Letters

Defining Consumer Deception

I was shocked and chagrined to read R. Jeffrey Smith's article (News and Comment, 24 Dec., p. 1289) questioning my attempts to institute more *scientific* standards for regulation here at the Federal Trade Commission (FTC).

Surely readers would not deny the need to base regulatory action upon good, scientific evidence. The questions, then, are how much and what kind. Congress and the courts have clearly told us (*1*) that the FTC has relied on too little scientific evidence in the past. Moreover, in the ad substantiation area, the FTC has too often been unwilling to evaluate scientific evidence presented by advertisers. And, unlike the Food and Drug Administration, the FTC has no current protocols as to what constitutes appropriate tests for claims made. Unfortunately, some have jumped to the erroneous conclusion that my attempt to answer these questions is evidence that the Reagan Administration believes that advertisers should not be held to any standard.

Smith says that, I, "in particular," dislike "the existing statutory ban on 'deceptive acts or practices.' " As I have indicated on many occasions, I do not object to the statutory ban, but I would like the term deception clearly defined. This is one of those issues where everyone knows what the term means, but it means different things to different people. A review of the FTC's decisions in this area constitutes ample evidence on this point.

Smith characterizes me and my colleague, Timothy Muris, as "champions of the business community's right to free speech." Yes, we are in favor of free speech, but we also support truth in advertising and oppose attempts by the advertising community to be excluded from the FTC's jurisdiction over "unfair acts or practices."

Smith implies that the advertising industry is opposed to my initiatives. The fact is that all three advertising trade associations have endorsed the need to define deception and also my proposed investigation of the FTC's various procedures for dealing with ad substantiation.

While on the one hand condemning my proposals for seeming to rely less on scientific evidence, Smith later reports Commissioners Clanton and Pertschuk as saying that my proposals would require extensive consumer surveys and other evidentiary bases for action. One obviously can't have it both ways.

Truthful advertising is an emotional issue for many people and a goal most support. But how best to achieve that goal is something over which reasonable people can disagree. Public debate over the issue is important, but it will not be helped by articles such as Smith's. The readers of *Science* deserve better.

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Reference

1. See, for example, the congressional restrictions imposed in the FTC Improvements Act of 1980 (PL 69-252 96th Congress, 2nd sess., 1980) and contained in last year's (unsuccessful) reauthorization bills (H.R. 6995 and S. 2499, 97th Congress, 2nd sess., 1982). With respect to the courts, see my remarks before the Virginia Bar Association, Virginia Beach, 19 June 1982.

The FTC Improvements Act of 1980 contains no criticism of the agency's policy on false or deceptive advertising. In fact, the legislative history explicitly states Congress' belief that "the advertising substantiation program has been an important element of the Commission's effort to police the marketplace and protect consumers."

With regard to Miller's disapproval of "the existing statutory ban on deceptive acts or practices," my article clearly stated that he favored a different definition and was not proposing to omit the requirement entirely.

As to the comment about "having it both ways," there is no contradiction. As the article noted, Miller's proposal would clearly shift the burden of proof from industry to the agency's Bureau of Consumer Protection. Less evidence would be needed to substantiate advertisements, while more would be required to substantiate regulatory enforcement.—R. JEFFREY SMITH

Byssinosis Research

We object to Eric Frumin's description (Letters, 28 Jan., p. 340) of our published findings (*1*) regarding the respiratory health of cotton textile workers, particularly what we said about the "mill effect." We also object to his discussion of our testimony at the Occupational Safety and Health Administration (OSHA) hearing on the proposed cotton dust standard.

Frumin quotes parts of a consultant's report written by John M. Peters (*2*) for the Amalgamated Clothing and Textile Union about the "mill effect" observed in the study of byssinosis performed by researchers at Tulane University (*1*). The "mill effect" represents unexplained variability in byssinosis prevalence between mills *after* accounting for variation resulting from the level of exposure, smoking, job category, and length of employment.

Peters purports to eliminate the "mill effect" by using four categories of exposure rather than the three we used, saying that the use of three exposure categories masked the effect of high exposure in one of the mills. His analysis shows a dose-response relationship when byssinosis prevalences are collapsed across mills. We also reported a dose-response relationship when three categories of exposure are used (*1*). There is, however, significant variability between mills, after accounting for dose, when either three or four categories are used. The "mill effect" thus persists in spite of the analysis by Peters.

That there is variability in responses to cotton dust exposure between mills not explained by other measured factors should not be particularly surprising and has been accepted by the National Institute of Occupational Safety and Health in their studies of exposure to nontextile cotton (*3*). Mill variability may result from qualitative or quantitative differences in cotton contaminants, unaccounted-for personal factors in the host, or levels of past dust exposure. There may be other explanations.

Because there may be information in the "mill effect," denying its existence may hinder the search for the unknown agent(s) and mechanism(s) responsible for byssinosis. It is only after they have been identified that environmental controls protecting all workers can be instituted. Until that time, dust levels should be controlled. The Tulane study suggests that a suitable exposure limit lies in the range between 200 and 500 micrograms per cubic meter. A range was given because physiological measurements

suggest that 200 $\mu g/m^3$ is appropriate and byssinosis symptoms suggest 500 $\mu g/m^3$.

Turning to the testimony of one of us (H.W.) at the OSHA hearing in 1977, our research findings, as summarized above, demonstrate a range of dust levels within which a prudent permissible cotton dust exposure limit should be established. This range reflects the scientific uncertainties that frequently must be accepted in public policy decision-making. Indicating that this range is 200 to 500 $\mu g/m^3$ does *not* say that the upper value should be used to promulgate the standard, nor that the lower level is inappropriate; we have said neither, then or since. Quite to the contrary, we have supported the standard and have never suggested a more permissive revision, a fact recently reported in an editorial by an investigator (*4*) whose data and conclusions are heavily supported by Frumin.

The following excerpts from H.W.'s testimony, which Frumin does not quote, conflict with his characterization of this testimony (*5*):

> There is a dose-response relationship between exposure to cotton dust and a biologic effect, measured either with the questionnaire or pulmonary function testing. (page 314)

> I strongly favor the prevention of the demonstrated reduction in air flow as measured spirometrically or obtained through the questionnaire responses. Because, ultimately, [by] preventing the bronchial constrictive effect, I think we will probably prevent the chronic disease. (page 317)

> Assuming that there is a relationship between the acute and chronic response, and I have no reason to believe there is not, then the detection of such a response or reaction is obviously highly desirable, because it allows an individual to modify his or her exposure in order to effect prevention of further changes. (pages 363–364)

> I don't know how to assess the contribution, say, of cigarette smoking in an individual like that and dust exposure, and I think probably, as has been done in other industries, the presumption may have to be made that that exposure may have played a role and, therefore, further exposure is contraindicated. (page 387)

> I've said throughout that the dust effect is real and it's there, but in addition to a dust effect there is also an influence exerted by something else which we're not measuring having to do with mill. . . . (page 432)

Neither science or occupational health benefits from misleading descriptions of research findings or publicly stated views on public policy issues.

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References and Notes

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2. J. M. Peters, personal communication to George Perkel, 8 June 1977 (OSHA Hearing Record, Exhibit No. 143-A, 1977), p. 7.
3. A. Engelberg *et al.*, "Characterization of byssinosis and other pulmonary abnormalities in the cotton waste utilization industry" (Paper submitted to the Department of Labor by the National Institute of Occupational Safety and Health, Department of Health and Human Services, Morgantown, W.Va., November 1981).
4. J. A. Merchant, *Am. J. Public Health* **73**, 137 (1983).
5. H. Weill, in "Transcript of informal public hearing on proposed standard for exposure to cotton dust" (OSHA Hearing Record, Washington, D.C., 6 April 1977), pp. 295–434.

Mexican Food System

In this day and age events sometimes move faster than editors. The Mexican Food System (SAM) mentioned in John Walsh's article on Mexican agriculture (News and Comment, 18 Feb., p. 825) was in fact abandoned in January as a part of the Mexican government's budget cutting.

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The Long Wave

David Dickson's discussion of the economic long wave or Kondratiev cycle (News and Comment, 25 Feb., p. 933) provides a good summary of the innovation theories of the long wave that have emerged since the stagnation of the late 1970's. There is no doubt that renewed emphasis on basic research and R & D are important ingredients for economic revitalization. However, vigorous stimulus of research is not a sufficient response to the current crisis.

Since 1975 the System Dynamics National Model (*1*) has provided an increasingly rich theory of the economic long wave. As mentioned in the article, the theory is not based solely on innovation but integrates many factors hypothesized by others as the prime mover in the long wave, including innovation, labor dynamics, price movements, financial and monetary policy, capital accumulation, international trade, and even political value shifts (*2*). The core of the theory, however, is the idea of capital "self-ordering"—the fact that in the aggregate, the capital-producing sector of the economy must order capital plant and equipment from itself in order to increase capacity. To illustrate, consider the situation after World War II: the nation's capital stock and infrastructure were old and severely depleted after 10 years of depression and roughly 5 years of wartime production. As the demand for consumer goods, services, and housing rose, manufacturers of capital plant and equipment had to expand their own capacity, further swelling the demand for structures, equipment, materials, transportation networks, and other infrastructure, and also boosting wages, encouraging more capital-intensive technologies. This self-reinforcing feedback stimulated further expansion of investment and started the boom of the 1950's and 1960's. In order to both satisfy long-run demand and rebuild the capital and infrastructure, the capital-producing sector had to expand beyond the long-run needs of the economy. By the late 1960's, the capital stock had been largely rebuilt, and investment began to slow to a level consistent with replacement and long-run growth. Excess capacity and unemployment began to show up in basic industries. Faced with excess capacity, investment in these basic industries was cut back, further reducing the need for capital and reinforcing the decline in investment as the economy moved through the 1970's and into the 1980's. Because physical capacity and infrastructure are quite long-lived, the excess capacity developed in the long-wave expansion continues to depress investment (and hence aggregate demand) long after output falls.

Simple formal models incorporating the self-ordering mechanism and rational decision rules for managing investment and production can generate the long wave without any technological change (*3*). As discussed by Forrester (*4*) and Graham and Senge (*5*), commitment to the existing technological base depresses innovation during the expansion phase, but during the downturn, as the old infrastructure is written off the books and physically depreciates, new technologies become more attractive. Thus in contrast to the "innovation hypothesis of the long wave" favored by the neo-Schumpeterian school, the national model suggests that a "long-wave hypothesis of innovation" better describes the situation.

Freeman and other long-wave theorists are correct that the current depression is not an ordinary trough in the business cycle. But while stimulating basic research and training the labor force for "new-wave" technologies are important, innovation alone will not be sufficient to lift the economy into a sustained recovery as long as excess capacity in basic industries continues to depress investment (*6*). Indeed, in the early years of the high-tech boom here in Massachusetts, many companies chose to rehabilitate unused mill and warehouse space rather than build new. Only after the stock of excess space was drawn down did construction of new facilities catch up to the expansion of the high-tech industries. The Japanese policy of planned capacity reductions and worker retraining in the aluminum and shipbuilding industries (*7*) provides an example of an effective response to excess physical capacity.

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2. For a comprehensive review of long-wave theories and evidence both old and new, see J. J. Van Duijn, *The Long Wave in Economic Life* (Allen & Unwin, London, 1983); *Futures* **13** (No. 4) (1981); *ibid.* (No. 5), pp. 347–388; P. M. Senge, "The long wave: A survey of evidence" (Working Paper D-3262-2, System Dynamics Group, Massachusetts Institute of Technology, Cambridge, 1983).
3. J. D. Sterman, "A simple model of the economic long wave" (Working Paper D-3410, System Dynamics Group, Massachusetts Institute of Technology, Cambridge, 1983).
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5. A. K. Graham and P. M. Senge, *Technol. Forecast. Soc. Change* **17,** 283 (1980).
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Lake Erie, Not Lake Huron

Does the recent closing of the University of Michigan's Department of Geography have anything to do with what appears to be Lake Erie on your cover of 25 February being designated as Lake Huron?

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Landsat 1 makes vegetation look red and Lake Huron look eerie.

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What led you to such a perception?
An eerie misrecollection?
Or was it a twisted neuron
Which caused you to call it Lake Huron?

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