them smoked cigarettes. . . . The team [of scientists conducting the research] . . . calculated that dietary changes accounted for 60 percent of the difference in the number of heart attacks and heart deaths suffered by the two groups of men. [*New York Times*, January 5, 1982, p. C-1]

No mention whatsoever is made that men of this age with levels of cholesterol averaging well over 300 mg/100 ml would have been in the upper 1 to 5 percent of the Framingham distribution—hardly typical people. And, in the penultimate paragraph, nestled away in an inside page, Brody adds: "The researchers conceded that 'if this had been a diet trial only, the difference in MI [myocardial infarction, or heart attack] incidence in the two groups would probably not have reached statistical significance.'"

As it turns out, cholesterol intake (even in this extreme group) is not a strong predictor of subsequent coronary heart disease and deaths. That certainly is not the tone or direction of the article. Not one scientific expert, apart from the study director, is asked to evaluate the quality of the work or its limitations. The results support the probable relationship and the prevailing presuppositions.

In reporting the MRFIT study (*New York Times*, September 17, 1982, p. A-10), Jane Brody's story details the methods used and several of the negative findings. Brody does a good job of this. But the article focuses on the various limitations of the study—a focus totally absent from the treatment of the Western Electric and other studies showing "positive" results. Brody notes the study director's own skepticism.

> Americans should not interpret the inconclusive results to mean it was all right to smoke, to be on a high fat diet and to have high blood pressure. "It is our judgment that the public should continue to reduce these risk factors associated with heart disease," said Dr. Oglesby Paul, professor emeritus of medicine at Harvard Medical School, chairman of the study's steering committee. . . . The researchers were also surprised to find that one treatment subgroup suffered a higher death rate than those getting regular treatment.

The criticism of MRFIT continues on October 10, 1982. The *New York Times* reports methodological problems enumerated by study officials.

Although caution and skepticism are no less in order for MRFIT than for the other studies, why is a "negative" or "inconclusive" result presented as a failing? Indeed, it is puzzling why the reporters covering the cholesterol question seemed committed to the point of view that cholesterol intake was a substantial health risk. To be sure, they were following the lead of health scientists, but why was the opposing point of view given less space and weight in their stories—especially since reporters find reports of controversy appealing?

In 1983 the probable link is stressed once again. The *New York Times*, January 11, 1983: In a brief article, "Reversing Heart Disease," this probability is suggested. "A newly published study indicates that significant improvement in cardiovascular health can occur after just 24 days of a radically changed diet and life style." This conclusion was reported on the basis of "a pilot study" of forty-six patients. Jane E. Brody in her "Personal Health" column of March 16, 1983:

> For a while it seemed as if the advice to lower cholesterol levels had fallen on hard times. . . . But before anyone could even digest a cholesterol rich meal, more meaningful data poured in. . . . The main recommendation is to eat less fat, in total, and especially less saturated fat, since this kind of fat raises cholesterol levels in the blood and increases the risk of heart disease. . . . So the heart-saving advice to reduce consumption of fats and cholesterol still holds. As a bonus, it may help protect you against cancer as well.

Brody fails to mention a single one of these new studies that "poured in"; no longer mentions skepticism about the results; omits all qualifiers about people at risk or the types of people who were enlisted in the research studies. The recommendation is apparently now applicable to all readers of the *Times*. Similar stories appear in other newspapers and on television news programs.

A week before *JAMA* publishes the Lipid Research Clinics' result, Peter Jennings on "ABC World News Tonight" anticipates the cholesterol publications when he and reporter George Strait interviewed Dr. R. Basil Rifkind of the NIH. Probability is now being transformed into fact:

> *Jennings:* Scientists now have conclusive proof that lowering the amount of cholesterol or fat that we eat can significantly reduce the chance of heart attacks. . . .
>
> *George Strait:* For more than a decade Americans have been warned to cut back on red meat, eggs and other foods high in cholesterol because scientists suspected that cholesterol was a prime cause of heart disease. Well, now they are sure. The new federal study confirms the definitive link between cholesterol and heart disease.

R. Basil Rifkind/NIH: The greater the reduction in cholesterol, the greater the reduction in coronary heart disease. And that a 25 percent reduction in blood cholesterol cuts risk by half. ["ABC World News Tonight," January 12, 1984]

The Lipid Research Clinics papers are published at this point. The media assumption that these studies provide a conclusive causal link between cholesterol and mortality from coronaries has been documented. What has happened since the scientific papers were published? As far as the media are concerned, we now are dealing with fact. So what has happened to the "fact" once "established"?

Phase III: From Probability to "Fact"

Since the publication of the LRC results, the media have moved still farther away from discussion of the study's limitations, have generalized from the limited findings, and have rarely seen fit to present opposing views. The same sources that condemned the National Academy for not reporting the full spectrum of opinion in *Toward Healthful Diets* have now totally abandoned the presentation of scientific disagreement. Thus:

Time, January 23, 1984: "Sorry, It's True, Cholesterol Really Is a Killer." And on March 26, 1984, *Time*'s cover story reads, "Cholesterol, and Now the Bad News." The *Reader's Digest* story is entitled "Cholesterol Is the Culprit." The statement that "every 1% cholesterol drop in the bloodstream means a 2% decrease in the likelihood of a heart attack in people with high cholesterol levels" is becoming a medical aphorism.

Victor Cohn in the January 29, 1984, *Washington Post:* "So now cholesterol is real. Government scientists have said so." The *Wall Street Journal,* March 15, 1984: "Cholesterol Levels in American Children Should Be Lowered, Heart Specialists Say." Jane Brody in the *New York Times,* May 16, 1984: "Diet to Prevent Heart Attacks Aims to Cut Blood Fat Levels." On March 27, 1984, Ted Koppel devotes ABC News' "Nightline" to cholesterol and heart disease. Dr. Robert Levy, a principal investigator in the Lipid Research Clinics study, states:

We've always known that cholesterol was a risk factor. We've always known that we've had diets and drugs that could lower cholesterol, but now for the first time in man, we have that conclusive evidence that if you lower your cholesterol your risk of heart attack and death . . . will decrease. . . . As you know, as with the tobacco story, there will be some who will never believe. But I think that the evidence that my family should act on, and I think all Americans should act on, is now in hand.

The views of other scientists who perceive the data differently are not presented or alluded to. The rule is now a general one. Finally, Jane Brody in a 1985 story discussing "new" findings from the Framingham heart study presents the health aphorism ("For every 1 percent change in cholesterol, the risk of heart disease changes by 2 to 3 percent") without any mention of the original source. It has now diffused into the culture.[9]

A Research Agenda for Understanding Distortions of Health Risks in the Media

The cholesterol case and others like it raise fundamental issues that need to be addressed before we can understand better what determines the distortion of health risks in the media. In this section questions are raised that might enter a research agenda on the presentation of health risks in the media. Specifically, processes at work in the development of scientific facts are examined, and a discussion follows on how the social organization of science and the media can influence the presentation of health-risk information.

Cholesterol and Coronary Heart Deaths: the Development of a Scientific Fact

The cholesterol case history is an exemplar for how values, intellectual conflicts, and socially structured interests influence the development of a scientific fact. The hypothesis we are considering for its fact status is: Dietary intake of cholesterol causes coronary heart disease and death. In its most graphic reportage form, the "fact" is that for every 1 percent reduction in cholesterol intake there is a 2 percent reduction in the risk of coronary heart deaths. Let us examine several elements in the struggle for fact status.[10]

The principal characters in this drama include a set of medical research scientists (the LRC group) who are proposing, in effect, that the results of their ten-year, $150 million study be accepted as a medical fact. Other actors include scientists who dispute this claim to fact, regarding the assertion as too strong an extrapolation from the available evidence and too sweeping in its claims to generality. Still others

[9] Even the National Research Council's Committee on Nutrition in Medical Education had changed its tune by 1985. See the National Academy of Sciences report, *Nutrition Education in U.S. Medical Schools* (1985).

[10] Space limits this section to the briefest elaboration of the relevance of ideas of Ludwik Fleck, Thomas Kuhn, Imre Lakatos, Bruno Latour and Steve Woolgar, Karen Knorr-Cetina, Robert K. Merton, and earlier work by Stephen Cole and myself as they bear on the development of "medical facts." See Cole 1986 for a broader discussion.

include powerful organizations and interest groups, among others, the American Heart Association, the National Institutes of Health, the National Academy of Sciences, and lobbies for the food industry, which had definite interests in the outcome of the fact dispute. The cast of characters is fleshed out by members of the press, science writers, editors, and reporters working the science beat, who transfer the content of this debate to the general public, and others with varying degrees of knowledge of the scholarly literature on which the fact claims are based. The contest is over the extent to which the fact claim is (*a*) accepted among influential members of the medical research community; (*b*) accepted for some duration of time by the research community to a degree that effectively eliminates disagreement from the scientific discourse; (*c*) accepted by science reporters as "definitive"; and (*d*) accepted by the public as a medical "fact."

The outcome of the contest can have significant effects on careers of scientists involved in the dispute, on the allocation of scientific resources for future research (and on processes of accumulating advantages and disadvantages), on the public's perception of health risks and what can be done about them, and on the economy of businesses whose products are affected by the public's perception of the effects of cholesterol.

How is the drama surrounding cholesterol related to current views about the social processes at work in the development of scientific facts? At least since Ludwik Fleck's work in 1935 on the development of the Wasserman reaction,[11] there is a recognition and growing acceptance that, at least in some measure, the choice of scientific problems, modes of presenting experimental evidence, acceptance or rejection of theories, selective evaluation of experimental results as a function of selective perception are socially conditioned.

More recently, Bruno Latour and Steve Woolgar (1979), as well as Karen Knorr-Cetina (1981), have suggested that the processes of "persuasion" and "negotiation" are central elements in the construction of scientific facts. Each of these elements plays a role in the fact dispute over the effects of cholesterol in the diet. In analyzing sixteen drafts of a single scientific paper on potato protein concentrates, Knorr-Cetina suggests that the actual difference in content had little to do with changes in evidence but was largely a consequence of extended negotiations between author and critics, including, among others, collaborators, laboratory directors, reference individuals, and authorities in the scientific speciality (Knorr-Cetina 1981, pp. 94–135, esp. p. 104).

[11] Fleck's (1935) work has become well known to American sociologists of science since its recent translation by Fred Bradley and Thaddeus J. Trenn, edited by Trenn and Robert K. Merton (1979).

Knorr-Cetina suggests that the typical outcome from negotiation in science is the changing of modalities of certain assertions, toning down of claims, movement from over- to understatement, from more to less dramatic styles of presentation, from assertions to caveats. In short, the early or original draft is stripped typically of some of its dramatic content—without any changes in evidence. The normative structure of science enjoins scientists toward such outcomes of negotiations.[12]

The published news story or broadcast is the analog to scientific publication. The final content of this story is, surely to some extent, also a process of negotiation. The critics with whom the reporter must negotiate include possible collaborators, science editors, and general editors. However, the norms governing negotiations in the news business tend to run counter to those operating in science (Goodell 1975; Nelkin 1985; Winsten 1985). Unlike most scientists, competent science reporters face pressures in negotiation to tone their stories up rather than down. Consequently, health-risk stories hitting the newsstand or reaching the air tend toward fewer modalities, qualifications, caveats, and statements of limitations. To get stories in print or on the air, science reporters are apt to have to expunge rather than add qualifying statements.

The differences in normative structures between science and news become particularly problematic for the final story when the earlier negotiation over the scientific paper has resulted in a highly "dramatic" and possibly exaggerated claim about experimental results. This dramatic and possibly distorted presentation can be exacerbated still further by the negotiation process in the newsroom.

Akin to negotiation is what Latour and Woolgar (1979) describe as the process of persuasion, that is, an ongoing contest between scientists, one set trying to change the modalities of statements so that the statement holds an increasing "factlike status" and another trying to be more restrained about claims to fact status. Some scientists, generally the authors of papers, are pushing statements that have a greater factlike quality; critics are frequently pushing for greater caution and qualification. Latour and Woolgar discuss five types of statements

[12] It is not self-evident that toned-down outcomes of negotiation facilitate the development of knowledge. We know little about the incidence of truly important papers that go largely unnoticed because negotiations led to overly restrained claims and, correlatively, the incidence of exorbitant claims that led to significant false starts and unproductive research programs.

found in scientific papers, ranging from statements of "facts" that everyone takes for granted (Type 5) to far less definite, limited, and more qualified statements, such as "*A* has a certain relationship to *B*," and finally to the fully qualified modality of conjectures and speculations (Type 1 statements). Latour and Woolgar characterize laboratory activity in terms of pushes and pulls toward fact status that result from interaction between fact claimants and their critics.

They are fully aware, of course, that sociological factors (such as power, authority, theoretical and methodological orientations, and personal values) play a critical role in the persuasion or negotiation process. But they do not consider the influence of structural features of science beyond the laboratory, or more macrolevel factors that may influence the outcomes of contests and struggles about fact status.[13]

This work bears directly on the recent cholesterol papers and publicity, because we see in the cholesterol controversy an ongoing contest about what should be granted fact status. We witness an effort by one set of researchers, most recently those associated with the Lipid Research Clinics' long-term clinical trial, who are attempting to persuade others—scientists, journalists, and the wider public—of the unadorned *fact* that dietary intake of cholesterol causes heart deaths. They are asserting with increasing frequency Type 5 statements that label cholesterol as a major cause of heart deaths for all members of the U.S. population. If they are the protagonists in this drama, the antagonists are those scientists, such as Ahrens as well as Brown and Goldstein, who are advocating scientific statements that continue to include strong qualifying modalities. It is important to emphasize that contests over fact status often take place with new empirical or experimental evidence continually influencing the dialogue, but in the recent cholesterol case the contest seems to be proceeding quite independently of additional evidence.

Throughout this paper examples have been presented of efforts by investigators associated with the LRC trial to reduce the number and types of qualifying statements, which have the effect of enhancing the fact claims by the authors. These actions suggest efforts by the principal investigators to persuade potential critics and the public of the fact status of their findings.

[13] Knorr-Cetina (1981) and Latour and Woolgar (1979) concentrate on changes in texts that are attributed to different interests of authors and critics. They underplay changes that result from an awareness gained by authors through criticism that they have overgeneralized or have made statements which, upon reflection, are viewed by the authors as errors. Furthermore, they fail to see the act of writing as a creative process that represents an action quite distinct from the activities in the laboratory. This process of creating a text can itself lead to new ideas, some of which appear upon critical reflection to be too strong, too weak, imprecise, or in error. Thus, changing modalities of statements in scientific papers are not purely a result of a clash of interests, values, and so on.

Here is where science reporters and the media in general enter the negotiation and persuasion process. They are recipients of the statements that emerge from scientific papers published in prestigious journals, such as *JAMA* or the *New England Journal of Medicine*, from personal interviews with scientists, and from formal news conferences and briefings. They are an important audience in the contest over fact status, because indirectly they influence opinion formation, not only in the larger public but in the attentive publics of physicians who offer advice to patients (but who do not necessarily read the scholarly literature on cholesterol), other scientists, health administrators, staff members of congressmen and administrative agencies, and decision makers at federal funding agencies who oversee the funding of large-scale laboratory and clinical trial programs.

When dealing with the press, scientists interested in changing the modality of statements about research results are often not subject to the same mechanisms of social control that operate within their discipline.[14] A simplification may involve only the omission of a strategically placed "could" or "maybe" or "applies only to . . ." from earlier, more cautionary statements. Reporters can hardly be expected to be familiar with much, or all, of the detailed work that comes out of a laboratory, or with the details of the published data.[15] When acceptance of less tentative statements is coupled with the normative tendency within journalism to strengthen rather than weaken stories through further reduction of qualifying modalities, there is an increased probability of "premature facts" being elevated to "fact" status and being widely accepted as such.[16]

Institutional Properties Influencing the Distortion of Health Risks in the Media

At least three institutional properties of both science and the media are apt to increase the probability of distorted representations of sci-

[14] We do not have any extensive data that can demonstrate this; there is plainly a need to develop systematic data to go beyond the impressions based upon more limited inquiries by Winsten and others.

[15] There are probably a few specific research areas that each science reporter is familiar with. Based upon the reporters' own science background and subsequent interests, this may allow them to be highly informed critics in these specialty areas. To cite only one notable example, Walter Sullivan of the *New York Times* has extensive knowledge and has written important books on the geology of plate tectonics.

[16] Ludwik Fleck has an extraordinary discussion of the simplification process and its consequences for fact development as we move from scientific papers to textbooks to popular accounts. See Fleck (1979), pp. 111–24.

entific "discoveries." The first of these is the structure of the reward and opportunity systems in *both* institutions; the second, inadequate training of reporters as critical observers of scientific information; and the third, ineffective mechanisms in the media and science to forestall the distribution of premature results or distorted news.

The Reward Systems—There are, of course, many forms of scientific recognition: prestigious positions; honorific awards, such as Nobel prizes or election to leading academies of science; and visibility and peer esteem (see Cole and Cole 1973; Merton 1942, 1973; Zuckerman 1977). There is a substantial overlap among the recipients of these various forms: The few receive the lion's share. But with increasing frequency, scientific and public visibility, if not notoriety, can be attained through extensive media exposure—and sometimes even by conducting scientific controversies through the media (see Cole 1979; Goodell 1975).

Scientists, like others, seek recognition from important reference groups. Although precise figures do not exist, we may ask what proportion of scientists completely eschew media attention of their work; what proportion accept it without seeking it; and what proportion actively seek representation in the media. For the group, however small, who seek visibility through the press, what determines their interest? Some scientists may have a personal and professional interest in being "represented" in the press.

The consequences of representation in the press can extend, of course, beyond personal psychic gratification. Press exposure may aid scientists in obtaining continued resources for their research—especially if the research program is extremely expensive and is directly reviewed by congressional committees and directors of funding agencies.

The problem goes deeper than passivity among scientists because some (again, the exact proportion is surely unknown) contribute directly to media distortions by tailoring their own results through incomplete and misleading paper abstracts, news releases, and press conferences that give the media what the scientist has defined as "strong" news stories—even at the expense of complete summaries and inclusion of essential limitations. In the cholesterol case, we see the principal investigators in television interviews and newspaper stories making extreme claims for the results without acknowledging the limits to the data. But for the medical research and larger scientific community, we know little about the sources of distortion in reporting.

Jay A. Winsten (1985) recently completed focused interviews with twenty-seven science reporters, editors, and television producers at leading news organizations. Many of the reporters interviewed were

authors of stories addressing health risks associated with cholesterol. The reporters repeatedly told Winsten that the competitive pressures of journalism and interest in recognition often led to altered stories, creating the appearance of either greater conflict and tension or greater consensus than in fact existed, and to other significant alterations in stories to meet the requirements of editors who were gatekeepers in opening or shutting off opportunities for advancement and recognition. For example, one reporter told Winsten of tension between building a "strong" story while maintaining credibility:

> I'm in competition with literally hundreds of stories each day, political and economic stories of compelling interest. In science, especially, we sometimes have to argue [with editors], pound the table, and say, "This is an important story. It turns a key of understanding, it affects a lot of people," or "it's just interesting, it's part of the unfolding romance of science." But we have to make that clear in our copy. We have to almost overstate, we have to come as close as we can within the boundaries of truth to a dramatic, compelling statement. A weak statement will go no place. [Winsten 1985, p. 9]

Science editors, Winsten reports, are aware of the tension and the competing values associated with tension and accuracy.

The pressures for recognition among reporters and science editors are summarized in Winsten, but we surely cannot tell from the twenty-seven interviews, however instructive, how generalizable the results are, that is, with what frequency reporters and editors push beyond the boundaries of truth as they come to know it.

Structural Problems that Can Lead to Distortion

Problems of the Training Necessary to Report on Scientific and Medical Research— A potential source for the publication of biased or distorted health-risk information lies in the quality and quantity of training of those on the "science beat." In fact, for most newspapers and broadcast journalism, general assignment reporters far outnumber science reporters in covering science topics. This was not true for the sources we used in tracing the cholesterol controversy, but it is the general case. Even if we focus exclusively on science reporters, what proportion of them and their editors can be sufficiently knowledgeable about the sciences covered in their stories to assume a critical posture toward the scientists who are making claims of fact? Given that science has become so highly specialized and technical, we can hardly expect that the majority of reporters would have specific knowledge of most of the sciences about which they write. Of course, it remains unclear how much detailed knowledge a reporter needs about the basic canons of scientific inquiry, of proof and evidence, of scientific

skepticism, and of how facts and theories develop in the scientific community to write stories that correctly depict the fact claims and the extent to which these claims are matters of contention within the research community. Short of extraordinary knowledge, reporters must, of course, rely heavily on scientific authority, on the "word" and reputations of the principal investigators and scientific institutions, and on the peer review system. This can be problematic for several reasons, especially in dealing with health risks and medical research.

First, many flawed papers pass through the peer review system and are published without adequate attention to methodological or substantive inaccuracy—and this is so for the journals of the highest rank as well as those of lesser stature. Recently, DerSimonian et al. (1982) studied the reporting of clinical trials in the *New England Journal of Medicine, British Medical Journal, Journal of the American Medical Association,* and *Lancet.* They chose eleven basic topics and examined whether each one had been discussed in the paper reporting the clinical trial experiment.[17] Of the sixty-seven papers reviewed, only 1 percent gave information on all eleven items; more than 50 percent provided information on six or fewer. In short, many papers that have passed through the peer review process fail to provide basic information that allows readers to assess the quality of the study.

Second, the peer review system is marked by honest, intellectual disagreement among scientists about what constitutes "high-quality work" and what is a valuable paper. Published papers often had one or more referees who identified substantial problems with the papers (among many others, see Ingelfinger 1974; Stinchcombe and Ofshe 1969; Zuckerman and Merton 1971). Such disagreements in appraisals may actually be healthy for a science and, in any event, are common when people evaluate new scientific work at the frontier of knowledge, but as a consequence, acceptance or rejection will depend significantly on the "luck of the reviewer draw" (see Cole and Cole 1985; Cole, Cole, and NAS Committee on Science and Public Policy 1981; Cole 1983; Cole, Cole, and Simon 1981). Thus, peer review may represent the best of all possible but highly imperfect worlds. In sum, the fact that a paper appears in a prestigious medical journal offers no strong guarantee against the appearance of poorly analyzed or distorted results of medical and scientific research.

What is the media to do about such problems? They surely cannot develop an independent refereeing system, but it may be possible

[17] The eleven items were: eligibility criteria, admission before allocation, random allocation, method of randomization, patient's blindness to treatment, blind assessment of outcome, treatment complications, loss to follow-up, statistical analysis, statistical methods, and power (quoted in Mosteller 1985).

to extend the training of experienced reporters in the methods used in research and experiments as well as new analytic and statistical techniques.

Problems of Timing— Television and newspaper reporters are often under considerable pressure to get stories out in a timely way. Constraints of time and space in storytelling can work against adequate presentation of the limitations of scientific findings, which often require a longer period of time to investigate the adequacy of the evidence. However, data do not exist that address the relationship among the length of news stories, the speed with which they must be developed, and the level of distortion in the final presentation of health risks.

Value Commitments of Scientists and Reporters in the Reporting of Science News

Personal values can also affect the handling of scientific data and preparation of science news stories. Work in the history and sociology of science over the past two decades supplies us with evidence that ideological presuppositions and values influence the perceptions of scientists; their choice of problems; their choices in examining one source of possible errors as opposed to another; their reading and interpretation of the data and results (Barber 1961; Gould 1981; Holton 1978; Merton 1972; Polanyi 1963). These cognitive tendencies, which do not necessarily involve motivated deception, may be exacerbated when personal rewards and resources are more apt to accrue to those who conform to the received wisdom in the scientific community.

Pressure for the presentation of eye-catching and somewhat overblown results may also come today from the sources for funding science. With increased interest in fiscal "accountability," with populist pressures for more egalitarian distribution of scarce federal dollars for researchers, it is probably difficult for science programs that spend upward of $100 million on research to conclude: "The results are inconclusive." This is a hard position to argue and sustain, especially when entire laboratories and scores of scientists depend upon the government's support for their continuing research. Such pressures may produce a strain toward early or premature publication and an attempt to characterize results for the press and scientist colleagues more dramatically than they warrant.

Finally, if scientists and medical researchers have their set of presuppositions that influence the conduct of their research and its outcome, surely reporters and health columnists have their own presuppositions and ideological beliefs. Logically, this should influence their choice of

stories and their selective perception and use of evidence, although little empirical work exists to specify this claim. Thus, the values, biases, and attitudes of both reporters and scientists can interact to create conditions under which the probability for significant distortion of health risks and other science news is increased.

The Absence of Effective Social Control

The absence of effective mechanisms of social control can also influence distortions about health risks. High-quality monitoring and feedback systems do not yet exist. Public "taste" for stories is plainly not a sufficient barometer of how well the media are doing.

One mechanism of social control is through complaints. However, the system of reporting health risks tends to reduce the effectiveness of this mechanism because scientists whose stories are covered tend to be a highly self-selected lot who are apt to be pleased by the coverage. Furthermore, those scientists who are skeptical about the likelihood of distorted reporting probably shy away from coverage.

Except for self-criticism within the media, there are few mechanisms to my knowledge that operate to minimize distortion. Jay A. Winsten's interviews suggest that editors' insistence on producing "strong" stories has the frequent consequence of distorting them:

> The desk editors are the ones who always want you to push it a little harder. . . . I go to the desk, and they say, "Well, can't you make it a little stronger?" And then they give you an alternative. And I say, "No, you've just eliminated a qualifier, or you've added that word there that's just not true." And you go back and forth like that. So there is movement in the direction of a stronger statement—running out to the boundaries at which you've overgeneralized, at which you've just overdone it. [Winsten 1985]

Thus, the forces operating to produce an engaging and readable newspaper story or television news program appear to work against accurate and balanced reporting of health risks. More effective feedback mechanisms about balance and accuracy need to be constructed, if we are to reduce distortions.

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