I scarcely think your readers will adopt the assumption that this promotion is misleading "since its purpose is to sell goods rather than to inform the doctor."

Admittedly, our perspective, as members of an industry operating in a business environment, is bound to be weighted in favor of our vigorous—and we think generally efficient—methods of promotion. But, for all its weaknesses, we believe our system brings prompt and thorough information to the physician and allows him the maximum opportunity, at greatest personal convenience, to be well informed on the range of medicines our industry has placed at his disposal.

Finally, you raise a fundamental question about the appropriateness of allowing the drug industry to function within the structure of the competitive enterprise system. One's immediate reaction to this is to ask, why then should food, water, shelter—the things that affect not merely health but sheer survival—be supplied under the stimulus of the profit system? Cannot business incentive serve health equally well? I submit that our industry has served the public interest as well as or better than any other I know of.

I recognize that the social, economic, and political issues involved cannot be dismissed casually, but I worry about a philosophy that seems to suggest that health be "quarantined" into the government domain.

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The "solid results" referred to were the indications that the climate of opinion brought about by the investigations had put the Food and Drug Administration in a position to initiate reforms which they had felt were advisable but which for years had been difficult to achieve because there had been little public interest in what the FDA wanted to do. One example is the recent tightening of regulations governing the information that must be included in drug promotion pieces. Another is the FDA proposal for a new factory inspection law.—ED.

Cannibalism

I was quite interested in Jay Boyd Best's recent article "Diurnal cycles and cannibalism in Planaria" (*1*) but question the use of *cannibalism* to describe the behavior observed. Webster's *New Collegiate Dictionary* defines a cannibal as "a human being that eats human flesh; hence, any animal that devours its own kind." Recent notes in ornithological journals report incidences

of one bird killing and eating another of a different species (*2*). If these instances are accepted as cannibalism, should eating of song birds by avian predators such as hawks be termed cannibalism also? Some species, such as the blue jay (*Cyanocitta cristata*), which are not ordinarily predators will kill and eat young of other species (*3*). Stewart (*4*) describes as cannibalism the eating by one captive tufted titmouse (*Parus bicolor*) of another killed by a rat; yet this probably would not have occurred in free-living individuals.

Scavengers and carrion feeders will often eat dead members of their own species (I have observed this in gulls). Cannibalism is also said to occur in those colony-nesting gulls which kill and eat the young of neighboring nests (*5*), in storks and other species which eat their own young ("kronism") (*6*), and in owls in which siblings apparently eat one another (*7*). These latter types of "cannibalism" may function in population dynamics as "feedback mechanisms" to adjust the number of the year's offspring to the available food supply.

It seems to me that to call these (and other) kinds of behavior "cannibalism" directs attention away from analysis of the true adaptive significances of the observed phenomena. I suggest that the term *cannibalism* be restricted to improbable occurrences of adult animals feeding on other adults of the same species in the wild.

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References and Notes

1. *Science* **131**, 1884 (1960).
2. See article and citations in J. K. Terres, *Auk* **73**, 289 (1956) and R. H. Backus, *ibid.* **71**, 471 (1954).
3. See article and citations in D. H. Lamore, *Wilson Bull.* **70**, 96 (1958).
4. *Auk* **72**, 83 (1955).
5. N. Tinbergen, *Herring Gull's World* (1953).
6. E. Schüz, *Die Vogelwarte* **19**, 1 (1957).
7. I. Collingwood, *Auk* **76**, 222 (1959); **77**, 256 (1960).

Jack Hailman is to be thanked for his contribution to linguistic precision. I stand corrected. On the other hand, the restricted definition proposed as a substitute for Webster's does not strike me as particularly useful. Of far richer import are Hailman's remarks hinting at what he believes to be the biological significance of cannibalistic (in Webster's sense) behavior. It is to this more beguiling problem that I would like to address the remainder of this letter.

The most interesting thing about cannibalism (in Webster's sense) is not that it occurs but rather why it does not occur more often than it does. By what cues, stimulus patterns, and behavioral restraint mechanisms is an animal that normally preys voraciously upon a wide spectrum of other species more or less inhibited from preying on its own kind? As a working hypothesis it would seem far more parsimonious to consider cannibalism to be an extension of an animal's normal spectrum of prey to include members of its own species than to consider it a specialized adaptive mechanism for adjusting population to food supply. Cannibalism could be expected, in the former view, to ensue whenever the hunger "drive" overrode the psychological mechanisms normally prohibiting predatory behavior directed against the animal's own species. That it may also function as a feedback mechanism in population dynamics, as suggested by Hailman, seems more or less incidental. Let us for convenience refer to Hailman's view as *A* and to the view which I choose to espouse as *B*.

Some patterns of attack specificity were observed among the planarians described in my report in *Science*. The immature *Dugesia tigrina* used in the experiments were kept in colonies of approximately 30 per bowl during the time they were being fasted. It was found that feeding reduced the incidence of attack of *D. tigrina* upon *Cura foremani* to zero. If cannibalism (in Webster's sense) had been common

among the *D. tigrina*, they would have been sated at the time of testing and attacks upon the *C. foremani* would not have been observed. Thus, the probability of predatory attack of immature *D. tigrina* upon *C. foremani* must be much greater than the probability of attack upon each other. Nor were *C. foremani* observed to attack each other. Yet both species will attack and eat a wide variety of other small organisms—for example, *Tubifex* worms, mosquito wrigglers, and brine shrimp—and large, sexually mature *D. tigrina* will attack and eat smaller immature *D. tigrina*. Thus, even among the planarians, which are about the most primitive animals possessing a real central nervous system, there is considerable specificity in regard to predatory behavior directed against close relations. The cues inhibiting or releasing such behavior in planarians are still largely unknown.

Hypotheses *A* and *B* imply different sets of consequences, and these consequences could be used to distinguish which of the hypotheses is valid. To decide between them one might ask the following questions.

1) Within the set of animals having the physical capacity to kill and eat a member of their own species, is the incidence of cannibalism in a species positively related to the diversity of other species preyed upon by the members of the species in question? Hypothesis *A* would imply no such relation; *B* would. Consider Hailman's own citation of the fact that owl siblings kill and eat one another. Apparently siblings of less voracious predators do not. Why? As a teleological mechanism to adjust population to food supply, cannibalism should be equally useful for all species.

2) Is cannibalism as frequent among predators and omnivores that hunt and travel in packs as it is among those that are solitary hunters when the breadth of prey spectrum between the two types is comparable? I do not believe hypothesis *A* would predict any essential difference. Hypothesis *B* would predict a higher probability of cannibalism among the solitary hunters when they are placed together, because group living would favor the evolutionary selection of more powerful inhibitory mechanisms against cannibalism.

3) Among those species which are physically incapable of eating other members of their own species but which do possess the capacity to kill other members of their own species, does one observe an enhanced rate of killing of members of their own species in the absence of an adequate food supply? According to *A* one should, since the advantages of such an action as a population regulator are as effective as cannibalism; *B* would predict that one would not.

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Systematic Collections

Before the regulation on deposition of types proposed by the conferences on systematic collections [A. B. Grobman, *Science* **131**, 938 (1960)] is put into effect, "stable institutions" should be required to obtain from some properly qualified and independent authority a statement certifying just which kinds of organisms now are adequately curated and will be so curated in the future. Duplicating curatorial concentration on certain favored groups is so great as to warrant more than a suspicion that staffs of all such stable institutions in North America, even if combined, would not be in a position to look after specimens of every sort of extant organism. Institutions desiring certification also should be required to prove that they know how to retain specimens in a condition to function with some degree of usefulness as types, or else that they are en-