DISCUSSION PAPER

• ABSTRACT

The culturally-dominant view of the popularization of science rests on a two-stage model: first, scientists develop genuine knowledge; second, popularizers spread streamlined versions to the public. At best, popularization is seen as a low-status educational task of 'appropriate simplification'. At worst, it is 'pollution' — the distortion of science by outsiders. This paper shows that the dominant view suffers from conceptual problems, and greatly oversimplifies the process. But, despite these weaknesses, the dominant view serves scientists (and others who derive their authority from technical expertise) as a resource in public discourse, providing a repertoire of rhetorical devices for interpreting science for outsiders, and a powerful tool for sustaining the social hierarchy of expertise.

The Dominant View of Popularization: Conceptual Problems, Political Uses

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The culturally-dominant view of the popularization of science is rooted in the idealized notion of pure, genuine scientific knowledge against which popularized knowledge is contrasted.[1] A two-stage model is assumed: first, scientists develop genuine scientific knowledge; subsequently, popularizers disseminate simplified accounts to the public. Moreover, the dominant view holds that any differences between genuine and popularized science must be caused by 'distortion' or 'degradation' of the original truths. Thus popularization is, at best, 'appropriate simplification' — a necessary (albeit low status) educational activity of simplifying science for non-specialists. At worst, popularization is 'pollution',[2] the 'distortion' of science by such outsiders as journalists,[3] and by a public that misunderstands much of what it reads.[4]

This paper argues that the dominant view of popularization greatly oversimplifies the process; but despite conceptual problems, the dominant

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view of popularization serves scientists (and others who derive their authority from science) as a political resource in public discourse. The dominant view provides a vocabulary of non-science used in rhetorical 'boundary work',[5] to demarcate 'genuine' from 'popularized' knowledge (or, in alternative, but roughly parallel formulations, to demarcate 'real science' from 'popularized science', or the 'products of science' from the 'products of popularization'). A concept of purity requires one of contamination, and the notion of popularization shores up an idealized view of genuine, objective, scientifically-certified knowledge. Furthermore, the dominant view establishes genuine scientific knowledge, the epistemic 'gold standard', as the exclusive preserve of scientists; policy makers and the public can only grasp simplified representations. Finally, this view of popularization grants scientists broad authority to determine which simplifications are 'appropriate' (and therefore usable) and which are 'distortions' (and therefore useless – or worse!).

As a consequence, scientific experts enjoy great flexibility in public discourse.[6] On the other hand, when it suits their purposes, they can issue simplified representations for broader audiences; the notion of 'appropriate simplification' justifies this practice, and enables scientists to invest these representations with the authority of the cultural symbol 'Science'. On the other hand, scientists at all times can draw on the notion of 'distortion' to discredit publicly-available representations.

The remainder of this paper briefly explores these points. I begin with a critique of the dominant view of popularization, then turn to the way it is used. Examples to illustrate my argument will be drawn from the case of an influential paper on the causes of cancer, and I will now give a short synopsis of its history.[7]

The Causes of Cancer

In 1981, the British epidemiologists Sir Richard Doll and Richard Peto published 'The Causes of Cancer: Quantitative Estimates of Avoidable Risks of Cancer in the United States Today' (hereafter 'the Causes of Cancer').[8] The paper, which appeared in the *Journal of the National Cancer Institute*, was a massive review of the literature on cancer etiology that Doll and Peto intended for 'interested non-specialists'.[9] In the paper, they developed quantitative estimates, expressed as percentages, of the number of cancer deaths 'in the United States today' that are attributable to various risk factors (see Table 1).

Doll and Peto's paper quickly became important in debate on cancer

TABLE 1
Doll and Peto's Original Table
Proportions of Cancer Deaths Attributed to Various Different Factors

		Percent of all cancer deaths	
Text section no.	Factor or class of factors	Best estimate	Range of acceptable estimates
5.1	Tobacco	30	25–40
5.2	Alcohol	3	2–4
5.3	Diet	35	10–70
5.4	Food additives	<1	−5[a]–2
5.5	Reproductive[b] and sexual behaviour	7	1–13
5.6	Occupation	4	2–8
5.7	Pollution	2	<1–5
5.8	Industrial products	<1	<1–2
5.9	Medicines and medical procedures	1	0.5–3
5.10	Geophysical factors[c]	3	2–4
5.11	Infection	10?	1–?
5.12	Unknown	?	?

[a]Allowing for a possibly protective effect of antioxidants and other preservatives.
[b]*See* section 5.5 for intended meaning.
[c]Only about 1%,not 3%, could reasonably be described as 'avoidable' (*see* text). Geophysical factors also cause a much greater proportion of non-fatal cancers (up to 30% of all cancers, depending on ethnic mix and latitude) because of the importance of UV light in causing relatively non-fatal basal cell and squamous cell carcinomas of sunlight-exposed skin.
Source: Doll & Peto, op. cit. note 8, 1256.

policy. Their numbers, once reified as facts, were used to argue for changes in the American diet, in regulatory policy, and in cancer research priorities. Their table of percentages became a standard slide in lectures on risk assessment in the United States, where it was used to document the claim that smoking and diet account for many more cancer deaths than does industrial activity. The paper was widely cited in scientific journals, receiving more than 490 citations in the *Science Citation Index* over the years 1981–86.[10] Doll and Peto's estimates were quoted in many other places, including government documents on cancer policy,[11] National Cancer Institute reports,[12] newspapers and magazines,[13] law review articles,[14] pamphlets offering diet advice,[15] and the publications of advocacy groups.[16]

At the outset of the Causes of Cancer, Doll and Peto discussed the conceptual and empirical problems involved in developing estimates of the percentages of cancer caused by different risk factors. They cautioned that their estimates attributed cancer causation to single factors; in reality, they noted, risk factors may interact (for example, as in the case of smokers who also are exposed to carcinogens in the workplace), making such single-factor attributions problematic.[17] Throughout the paper, they also warned about the limitations of the data, particularly in the case of diet, and they argued that their percentages were 'not really comparable' with one another, since some were fairly precisely known and others were much more uncertain.[18] Nevertheless, they concluded that despite such 'drawbacks, the "percentages" that we have attributed to each way. . .of avoiding cancer remain for us a useful summary of certain facts'.[19]

Doll and Peto gave a 'best estimate' and a 'range of acceptable estimates' for each risk factor. In the case of the percentage of cancer deaths attributable to diet, which I will pay particular attention to below, the best estimate was 35%, and the range was 10–70%. As Doll and Peto's estimates spread into numerous publications, they were simplified and presented in many forms. For example, later users of the estimates frequently dropped the ranges. In general, as the estimates spread, qualifications and modalities were deleted, and the numbers seemed to grow more authoritative.[20] Table 2 provides some illustrative quotations that presented the diet estimate, which Doll and Peto wrote had been 'guestimated',[21] and was 'highly speculative',[22] but which subsequently was often treated as an unproblematic fact. Figure 1 summarizes some ways in which the diet estimate was transformed as it spread. Because simplified versions of Doll and Peto's estimates appeared in many places, the history of the Causes of Cancer provides useful case material for considering the dominant view of popularization.

The Problems of 'Popularization'

In light of recent scholarship in the sociology of scientific knowledge,[23] the dominant view of popularization has seemed increasingly inadequate. For one thing, popularized knowledge feeds back into the research process.[24] Scientists learn about fields outside their immediate research areas from popular accounts, and these shape their beliefs about both the content and the conduct of science.[25] Second, simplification is important in scientific work, both within the laboratory,[26] and in communicating with students, funding sources, and specialists in adjacent fields.[27] Finally, recent work has argued that scientific knowledge is

TABLE 2
Illustrative Versions of Doll and Peto's Diet Estimate

Doll & Peto, Causes of Cancer, 1135.

> The outcome of future controlled laboratory research and of the epidemiologic observation of human risks may well be, in our view, that diet will be shown to be a factor in determining the occurrence of a high proportion of all cancers of the stomach and large bowel as well as of the body of the uterus (endometrium), gallbladder, and (in tropical countries) of the liver. Diet may also prove to have a material effect on the incidence of cancers of the breast and pancreas and perhaps, through the anti-carcinogenic effects of various micronutrients, on the incidence of cancers in many other tissues. If this is so, it may be possible to reduce US cancer death rates by practicable dietary means by as much as 35% ('guestimated' as stomach and large bowel, 90%; endometrium, gallbladder, pancreas, and breast, 50%; lung, larynx, bladder, cervix, mouth, pharynx, and esophagus, 20%; other types of cancer, 10%). Although this figure of 35% is a plausible total, the parts that contribute to it are uncertain in the extreme, so the degree of uncertainty in the total should be obvious, and we make no pretence of its reliability. It is still possible (though rather unlikely) that no practicable modifications of diet will be discovered that can reduce the total US cancer death rates by more than 10%, but it is equally possible that reductions of perhaps even double our suggested total of 35% might ultimately be achievable, although this certainly cannot be expected in the near future.

C. H. Hennekens, 'Micronutrients and Cancer Prevention', *New England Journal of Medicine* (13 November 1986), 1288.

> Promising, but unproved, hypotheses concern the prevention of cancer through diet, which has been postulated to account for as many as 35% of annual cancer deaths. [citation to the Causes of Cancer] Which characteristics of diet, if any, increase or decrease the risk of cancer is unclear.

O. Alabaster, 'Change Your Diet, Save Your Life', *Cosmopolitan* (May 1985), 112.

> The consensus today is that at least 35 percent of all cancer in this country could be eliminated by simple changes in the nation's diet – using our current knowledge of dietary risk factors.

S. Weinhouse, 'The Role of Diet and Nutrition in Cancer', *Cancer*, Vol. 58 (15 October 1986), Supplement, 1792. (The paper was originally the keynote address at the American Cancer Society Second National Conference on Diet, Nutrition, and Cancer, September 1985.)

> Expert opinion, compiled by Doll and Peto [citation to the Causes of Cancer], considers that smoking and diet are risk factors in about 65% of all human cancer deaths in the US.

National Cancer Institute, *Cancer Control Objectives for the Nation: 1985–2000* (NCI Monographs, 1986), No. 2, 20.

> Roughly 35 per cent of cancers may be attributable to dietary factors [citation to the Causes of Cancer]

W. S. Ross, 'At Last, An Anti-Cancer Diet', *Reader's Digest* (February 1983), 78.

In 1981 British researchers Richard Doll and Richard Peto suggested that it may be possible through dietary changes to reduce US cancer deaths by about a third.

National Cancer Institute, *National Cancer Program: 1983–1984, Director's Report and Annual Plan FY 1986–1990* (NIH Publication No. 86-2765, November 1985), 16.

Nearly 35 percent of cancer is influenced in some way by diet.

T. C. Campbell (a nutrition scientist) interviewed by E. Levin, 'In the War Against Cancer, the Latest Weapons are Fruits and Vegetables', *People* (12 July 1982), 65.

Recently two respected British researchers suggested that diet might be responsible for 35 percent of all cancers. The number was a guesstimate; they said it might be as low as 10 percent or as high as 70 percent.

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constructed through the collective transformation of statements,[28] and popularization can be seen as an extension of this process, rather than an entirely different one.[29]

'Genuine Knowledge' or 'Popularization'

The dominant view of popularization rests on the assumption that the task of differentiating 'genuine knowledge' from 'popularized knowledge' is straightforward. Certainly, this view fits with common sense, and much of the time it is workable enough. For example, most observers would agree that a television news report is 'popularized' and a paper in a scientific journal is not. However, when one looks carefully for the *precise location* of the boundary between genuine scientific knowledge and popularized representations, one runs into trouble, stemming from the fact that scientific knowledge is presented in many contexts.

Figure 2 arrays these contexts in a spectrum ranging from laboratory 'shop talk',[30] to technical seminars, to scientific papers in journals, to literature reviews, grant proposals, textbooks, policy documents, and mass media accounts. Clearly, contexts toward the left end of the spectrum are directed at much narrower, more specialized audiences than those 'downstream' toward the right end. Nevertheless, ambiguities may arise when one tries to squeeze this spectrum into the binary categories of 'genuine' versus 'popularized' knowledge that underlie most discussions of popularization.

Strategies for dividing this complex spectrum into these binary categories inevitably suffer from problems. The most promising strategies for doing this are based on examining the communication context in which knowledge is presented. For example, one could define genuine scientific knowledge as that which is presented by scientific experts to scientific audiences in scientific forums; all the rest, then, would be defined as popularization. Such an approach transforms the problem of locating the boundary between genuine and popularized knowledge into the problem of identifying the experts, audiences, and forums that represent the 'genuinely scientific'. But this manoeuvre produces new ambiguities about how broadly to define these categories. Does the set of speakers who possess genuine scientific knowledge about superconductivity include only a 'core set' of a few world experts,[31] or every member of the American Physical Society? Do medical doctors constitute a truly scientific audience, or should one reserve that category for active researchers? Are literature reviews, textbooks, and articles in *Scientific American* forums for communicating genuine scientific knowledge, or do they merely retail popularized accounts? And how should one categorize so-called 'hyphenated scientists' (for example, scientist–administrators, scientist–bureaucrats, scientist–entrepreneurs, or scientist–PR operatives)?[32] Obviously there is considerable flexibility in how one answers such questions.

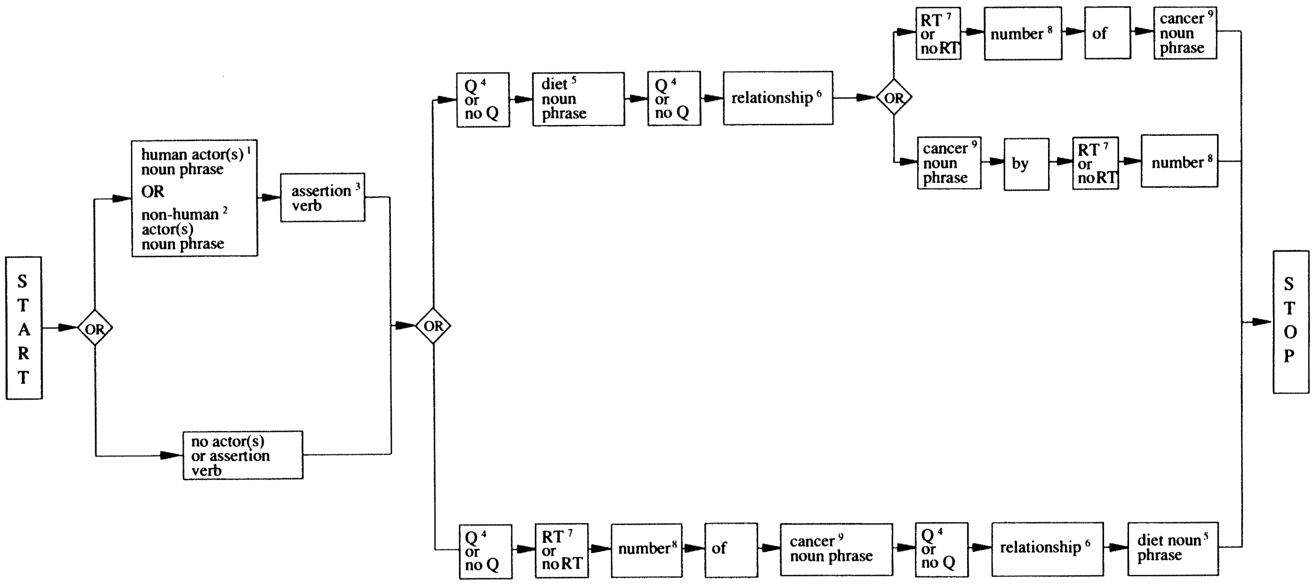
A second strategy for defining the boundary between genuine and popularized knowledge would be to look at the content. Clearly, claims circulated among scientific peers tend to be stated in more specialized language and with greater precision than those advanced in more public contexts. Thus, one could try to identify the boundary between genuine and popularized knowledge by looking at the nature of the claims. But the precision with which a claim is stated is clearly a matter of degree, so, again, the boundary between genuine and popularized science could be drawn at a number of points.

A third strategy relies on identifying the 'original' knowledge and strictly distinguishing between its creation and its spread. For example, if one could identify the specific point where the genuine, scientific knowledge was first created, then one could argue that all downstream representations were popularization. Following this line of reasoning, one might maintain that only the original report of a new scientific fact constituted genuine knowledge, and that everything downstream was popularization. But because novel claims must be certified by the relevant expert community before they are treated as credible, the original report cannot be considered factual at the time of its publication; it must first be picked up, discussed, and modified in downstream meetings, papers, and reviews. Because facts emerge only as they stabilize and are accepted

FIGURE 1

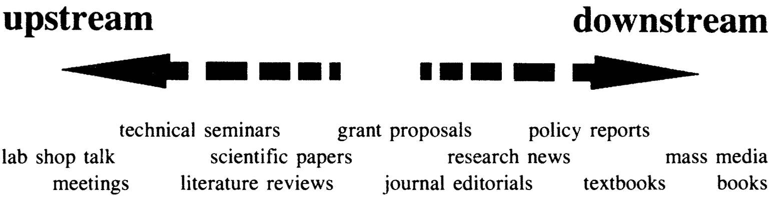
Transformations of theAuthorship and Content of the Diet Estimate

As the diet estimate spread, both the authorship and the content of the claim were reported in various ways. Grammatical sentences can be assembled by following the flow chart and making appropriate selections from the lists of options at the bottom of the figure. All of the options in these lists appeared in actual reports of the estimate, as did many others. Even in this simple structure (and clearly much more complex ones are possible) a wide range of representations can be constructed.



[1]*human actor(s) noun phrase*	[3]*assertion verb*	[5]*diet noun phrase*	[7]*rounding term (RT)*	[9]*cancer noun phrase*
British researchers Doll and Peto	to show	dietary factors	approximately	US cancer deaths
two world-famous scientists	to indicate	diet	about	US cancer rates
epidemiologists	to suggest	what we eat and drink	more than	cancer deaths
other researchers	to demonstrate	dietary changes	up to	cancers
scientists	to estimate	changes in diet	as much as	cancer
experts	to believe		nearly	
	to say			
[2]*non-human actor(s) noun phrase*	[4]*qualifier (Q)*	[6]*relationship*	[8]*number*	
the latest data	may	to cause/to be caused by	between 10 and 70 percent	
epidemiological research	might	to account for/to be accounted for by	35 percent	
recent studies	perhaps	to influence/to be influenced by	35%	
research	in some way	to be prevented by	thirty five percent	
	it is possible that	to be a factor in	a third	
		to be traceable to		
		to be related to		

FIGURE 2
Contexts in which Scientific Knowledge is Communicated



in downstream representations,[33] this strategy for drawing the boundary between genuine and popularized knowledge relies on selective hindsight; its failure leads one back to one of the first two strategies, with their attendant ambiguities.

The point of the above discussion is not that there are no differences between a report on DNA repair in *Cell* and an article on the same topic in *The Baltimore Sun*; obviously, there are. The point is simply that 'popularization' is a matter of degree. The boundary between real science and popularized science can be drawn at various points depending on what criteria one adopts, and these ambiguities leave some flexibility about what to label 'popularization'.

For a concrete example of these ambiguities, consider the case of the Causes of Cancer. Scientific knowledge about cancer etiology was represented in numerous forms as it was reviewed in the paper, packaged in the estimates, presented in tables, and quoted and requoted. One must ask, then, which of these representations should be considered 'real science', and which considered popularized? Or alternatively, at what point in the history of the paper did these representations of scientific knowledge cross over into the realm of popularization? Multiple answers are possible. One could, for example, argue that the literature the paper summarized was real science, but that the Causes of Cancer was popularization because it was a 'review' which its authors said was intended primarily for 'non-specialists'. Alternatively, one could claim that the paper was not popularization because it synthesized the literature in a way that created new knowledge; because later users (including scientists) treated the paper as genuine science, citing it widely and praising it;[34] and because the prestigious *Journal of the National Cancer Institute* would never devote more than 100 pages to mere popularization. Or, in a hybrid of these arguments, one could claim that portions of the paper were popularization, and portions were not.

Further ambiguities arise when one examines the spread of Doll and

Peto's estimates to publications downstream. Consider, for example, the diet estimate. Simplified versions of the estimate appeared not only in newspapers and magazines, but in a wide variety of publications, including reports by the National Cancer Institute (a science-based agency) and articles in scientific journals (written by scientists). Once again, multiple answers are possible to the question 'which simplified versions should be considered popularization?'.

In an effort to resolve these ambiguities, one could take yet another tack, arguing that Doll and Peto's original statement of the diet estimate was genuine science, but that *all* simplified representations of it qualify as popularization. But this line of reasoning leads to the conclusion that the process of popularization began *within* the Causes of Cancer,[35] when Doll and Peto moved from their initial, paragraph-long presentation of the number to the shorter versions that appeared elsewhere in the paper (such as in Doll and Peto's table of percentages; see Table 1 in this paper). The point of these examples is not to engage in a potentially endless debate about 'how popularized is popularized?', but to illustrate the flexibility with which the term can be deployed.

'Appropriate Simplification' or 'Distortion'?

A second problem with the dominant view of popularization is that it suggests that one can distinguish 'appropriate simplification' from 'distortion' in a straightforward manner. Even more than the boundary between genuine and popularized knowledge, this boundary is ambiguous, flexible and dependent on the context. It is impossible to restate a claim without transforming it in some way,[36] and for a variety of reasons,[37] as scientific knowledge spreads, there is a strong bias toward simplification (that is, shorter, less technical, less detailed representations). Comparing a later, 'downstream' representation with an earlier, 'upstream' version will always reveal some differences. The question, then, in any particular case, is whether the changes are *significant*. Is the particular transformation 'misleading' (and therefore blameworthy)? Or would it be nitpicking to argue that the difference mattered? Do the changes make the downstream statement 'distorted', 'incorrect', or 'oversimplified'? Or do they leave it 'appropriate', 'accurate', and 'essentially correct'? Clearly, these are not questions that can be answered objectively, but are judgements about what is 'good enough' under the circumstances. And just as clearly, in many situations, observers will make different judgements depending on their social location, interests, and appraisal of 'the' circumstances.

For an illustration of the ambiguity of the boundary between appropriate simplification and distortion, consider again the case of the Causes of Cancer. At what points in the history of the paper did knowledge about cancer etiology get 'distorted'? Did distortion occur when Doll and Peto decided (despite conceptual and empirical problems) to use percentages to summarize the unwieldy data on cancer etiology? Or was the use of percentages an appropriate expository device, a useful way to give a 'summary of certain facts'?[38] Did distortion occur when they developed the 'highly speculative',[39] 'guestimated' diet estimate?[40] When they placed that estimate in a table (which, after all, is a device for facilitating comparison), after noting that their numbers were 'not really comparable' with one another?[41] One could argue that distortion occurred when others quoted the diet estimate, transforming it along the way. One could carefully scrutinize every little change to determine whether it was appropriate simplification or distortion. Was it distortion to drop the 10–70% range? To transform authorship (see Figure 1)? To call the 'estimate' a 'finding'? To delete qualifiers? To change the domain to which the number referred (for example, from cancer deaths to cancer cases)? To link the estimate to various 'implications' (for example, that Americans should eat high-fibre breakfast cereals)? One could argue that distortion occurred at all of these points, at one of them, or at no single point, but through the process as a whole. Clearly, observers often enjoy considerable flexibility when applying the concepts of appropriate simplification and distortion.[42]

The Uses of 'Popularization'

As we have seen above, the dominant view of popularization suffers from conceptual and empirical problems, and consequently it has limitations as an analytic tool. Nevertheless, the dominant view remains a useful political tool for scientific experts, and this section of the paper explores its political uses. Most fundamentally, the dominant view sets aside genuine scientific knowledge as belonging to a realm that cannot be accessed by the public, but is the exclusive preserve of scientists. It thus buttresses the epistemic authority of scientists against challenges by outsiders. At the same time, the dominant view provides scientific experts with a repertoire of conceptual and rhetorical tools that can be used when representing science to outsiders.

For one thing, the flexibility of the boundary between appropriate simplification and distortion permits scientists considerable leeway when constructing simplified representations of scientific knowledge. Experts are granted broad discretion about what aspects of a subject to simplify, how much to simplify, what language and metaphors to use in simplified accounts, and what criteria to use when matching their presentations to their audiences. Obviously, it would be naïve to assume that their simplified representations are politically neutral. On the contrary, a mountain of evidence shows that experts often simplify science with an eye toward persuading their audience to support their goals:[43] whether they seek to motivate people to follow public health recommendations, build support for research programmes, convince investors that a finding shows commercial promise,[44] or advocate positions in science-intensive policy controversies.

In the case of the Causes of Cancer, critics of government cancer policy pressed into service simplified versions of Doll and Peto's numbers. Seeking to relax the regulations governing exposures to carcinogens, such critics often presented Doll and Peto's estimates as straightforward facts with direct implications for policy.[45] Similarly, scientists and officials at the National Cancer Institute (among other organizations), who sought to motivate people to change their eating habits, used simplified representations of the diet estimate in public statements advocating dietary change.[46] (These representations typically presented the diet estimate as a fairly solid fact; 35% was a 'large' number,[47] that demanded a response.[48])

Clearly, the discretion experts enjoy when simplifying science is a form of power, useful for influencing downstream audiences, but scientists can also use the notion of distortion to advantage. Especially in science-intensive policy controversies, experts often deploy the notion that scientific knowledge gets distorted as it is popularized. Frequently, scientists blame 'the' media for distorting science,[49] charging that oversimplified reports abound, and that journalists often present tentative claims as solid facts and established facts as mere claims.[50] But journalists are not the only potential targets of attacks built around the notion of 'distortion via popularization'. Scientific experts also can launch them at other scientists, policy-makers and the public. (Also, by using the passive voice, scientists can attack a 'popularized' claim without specifically identifying the actors who 'distort' it.)

The case of the Causes of Cancer illustrates how the notion of distortion via popularization can be used in public debate. As Doll and Peto's numbers grew prominent in the controversies over chemical regulation and dietary change, some scientists began to attack publicly-available versions of them. Consider, for instance, this quotation from a pamphlet published by an advocacy group and written by Michael Pariza,[51] a food scientist and critic of dietary recommendations intended to prevent cancer.

> Recently two eminent epidemiologists estimated that as much as 35 percent of cancer might be related to the diet. [citation to the Causes of Cancer] If true, this would make diet the number one factor in human cancer, since tobacco use was given only a 30 percent 'share' and other possible factors, individually, 10 percent or less.
>
> This estimate has been quoted widely and was even featured in a press release of the Department of Health and Human Services (HHS). [citation to news release] It might be instructive, therefore, to examine what the two epidemiologists had to say about their own estimate.
>
> In the text of the paper [citation to the Causes of Cancer], the authors warn: 'It must be emphasized that the figure chosen is highly speculative and chiefly refers to dietary factors *which are not yet reliably identified*' (emphasis added [and noted as such by Pariza]). [citation to the Causes of Cancer]. . . .
>
> This hardly sounds like the stuff of strong statements, such as the recent HHS estimate that '20,000 lives per year by the turn of the century' could be saved 'through improved diet'. [citation to news release][52]

In this statement, pariza contrasts downstream versions of the diet estimate with the original statements of the 'eminent epidemiologists'. He suggests that Doll and Peto's claims were distorted by HHS and others, who treated the estimate as more certain than its authors had intended, and used it to make bold predictions of benefits from dietary change. The references to a government agency (HHS) and its press release insinuate that policy-makers, on the one hand, and the mass media, on the other, may have contributed to the distortion. The 'eminent epidemiologists' are not blamed; the distortion seems to have occurred when the estimate was 'quoted widely', not before. Pariza thus uses the notion of distortion via popularization to strip popularized versions of the claim (for example, the news release) of scientific authority. This example is by no means unique,[53] but is part of a recurring pattern: in such accounts, the dominant view of popularization is used to 'debunk' 'popularized' claims, while simultaneously reaffirming the authority of genuine scientific knowledge.[54]

Scientific experts (and their agents and assignees) can, of course, invoke the notion of distortion via popularization implicitly in much shorter and more sketchy statements. The terse charge that a news report is 'oversimplified', for example, draws on and evokes the dominant view of popularization, because it implicitly contrasts authentic and sophisticated scientific knowledge with a misleading and vulgar popular representation.[55]

Conclusion

The dominant view of popularization is a serious oversimplification that cannot, on its own terms, provide an adequate model of the process through which scientific knowledge spreads. Its chief limitations stem from an underappreciation of the ambiguity and flexibility of such concepts as 'genuine science', 'popularization', 'appropriate simplification' and 'distortion'. Much social research on popularization and science in the media rests on the assumptions of the dominant view, enquiring into the determinants of the distortion of scientific knowledge in popular accounts.[56] Future research is needed that steps outside the dominant view and asks somewhat different questions: What determines which simplified representations of science get labelled 'distortions' and which get accepted as 'appropriate'? Who deploys which labels and when? How are these labels used during controversies? And what role does the dominant view of popularization play in maintaining hierarchies of expertise?

Adequate models of the processes through which scientific knowledge spreads must explicitly address the fact that the dominant view of popularization has political uses. The dominant view provides a repertoire of conceptual and rhetorical devices for interpreting science to outsiders, and its ambiguities allow individual experts to gerrymander boundaries to suit their strategic purposes. When an expert advances claims, the notion of 'appropriate simplification' can justify much – whether it is ignoring conceptual and empirical problems, using guestimates to summarize complex evidence, or engaging in any of the kinds of simplifying transformations illustrated in Figure 1.[57] When attacking the claims of others, the notion of 'distortion via popularization' can be deployed against any of a spectrum of targets, including other scientists, journalists, officials, and the public. Given this flexibility, it is not surprising that scientists often blame extra-scientific actors, such as the mass media or the public, for distortion, because such attacks are both easier to mount,[58] and are more conducive to solidarity among scientists.[59]

Finally, and most fundamentally, the dominant view shores up the epistemic hierarchy which ranks scientists above such actors as policy-makers, journalists, technical practitioners, historians and sociologists of science, and the public. Because scientific experts are entitled to draw and redraw the boundary between 'appropriate simplification' and 'distortion', non-experts remain forever vulnerable to having their understandings and representations of science derided as 'popularized', and 'distorted' – even if they accurately repeat statements made to them by scientists.

In summary, at a cultural level, the dominant view of popularization grants scientists (and others who derive their authority from science) something akin to the epistemic equivalent of the right to print money. Genuine knowledge, the 'gold standard', is their exclusive preserve. The notion of appropriate simplification authorizes them to issue a common 'currency' – streamlined versions of their knowledge – for circulation among the non-expert community. And the concept of distortion permits them, when it suits their purposes, to identify publicly-available representations of science as 'counterfeit'.

But, despite the depth and sweep of the epistemic authority that the dominant view of popularization grants scientists at a cultural level, there are obviously practical limits to scientists' ability to control public discourse about science. To a large extent, these limits stem from two features of social structure. First, there is no equivalent of a 'central bank': scientists are not organized into a single, monolithic hierarchy, but are divided on many issues. Thus, one expert's 'appropriate simplification' may be another's 'distortion'. Second, there is no institutionalized 'police force' for detecting and sanctioning counterfeit claims. Scientists are busy people, and they are few in number in comparison with the volume of science-based information that circulates in our technological society. For these reasons, the epistemic authority that the dominant view of popularization grants to scientists can be translated only into partial control over the presentation of science to wider audiences.

• NOTES

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1. For a description and critique of the dominant view, see R. Whitley, 'Knowledge Producers and Knowledge Acquirers: Popularisation as a Relation Between Scientific Fields and Their Publics', in T. Shinn and Whitley (eds), *Expository Science: Forms and Functions of Popularisation, Sociology of the Sciences Yearbook*, Vol. 4 (Dordrecht & Boston, MA: Reidel, 1985), 3–28.

2. J. Green, 'Media Sensationalisation and Science: The Case of the Criminal Chromosome', in Shinn & Whitley, op. cit. note 1, 139–61. Green argues that the myth 'popularization equals pollution' is a component of scientists' ideologies that contributes to their collective solidarity; ibid., 158.

3. For a discussion of scientists' tense relations with the press, see D. Nelkin, *Selling Science: How the Press Covers Science and Technology* (New York: W. H. Freeman, 1987), esp. 154–69. See J. Burnham, *How Superstition Won and Science Lost* (New Brunswick, NJ: Rutgers, 1987), for a recent history of popularization in the United States.

4. In the dominant view, 'appropriate simplification' distils the essence of the knowledge, presenting it in a succinct and usable form that is appropriate for the audience. 'Distortion' adulterates the knowledge, contaminating it with untruths or oversimplifying it beyond recognition and rendering it useless – or worse. However, even in the case of 'appropriate simplification', the popularized knowledge is altered and loses some of the richness, detail and purity of the original.

5. For a discussion of boundary-work, see T. F. Gieryn, 'Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists', *American Sociological Review*, Vol. 48 (1983), 781–95; Gieryn, G. F. Bevins and S. G. Zehr, 'The Professionalization of American Scientists', ibid., Vol. 50 (1985), 392–409; and Gieryn and A. E. Figert, 'Scientists Protect Their Cognitive Authority: The Status Degradation Ceremony of Sir Cyril Burt', in G. Bohme and N. Stehr (eds), *The Knowledge Society: The Growing Impact of Scientific Knowledge on Social Relations* (London & Boston, MA: Reidel, 1986), 67–86; see also S. Jasanoff, 'Contested Boundaries in Policy-Relevant Science', *Social Studies of Science*, Vol. 17 (1987), 195–230.

6. This flexibility is similar to what Molotch and Boden have termed the 'third face of power'; see H. L. Molotch and D. Boden, 'Talking Social Structure: Discourse, Domination and the Watergate Hearings', *American Sociological Review*, Vol. 50 (1985), 273–88.

7. For a more detailed case study of the Causes of Cancer, see S. Hilgartner, 'A War of Numbers: The Creation and Spread of Statistics on Cancer Etiology', unpublished mimeo.

8. R. Doll and R. Peto, 'The Causes of Cancer: Quantitative Estimates of Avoidable Risks of Cancer in the United States Today', *Journal of The National Cancer Institute*, Vol. 66, No. 6 (1981), 1192–308.

9. Ibid., 1196.

10. The paper was also reprinted as an Oxford University Press paperback; the figure 490 includes citations to either the paper or the book.

11. For example, the paper served as a background paper to a report of the Office of Technology Assessment; see US Congress, Office of Technology Assessment, *Assessment of Technologies for Determining Cancer Risks from the Environment* (Washington, DC: Office of Technology Assessment, 1981). This report developed a similar set of numbers to those of Doll and Peto, although its estimate for the percentage of cancer attributable to occupational factors was much higher than was Doll and Peto's.

12. See, for example: National Cancer Institute, *National Cancer Program: 1983–1984, Director's Report and Annual Plan FY 1986–1990* (Washington, DC: US Department of Health and Human Services, Public Health Service, National Institutes of Health, NIH

Publication No. 86–2765, November 1985), 3, 16, 63, 65; and National Cancer Institute, *Cancer Control Objectives for the Nation: 1985–2000* (Washington, DC: US Department of Health and Human Services, Public Health Service, National Institutes of Health, NCI Monographs, 1986, No. 2, NIH Publication No. 86–2880), 4–5.

13. For example, Doll and Peto's estimate for the percentage of cancers attributable to diet appeared in many American magazines, including *Consumers' Research, Cosmopolitan, Current Health, Discover, Forecast for Home Economics, Glamour, Interview, Mademoiselle, McCall's, Newsweek, Organic Gardening, People, Reader's Digest, Runner's World* and *Science '84*. The diet estimate also appeared in the *New York Times* and the *Washington Post*, and on television.

14. See, for example, P. Huber, 'Safety and the Second Best: The Hazards of Public Risk Management in the Courts', *Columbia Law Review*, Vol. 85 (1985), 277–337; and R. A. Merrill, 'Reducing Diet-Induced Cancer Through Federal Regulation: Opportunities and Obstacles', *Vanderbilt Law Review*, Vol. 38 (1985), 513–38.

15. National Cancer Institute, *Diet Nutrition & Cancer Prevention: A Guide to Food Choices* (Washington, DC: US Department of Health and Human Services, Public Health Service, National Institutes of Health, NIH Publication No. 85–2711, November 1984), 5.

16. The estimate for diet, for example, appeared in M. Pariza, 'Diet and Cancer' (a Report by the American Council on Science and Health, pamphlet, February 1985), 27; 'US Institute Latest to Stress Connection Between Diet and Cancer', *Nutrition Action* (May 1984), 3; 'Preventing Cancer', ibid. (July/August 1987), 4; and Council for Agricultural Science and Technology (CAST), 'Diet and Health' (Report No. 111, March 1987), 38.

17. Doll & Peto, op. cit. note 8, 1196, 1219–20.

18. Ibid., 1196.

19. Ibid., 1196.

20. Once again, as one moves away from the research front, science appears increasingly as a producer of certainty. As Collins puts it, *ceteris paribus*, 'distance lends enchantment'; see H. M. Collins, 'Certainty and the Public Understanding of Science: Science on Television', *Social Studies of Science*, Vol. 17 (1987), 689–713.

21. Doll & Peto, op. cit. note 8, 1235.

22. Ibid., 1258.

23. See the collection of papers in Shinn & Whitley (eds), op. cit. note 1.

24. Whitley, op. cit. note 1.

25. L. Fleck, *Genesis and Development of a Scientific Fact* (Chicago, IL: The University of Chicago Press, 1979), 112–13; J. Bunders and R. Whitley, 'Popularisation Within the Sciences: The Purposes and Consequences of Inter-Specialist Communication', in Shinn & Whitley (eds), op. cit. note 1, 61–77; Whitley, op. cit. note 1; E. Yoxen, 'Speaking Out About Competition: An Essay on "The Double Helix" as Popularisation', in Shinn & Whitley (eds), op. cit. note 1, 163–81.

26. S. L. Star, 'Simplification in Scientific Work: An Example from Neuroscience Research', *Social Studies of Science*, Vol. 13 (1983), 205–28.

27. Ibid.; Whitley, op. cit. note 1.

28. B. Latour and S. Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton, NJ: Princeton University Press, 2nd edn, 1986); Latour, *Science in Action* (Cambridge, MA: Harvard University Press, 1987).

29. As Latour puts it: 'The fate of facts and machines is in later users' hands'; Latour (1987), ibid., 259, and also Chapter 1. Those users are by no means restricted to scientists; ibid., Chapter 4.

30. On laboratory 'shop talk', see M. Lynch, *Art and Artifact in Laboratory Science:*

A Study of Shop Work and Shop Talk in a Research Laboratory (London: Routledge & Kegan Paul, 1985); and C. Amman and K. Knorr-Cetina, 'Thinking Through Talk: An Ethnographic Study of a Molecular Biology Laboratory', paper presented at the 1988 Symposium on Science Communication, Annenberg School of Communications (Los Angeles, California, 15–17 December 1988).

31. H. M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Beverly Hills, CA: Sage, 1985).

32. The phrase is from a 'how-to' book by an American marine biologist, C. J. Sinderman, *Survival Strategies for New Scientists* (New York: Plenum, 1987), 107–08.

33. Latour & Woolgar, op. cit. note 28.

34. In editorials published in scientific journals, the Causes of Cancer has been lauded as a 'monumental paper' (for example, D. Smithers, 'On Some General Concepts in Oncology with Special Reference to Hodgkin's Disease', *Journal of Radiation Oncology, Biology, Physics*, Vol. 9, No. 5 [1983], 732), a 'monumental study' (for example, W. F. Sunderman, 'Cancer in Perspective', *Annals of Clinical and Laboratory Science*, Vol. 13, No. 3 [1983], 170), and a 'remarkable review' (A. I. Mendeloff, 'Appraisal of "Diet, Nutrition, and Cancer" ', *American Journal of Clinical Nutrition*, Vol. 37 [March 1983], 495).

35. Such a view would be more consistent with recent findings of sociology of scientific knowledge than with the dominant view of popularization.

36. See, for example, Fleck, op. cit. note 25, 111.

37. Transformations can stem from at least four, somewhat overlapping, reasons. First, they can result from mistakes, as in typographical errors, unintentional misquotations, and so on. Second, they may result from the use of (implicit, explicit or *ad hoc*) 'editing rules'. These might include notions of clear and vigorous writing, rules for rounding numbers, official 'styles' or 'formats' of publications, or beliefs about the need to paraphrase other people's statements to avoid plagiarism. Third, they may result from efforts to economize on scarce resources, such as space in publications or the time, patience, and tolerance for complexity of the audience (see S. Hilgartner and C. L. Bosk, 'The Rise and Fall of Social Problems: A Public Arena Model', *American Journal of Sociology*, Vol. 94, No. 1 [July 1988], 53–78). Fourth, they can result from efforts to cultivate an image of authority, to be persuasive, or to build or challenge the factual status of a claim.

38. Doll & Peto, op. cit. note 8, 1196.

39. Ibid., 1258.

40. Ibid., 1235.

41. Ibid., 1196.

42. My argument, of course, is not that the concepts of appropriate simplification and distortion are never of any use, but that the question of when each should be applied is at times subject to multiple interpretations.

43. See M. Callon, J. Law and A. Rip (eds), *Mapping the Dynamics of Science and Technology* (London: Macmillian, 1986), and Latour (1987), op. cit. note 28, for a discussion of the importance of using texts to enrol allies.

44. For a journalistic account that discusses the vulnerability of investors to the representations of scientists, see R. Teitelman, *Gene Dreams: Wall Street, Academia, and the Rise of Biotechnology* (New York: Basic Books, 1989).

45. See, for example, the article by Peter Huber, a critic of the American legal system's approach to risk, op. cit. note 14. Huber presented a version of Doll and Peto's table that included the estimates (without the ranges) listed under the heading 'Percent of Total US Cancers'. (Huber, who has a PhD in nuclear engineering from the Massachusetts Institute of Technology and a JD from Harvard Law School, is a good example of a 'hyphenated

scientist': a scientist–engineer–lawyer–political operative.)

46. See, for example, National Cancer Institute, op. cit. note 15, 5; and Department of Health and Human Services, 'HHS News' (Washington, DC: US Department of Health and Human Services, Public Health Service, National Institutes of Health, National Cancer Institute, News Release, 6 March 1980).

47. One physician, active in promoting public health recommendations, told me that when she presented the Doll and Peto diet estimate, she did not like to use the range of 10–70%. The reason, she said, was that people were looking for excuses not to make necessary dietary changes, and she feared they would latch on to the 10% figure, which sounds rather 'small', as justification for continuing to eat badly.

48. For a discussion of another 'large' number with 'important' implications, see J. R. Gusfield, *The Culture of Public Problems* (Chicago, IL: The University of Chicago Press, 1981), 55.

49. The quotation marks are intended to call into question the idea that the mass media is a single, monolithic entity.

50. Nelkin, op. cit. note 3.

51. Pariza, op. cit. note 16. Pariza has a PhD in microbiology from the University of Kansas.

52. A virtually identical statement appeared in a report by CAST, of which Pariza was a principal author: see CAST, op. cit. note 16.

53. When commenting on Doll and Peto's estimates, other authors have made statements of a similar form; for a similar comment on the diet estimate, see D. Kritchevsky, 'Nutrition and Cancer: Introduction', *Proceedings of the Society for Experimental Biology and Medicine*, Vol. 183 (1986), 279. In an editorial that commented on Doll and Peto's estimate for occupational exposures, O. M. Jensen and E. Lynge, in 'The Contribution of Epidemiology to the Study of Occupational Cancer', *Journal of Cancer Research and Clinical Oncology*, Vol. 108 (1984), 257–63, also chose not to blame Doll and Peto, but to blame those who quoted the estimate for making it appear unduly factual. After giving Doll and Peto's estimate, they wrote: 'Such estimates have been widely quoted in recent years. Although the value of such summary estimates should not be disregarded for the purpose of health education, and fund raising, and should be recognized in that they contribute to the establishment of national public health policies, their limitations should be recognized' (259). Jensen and Lynge went on to discuss some limitations (interpretation problems and the issue of interacting risks). They then commented: 'Although the difficulties have been clearly recognized and pointed out by the scientists [who created the estimates]..., the limitations of such estimates are frequently disregarded' (259). Obviously, in Jensen and Lynge's view, it is the users of the estimates who have distorted their meaning.

54. When scientists charge that a simplified representation is a 'popularized' 'distortion' they simultaneously discredit it and blame contingent factors 'outside' science (such as the media or the public) for creating the erroneous information: see G. N. Gilbert and M. Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse* (Cambridge: Cambridge University Press, 1984), for a discussion of the contingent repertoire.

55. Although the examples I have given show how the dominant view of popularization can be used to attack publicly-available versions of the content of science, it can also be used to attack 'distorted' 'popular' views of the conduct of science. In the current debate over scientific fraud and misconduct, for example, scientists have repeatedly suggested that the public, the press, and the Congress misunderstand scientific practice. Once again,

the argument goes, scientists' truths (this time about themselves) are distorted as they spread to outsiders.

56. See, for example, J. R. Cole, 'Dietary Cholesterol and Heart Disease: The Construction of a Scientific "Fact" ', in H. J. O'Gorman (ed.), *Surveying Social Life: Papers in Honor of Herbert H. Hyman* (Middletown, CT: Wesleyan University Press, 1988), 437–66. Cole proposes 'a research agenda for understanding distortions of health risks in the media'.

57. Science journalists and policy-makers can also take advantage of this notion. But they remain quite vulnerable to charges by experts that their simplifications are 'distortions'.

58. That is, it requires fewer resources to challenge media sensationalization than to attack the claims of eminent researchers. See Latour's general discussion of the resources required to challenge claims; Latour (1987), op. cit. note 28, Chapters 1 and 2.

59. Green, op. cit. note 2. Pariza, for example, can blame those who quoted the estimate, such as the authors of the HHS news release, rather than the 'eminent epidemiologists', for distortion.

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