Meetings

Forthcoming Events

October


13-15. Optical Soc. of America, Boston, Mass. (M. E. Warga, OSA, 1155 16 St., NW, Washington 6)


15. American Soc. of Safety Engineers, annual, Chicago, Ill. (A. C. Blackman, ASSE, 5 N. Wabash Ave., Chicago 2)

15-16. American Acad. of Psychotherapists, 5th annual conf., Cleveland, Ohio. (B. J. Barkley, 1856 Coventry Rd., Cleveland Heights 18, Ohio)


16. American College of Dentists, Los Angeles, Calif. (O. W. Brandhorst, 4236 Lindell Blvd., St. Louis 8, Mo.)

16-22. High-Speed Photography, 5th intern. congr., Washington, D.C. (W. H. Allen, Soc. of Motion Picture and Television Engineers, 55 W. 42 St., New York 36)


17-20. American Dental Assoc., Los Angeles, Calif. (H. Hillenbrand, ADA, 222 E. Superior St., Chicago, Ill.)


17-22. Diagnosis and Treatment of Acute Radiation Injury, Geneva, Switzerland. (World Health Organization, Palais de Nations, Geneva)

17-26. Plastics Processing, intern. congr. and exhibition, Amsterdam and Utrecht, Netherlands. (Secretariat, c/o N. V. 't Raadthuys, Tesselchadestraat 5, Amsterdam-W, Netherlands)

18. Oak Ridge Inst. of Nuclear Studies, Oak Ridge, Tenn. (W. G. Pollard, Box 117, Oak Ridge)

18-20. Mathematical Optimization Techniques, symp., Berkeley, Calif. (R. M. Oliver, Dept. of Industrial Engineering, Univ. of California, Berkeley 4)

18-21. American Dietetic Assoc., 43rd annual, Cleveland, Ohio. (M. L. Ross, Simmons College, The Fenway, Boston 15, Mass.)


19-21. Design of Experiments, 6th conf. (by invitation only), Aberdeen Proving Ground, Md. (F. G. Dressel, Office of Ordnance Research, Box CM, Duke Station, Durham, N.C.)

19-21. Spatial Agitation, symp., Columbus, Ohio. (Institute of Radio Engineers, 1 E. 79 St., New York 21)


20-22. Acoustical Soc. of America, San Francisco, Calif. (V. Salmon, Stanford Research Institute, Menlo Park, Calif.)


21-22. Association of Midwestern College Biology Teachers, 4th annual, Mankato, Minn. (L. Zell, Mankato State College, Mankato, Minn.)

21-22. Research Approaches to Psychiatric Problems, symp., Galesburg, Ill. (T. T. Tourlentos, Galesburg State Research Hospital, Galesburg)

21-23. American College of Cardiology, St. Louis, Mo. (F. Greco, 114-08 Lincoln Blvd., Ozone Park 16, N.Y.)

21-25. American Heart Assoc., annual, St. Louis, Mo. (AHA, 44 E. 23 St., New York 10)

22. Midwest Solid State Conf., 8th annual, Lincoln, Neb. (J. W. Weymouth, Physics Dept., Univ. of Nebraska, Lincoln)


24-26. Medical and Biological Aspects of the Energies of Space, symp. (School of Aviation Medicine, USAF Aerospace Medical Center, San Antonio, Tex.) (J. Harmon, Symposium Coordinator, Southwest Research Inst., E.O. Box 2296, San Antonio 6)

24-27. Hot Atom Effects, symp., Prague, Czechoslovakia. (International Atomic Energy Agency, 11 Kärntner Ring, Vienna 1, Austria)


26-28. Animal Care Panel, 11th annual, St. Louis, Mo. (ACP, P.O. Box 299, Lemont, Ill.)


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science vol. 132
27-29. American Soc. for Aesthetics, Brooklyn, N.Y. (J. R. Johnson, Cleveland Museum of Art, Cleveland 6, Ohio)
27-29. International Assoc. of Milk and Food Sanitarians, Chicago, Ill. (V. T. Foley, Kansas City, Missouri, Health Dept., 21st Floor, City Hall, Kansas City, Mo.)
31-2. Electrical Techniques in Medicine and Biology, 13th annual conf., Washington, D.C. (G. N. Webb, Room 547, CSB, Johns Hopkins Hospital, Baltimore 5, Md.)
31-2. Geological Soc. of America, Denver, Colo. (F. Betz, Jr., 419 W. 117 St., New York 27)

November
1-16. International Electrochemical Commission, New Delhi, India. (American Standards Assn., 70 E. 45 St., New York 17)
2-4. Plasma Physics, 2nd annual, Gatlinburg, Tenn. (A. H. Snell, Oak Ridge Natl. Lab., Oak Ridge, Tenn.)
2-4. Society for Experimental Stress Analysis, Berkeley, Calif. (W. W. Murray, Massachusetts Inst. of Technology, Cambridge)
2-5. American Soc. of Parasitologists, Los Angeles, Calif. (F. J. Kruidenier, Zoology Dept., Univ. of Illinois, Urbana)
2-5. American Soc. of Tropical Medicine and Hygiene, Los Angeles, Calif. (R. B. Hill, 3573 St. Gaudens Rd., Miami 3, Fla.)
3-4. Muscle as a Tissue, conf., Philadelphia, Pa. (Division of Research, Lankenau Hospital, Philadelphia 31)

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24–26. Cent. Assoc. of Science and Mathematics Teachers, 60th annual conv., Detroit, Mich. (L. A. Conrey, School of Education, Univ. of Michigan, Ann Arbor)


25–26. National Council for Geographic Education, Cincinnati, Ohio. (L. Kenndmer, Univ. of Texas, Austin)

25–16. Bahamas Medical Conf., Nassau. (B. L. Frank, P.O. Box 4037, Fort Lauderdale, Fla.)

27–1. Latin American Cong. of Neurology, Santiago, Chile. (R. Nunez, Almirante Montt 485, Dep. 11, Santiago, Chile)


27–5. International Federation of Agricultural Producers, 11th conf., New Delhi, India. (IFAP, 1624 Eye St., NW, Washington 6)

28–1. Entomological Soc. of America, Atlantic City, N.J. (R. H. Nelson, 4603 Calvert Rd., College Park, Md.)
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- ANALOG-TO-DIGITAL CONVERTER is a solid-state instrument with a speed of 10,000 conversions per second at accuracy said to be ±0.05 percent. Display is either 8-bit binary or 3-digit binary-coded decimal plus sign. Four decimal digits and sign can also be obtained. Input impedance is 10,000 ohm/volt. (Packard Bell Electronics, Dept. Sci779, 12333 W. Olympic Blvd., Los Angeles 64, Calif.)

- SILICON CARBIDE rectifiers are said to withstand temperatures of 500°C and to be one-tenth as subject to radiation damage as silicon. Typical reverse currents are less than 10mA at 50 volts at 400°C. At 500°C maximum average forward current is 100 ma and maximum peak inverse voltage is 50 or 100 volts. (Transitron Electronic Corp., Dept. Sci780, 168 Albion St., Wakefield, Mass.)

- OHM-METER measures deviation of resistance from a selected nominal value and displays the result in digital form as a percentage of the nominal value. A range of ±5 percent of total resistance is measured with absolute accuracy said to be ±0.01 percent, the 5-percent range being measured with 3-digit accuracy. Average balancing time is 1 sec. (Electro Instruments Inc., Dept. Sci781, 3540 Aero Court, San Diego 11, Calif.)

- INFRARED TRANSMITTING GLASS will transmit 77 percent of infrared radiation at 4.0 μ and 38 percent at 5.5 μ in a thickness of 2 mm. Transmission loss at 600°C is 13 percent at 5.5 μ and zero at 4.0 μ. Various shapes and sizes can be mass produced and given an optical finish with standard grinding and polishing techniques. (Corning Glass Works, Dept. Sci789, Corning, N.Y.)

- VOLTAGE SAMPLER MODULE is designed to follow a rapidly changing voltage wave. It is functionally equivalent to an input operational amplifier connected through a switch to an RC filter and followed by a low impedance output amplifier. With the switch closed, the output follows the input in about the same fashion as a 2-μsec delay network. When the switch opens, the voltage is held within 0.05 percent for 1 μsec, long enough to be converted to digital form by the manufacturer's converter. (Adage, Dept. Sci791, 292 Main St., Cambridge 42, Mass.)

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ELECTRONIC COMMUTATORS are solid-state devices that can sample low-level differential instruments such as strain gages and thermocouples at a rate up to 10,000/sec and accuracy said to be ±1 percent or better of full scale at levels of 5 mv or greater. The units contain up to 90 channels. A 30-channel unit weighs 2.5 oz and has less than 1 in.$^2$ volume per channel. (General Devices Inc., Dept. Sci803, P.O. Box 253, Princeton, N.J.)

RUGGEDIZED TELEVISION SYSTEM consists of a camera and control capable of being operated up to 2000 ft apart. No additional housing is required for the camera to operate in all environments in which the equipment could be used. The system will operate at temperatures from $-20^\circ$ to $+160^\circ$ F, in ambient noise levels of more than 185 db, and with no limit on altitude. The system has transistor circuitry exclusively. An automatic light-level compensator adjusts for light-level changes over a 2000-to-1 range. A wide variety of accessories is available. (Thompson Ramo Wooldridge Inc., Dept. Sci810, Michigan City, Ind.)

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Thermosat Baths, one for thermometer calibration and another for viscometry, have transparent sides. Temperature range is up to 230°C. A circulating plate is said to permit control of temperature fluctuations in the working space within ±0.001°C for water and ±0.005°C for oil. An auxiliary thermostat keeps the emergent stem of the contact thermometer at constant temperature and permits micrometric adjustment of temperature. The units are constructed of stainless steel with transparent areas 22.5 by 10.5 in. and 12.5 by 10.5 in., respectively, for the thermometer calibration bath and the viscometer bath. (Instrumentenfabriek P.M., Dept. Sci816, Tamson N.V., The Hague, Netherlands)

Automatic Plotter has a working area that measures 47.25 by 47.25 in. Maximum operating speed is in 3 in./sec, and slowest operating speed is 0.003 in./sec. Accuracy is said to be ±0.0015 in. Input may be in the form of tape or cards. Position of X and Y rails is shown by a projection-type display. An optical line-following device is offered as an accessory. A Flexowriter is used to provide keyboard input, punching of tape, reading of tape, and typing of printed record. (Aero Service Corp., Dept. Sci806, 210 East Courtland St., Philadelphia 20, Pa.)

Stretchable Cable is constructed with a rubber core with insulation of nylon and conductors of copper wire, bronze tinsel, or copper tinsel. A natural rubber core is used in commercial types; military types have a silicone rubber core. Elongation of the former is 300 percent for single-conductor cable and 275 percent for multiple-conductor cable. Corresponding elongations of the military types are 250 and 225 percent. (National Radio Co., Dept. Sci807, Malden, Mass.)

Leveling Base for a variety of instruments uses three leveling screws mounted at the corners of a triangular platform. A circular level, mounted at one corner, guides adjustment. (W. & L. E. Gurley, Dept. Sci826, Troy, N.Y.)

Pressure Transducer uses as sensitive element a free-edged circular diaphragm of Ni-Span-C that forms the movable common plate between two capacitor plates. Pressure change is measured by change in capacitance. Hysteresis effects are said to be less than 0.3 percent and sensitivity better than 0.01 percent of full scale. Full scale ranges are available from 1 to 100 lb/in.² First resonance of an alimeter using the transducer is greater than 1650 cy/sec, and time constant is 0.004

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- **CLINOMETER ACCESSORY** that adapts a precision clinometer for use in a vertical position consists of a special base, with adjustments for making the axis of rotation of the circle normal to the supporting surface, and a 4.75-in.-diameter work table for measurement of angular displacement of small parts. The clinometer is available in 10-sec and 1-sec sensitivities. (Engis Equipment Co., Dept. Sci805, 431 South Dearborn St., Chicago 5, III.)

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23 SEPTEMBER 1960

JOSHUA STERN
National Bureau of Standards,
Washington, D.C.
Letters

The Drug Industry

Because so much of a superficial nature has been written as a consequence of the Senate's hearings on the drug industry, I have been particularly interested in the somewhat more thoughtful analyses prepared by Science. Your article last April [Science 131, 1299 (1960)] showed a strong effort to maintain complete objectivity. But I have been puzzling for some time now over your report of 16 June, under the heading "The drug inquiry: a curious affair that has netted some solid results" [Science 131, 1794 (1960)]. In brief, I find this report disappointing.

In the first place, you use the phrase "whatever complaints can be made about the style of the investigation." Covered by this brief, qualifying phrase is a perfectly outrageous demonstration of misleading, headline-hunting tactics. By implication, you take issue with such tactics.

However, you immediately go on to state that the "solid results" have "for the most part developed out of the publicity given the hearings, which the hearings won, in part, through the way in which they were conducted." Thus, you seem to endorse the use of questionable means to reach an end. And I cannot see that publicity is responsible for any results except unfair and misleading ones.

What do you call "solid results"? It seems clear to us that you are saying just this: after months of thin pickings, in which the drug industry could be pilloried only by misleading headlines, the Senate subcommittee came up with solid results when it turned its sights onto government agencies. Let me make it clear that if this is the real implication of what you are saying, I certainly do not espouse it, for I cannot condone the defamation of a fine agency such as the Food and Drug Administration on the basis of one or two incidents. If, on the other hand, you are saying that these "solid results" have come in regard to our industry, I can only ask, what are you referring to?

May I also refer to your comments on the "high-powered promotion race." While no one can reasonably deny the extent of this competitive manifestation, I think that most of your readers who are familiar with our industry will disagree with you that "there is pressure on every company to match the promotional effort of the least responsible companies." Likewise, I am confident that your readers will reject the generalization that "much of [this barrage of promotion] tends to be misleading." And, in the same sentence,
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 incentives 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.

G. F. ROLL
Smith Kline and French Laboratories, Philadelphia, Pennsylvania

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 predation on different species by birds. This might lead to confusion if the term cannibalism is used indiscriminately.

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 incentives 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.

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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.

Jack P. Hailman
6037 Lake Terrace Circle,
Norfolk, Virginia

References and Notes
7. J. 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 foremanii to zero. If cannibalism (in Webster's sense) had been common.

![AT LAST! A SYRINGE THAT IS GAS TIGHT](image)
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.

JAY B. BEST
Department of Neurophysiology,
Walter Reed Army Medical Center,
Washington, D.C.

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This new form of aid to the education of scholars and scientists seems to be a natural extension of the role of philanthropic foundations. Continuing a tradition of specialization, and in order to provide some measure of long-range effectiveness, a foundation interested in such a program might select a definite field of knowledge in which to help books reach their destined readers. Large foundations might choose large fields: virology, nutrition, cellular biology, linguistics. Less wealthy foundations could establish their identity by supporting more circumscribed fields: psychology of perception, history of biology, philosophy of science.

Financial aid in the publication of scholarly books is not a new idea, but heretofore such help has not been notably effective in making books cheaper. What is needed is some form of aid that would let a $6 book sell for $3, a $10 book for $4; perhaps no scholarly book should cost an interested individual more than $5 or $6. The selling price ought to be a test of serious interest and not a test of ability to pay. Anybody willing to spend $5 on a book on the chemistry of amino acids, or on comparative linguistics, deserves to have the book.

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free enterprise in the production and selling of books; our scientific publish-
ers and booksellers are doing a fine job, and no one would want them disturbed. In fact, the type of program suggested here would strengthen the industry and the trade. The system should enable an individual to get the books he wants without much ado; there should be no test of the "need to have" or "need to know." To a layman in these matters, for booksellers simply to bill a given foundation for part of the price of each book sold from among those it supports would seem an acceptable solution; experts probably could think out something better, even if less simple. Public libraries, universities, and business firms would continue to pay the full price of supported books. The distribution of journals could be similarly helped; some of them, especially the ones in newer fields, are frightfully expensive. There is precedent for a journal's selling for less to individual subscribers than to organizations: subscription rates for the Philosophical Review of Cornell University are $3 per year for individuals and $6 per year for institutions. Regrettably this example is not widely followed.

STEVEN E. ROSS
2210 Jackson Street,
San Francisco, California

Science Teaching


In what ways can teachers be equipped to change the pattern of science instruction from one involving the mere cataloging of isolated scientific facts to one that reveals how scientists make use of the power of the human mind to perceive, think about, and eventually integrate seemingly unrelated arrays of impressions into broad conceptual schemes?

With the rate of growth of knowledge constantly increasing, the teacher who tries to present a body of facts (biology, chemistry, physics) without evoking some degree of understanding and appreciation of unifying fundamen-tals is bound to lead his students into a morass of frustrating confusion.

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Pseudo Science and Censorship

The letter by H. C. Dudley on “Pseudo science and censorship” [Science 132, 378 (12 Aug. 1960)] requires additional comment in rebuttal. Dudley makes a plea for fair-mindedness and questions the right of anyone to censor another’s pronouncements as being “preposterous” or “crackpot.”

Although examples may be quoted wherein reputable discoveries were denounced by contemporaries, examples of the reverse are truly legion. For an excellent compendium of wild ideas, crazy machines, crackpot inventions and theories, and general “magic black box” hoaxes, one may well read Fads and Fallacies in the Name of Science, by Martin Gardner (Dover, New York, 1957).

Gardner sets up a rather useful group of ground rules for separating
the honest effort in a new field from the
work of the venial deluder or psycho-
pathic crank. Thus, on pages 12 and 13
he outlines five excellent criteria which
will trip up the crank.

Dudley's sense of fair play is ad-
mirable but does not, apparently, in-
clude the judicious selectivity required
so desperately in our modern society.
So often, today, individuals worry so
about the rights of others and the
problems of the minority that they al-
low the minority to subvert the ideals
and steal the rights of the majority.

One is reminded of the tale of "The
bear who let it alone" in James Thur-
ber's Fables for Our Times. The moral
of the tale is very relevant to condi-
tions today. "You might as well fall flat on
your face as lean too far over back-
wards."

F. W. NORTHLAND
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Starvation with and without

Painful Hunger Pangs

Kelman (1), Wittenberg (2), and Ross
(3) regarded starvation as too painful to
justify its use in studies on animals such
as Denenberg and Karas (4) made. The
gnawing pangs of hunger are commonly
considered to be the most disagreeable
accompaniment of prolonged starvation,
and hunger generally makes even a few
days of fasting difficult. However, Kel-
man, Wittenberg, and Ross apparently
did not take into consideration the fact
that disagreeable hunger sensations usu-
ally decrease or disappear after the first
few days in prolonged starvation with-
out any food. Unfortunately, even
widely accepted authorities like Cannon
and Carlson did not agree about the
manifestation of hunger during pro-
longed starvation, although both regard-
ed the hunger experienced during a few
days of starvation as consisting mainly
of uncomfortable or gnawing sensations
produced by periodic contractions of the
fasting stomach (5, 6). Cannon main-
tained that hunger disappears after the
first few days in prolonged starvation,
while Carlson maintained that hunger
persists. Cannon's view was based on
the reports of others but not on any
study made by him or his students. Car-
son's view was based on a study made
on himself and an assistant while they
fasted about 5 days.

Eight years ago before I learned of
Carlson's view by reading his mono-
graph on hunger (6), I accepted the
explanation of hunger or normal appetite
advanced by Fletcher (7). Fletcher dis-
tinguished between a normal appetite
and a false appetite but not between
hunger and appetite. He considered nor-
mal hunger or appetite to be mainly of

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mental origin, false appetite to be often a manifestation of stomach craving. My observations largely seemed to support Fletcher’s views. In 1913, I fasted 26 days, and my impression was that the reference of hunger to the stomach tended after about the fifth day. It therefore seemed that Carlson and his assistant simply did not fast long enough to note that the reference of hunger to gastric contractions decreases or disappears in prolonged starvation. Moreover, I believed that Carlson (and Cannon’s student, Washburn) had merely found a better explanation of the desire to eat which Fletcher considered to be false appetite. A discussion with Carlson of my experience and findings led to his making a study of hunger on me at the University of Chicago, while I fasted 15 days in the summer of 1917. Actually, observations were made daily before, during, and after this fast and an additional 8-day fast, or during a total of 10 weeks in which I lived in Carlson’s laboratory (8).

The periodic gastric or hunger contractions were found to be manifested throughout the 15-day fast, and the desire to eat was always increased more or less when the gastric contractions occurred. Carlson therefore felt that his previous view concerning the persistence of hunger during prolonged starvation had been supported. However, the increase in my desire to eat with the occurrence of gastric contractions was less after the sixth day of fasting. Carlson attributed this to a decrease in appetite, not to a decrease in hunger, because the records of the gastric contractions showed no change. He had previously explained in the same way a change in sensations noted after the second or third day of his 5-day fast. Neither Carlson nor Cannon ever made a clear distinction between hunger and appetite, and, as indicated in a letter published as an appendix to Carlson’s report (8), I remained unconvinced that the sensations produced by the so-called hunger contractions represented the basic element in normal hunger.

Nevertheless, after my fasting in 1917 I seemed to find a closer relation between the need of food, the desire to eat, and the gastric hunger contractions than I had found before or during the fasting, and I realized that Carlson’s and Cannon’s explanation of hunger would have to be taken into consideration in any attempt to find a better explanation. I therefore gave my epigastric hunger sensations close attention, especially when I made changes in my dietary regimen. Thus, in 1924 I found that the periodic gastric contractions could be felt without any desire to eat whatever, under appropriate nutritional conditions. This was first noted when my stomach became empty early in the evening after I had deliberately eaten a large meal of easily digested and rapidly absorbed food early in the day. Subsequent observations made under less extreme conditions clearly indicated that the basic desire to eat (hunger) is more directly related to a central need of food than to emptiness of the stomach or the hunger contractions (9).

Before the foregoing finding was published, Carlson enabled me to begin a study with the use of facilities at his laboratory to determine whether a relation existed between the secretory activity of the fasting stomach and epigastric hunger sensations. Some of the gastric sensations seemed explainable by simple or spasatic contractions of the muscles of the stomach, but others seemed to involve irritation or burning by acid. Observations on the secretory activity of the fasting stomach were usually made before and after observations were made on the motor activity with the aid of an inflated balloon in the stomach, in 1917. Variations in the gastric acidity were then noted, but no definite relation to the desire to eat or to the hunger contractions was noted. In 1925 I found that by making gastric aspirations every half hour, data were obtainable on both the secretory and the motor activity of the fasting stomach, and an interrelationship between the two functions was revealed (10). By alternately using the aspiration method and the balloon method of study, it became obvious that the gastric hunger contractions were simply the final gastric emptying contractions and that the number of contractions that could be felt was greatly increased by the presence of an inflated balloon in the stomach. There was no evidence that the fasting gastric functioning was increased by the use of an aspiration tube—a conclusion reached after about 500 aspirations; over 3500 additional aspirations were then made. In a study made with Kleiman it was also found that a non-nutritive substitute for food tended to promote the development of hunger contractions instead of serving to prevent them while basal metabolism tests were being made (11). At the same time that I began making observations on the secretory activity of the fasting stomach, in 1925, I began making variations in my protein intake in the hope of discovering, by chance, how protein is craved. My attempts to live on low protein or vegetarian diets between 1908 and 1918 always led to the development of a practically irresistible craving for meat. Raw beef was greatly appreciated under such circumstances and fully satisfied the craving, or more than sat-
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isfled it in a few days. This craving seemed to be an instinctive call for more or better protein, but no explanation of the manner in which the craving was manifested and dispelled suggested itself. However, it soon became evident in 1925 that the gastric acidity of fasting increased with protein restriction and decreased with restoration of protein (12), and when protein restriction was carried to an extreme, the easily recognized protein craving or protein hunger manifested itself and soon became excruciating epigastric hunger pangs (13). These were not directly related to the gastric hunger contractions but took my breath away and were first objectively recorded by disturbing my breathing when a basal metabolism test was being made. The painful sensations seemed to be produced by irritation of the duodenum or parapyloric region by highly acid gastric contents when they were being discharged from the stomach. The duodenum or parapyloric region probably was also made hypersensitive by the protein restriction or protein starvation. At the same time the gastric secretion was evidently kept at a high level through consumption of plenty of low-protein food.

As an alternative to simple protein restriction, I also tried fasting in 1925 to produce protein starvation. My original intention was to fast only 1 or 2 days intermittently, but I took advantage of an opportunity to serve as a subject in a study of the effect and after-effect of prolonged fasting on mental performance (14) and thus undertook my longest fasts, of 33 and 41 days, respectively. Prolonged fasting involves more or less dehydration, partly because of carbohydrate starvation, and the gastric secretion was found to be depressed more or less after the first few days. No epigastric protein hunger pangs were experienced during the prolonged fasts, but displays of fat meat seemed almost irresistible toward the end of the fasts. After the fasts, the gastric acidity became higher than before fasting, and I experienced more or less protein hunger. It also was difficult to restrict the food intake again until after I had become fatter than before fasting. During these long fasts, tests were made to determine the effect of drinking more water than I needed (500 ml). The result was that the gastric acidity became increased after the water left the stomach, and the gastric hunger sensations were also increased (15).

I never tried fasting without drinking water, but Cannon (16) cited the experience of Viterbi, a political prisoner, who kept a diary while he abstained from drinking as well as eating until he died on the 18th day. Apparently, hunger disappeared sooner and more completely after the first two days than would have
been the case had he drunk water, because of the dehydration involved, and death was hastened.

In any case, it seems of interest that the lack of an adequate amount of an essential food like protein is likely to be accompanied by more pain than starvation without any food or water.

FREDERICK HOELZEL
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References
10. ibid. 73, 463 (1925).
11. N. Kleitman, ibid. 77, 233 (1926).
12. F. Hoeltzel, ibid. 77, 166 (1926).

Training of College Teachers

I note in your editorial “Real Professionalism” [Science 132, 439 (19 Aug. 1960)] that the National Education Association is now attempting to impose state licensure and a requirement “to study the theory and practice of education in the course of their professional education” on college and postgraduate educators.

I have long been disenchanted with the training too many of the school educators receive at the “teachers colleges,” whose curriculum is so heavily laced with courses on how to teach but seems so weak in the subjects to be taught. Are we now to inflict the same “uniformity” (or is it mediocrity) on the collegiate instructors?

Perhaps I am too naive in my belief that the two essential characteristics of a good teacher are (i) enthusiasm and (ii) thorough knowledge of and interest in his subject.

JOHN HELWIG
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Brain Dysfunction

A report by Wells and Wolff [Science 131, 1671 (1960)] offers a slim but adequate excuse for me to make some remarks which I have been saving up for some time.

The authors cite a fascinating experi-
ment in conditioning of cerebral responses, finding significant differences between "normals" and persons evidencing severe anxiety. This observation in itself would be of sufficient interest, but the authors have gone on to use the data in a rather backhanded fashion to imply that the differences must be due to brain damage of some sort, hinting that with sufficiently sensitive techniques, they might "demonstrate other evidences of impairment" which are the real, actual, true cause of functional behavior disorders and not some magical kind of "neurosis" or "psychosis" as some people think. Perhaps I am unfair to Wells and Wolff, but I have heard essentially this view expressed by enough members of the working and teaching medical profession to convince me that it is widespread.

In support of this view that brain damage lies behind every psychological malfunction is an impressive array of clinical information demonstrating that psychological malfunctions can be and are caused by certain kinds of brain damage (although brain damage sometimes shows a disconcerting failure to affect behavior at all). There is no evidence at all to show that every functional disorder is the result of structural damage to the brain. The science least likely to resolve this implied problem is the science of electroencephalography.

The electroencephalograph measures the gross electrical activity of the brain, which results from the fields and currents generated in unknown patterns by an unknown set of neurons somewhere near the surface of the brain. The electroencephalograph can thus detect changes in activity of large assemblies of neurons, and because of this and because of the complexity of the signal, can differentiate between grossly different patterns of brain activity. In the same manner, a high-impedance probe inserted into the main frame of a computer could be used to detect (empirically) changes in the routing and number and time patterns of electrical signals; a patient observer might correlate these changes with certain gross malfunctions in the computer and use them for diagnosis, as the electroencephalogram is often used.

Suppose, however, that one day the computer technician attached his probes and recorded the patterns of signals and found some extreme changes which failed to resemble any of the "normal" traces. Should he decree an emergency operation? Perhaps, or perhaps not; he will have to find out first (i) whether there is a mistake in the program so that the computer has started dividing by zero; (ii) whether a new type of program is being used so that the patterns of activity are not typical of the usual sort of program; (iii) whether the machine has been told to do something it cannot do; and (iv) whether any of the other things which can have the observed effect without failure of any part of the machine (and there would be many of them) have happened. True, certain typical disorders are likely to have typical effects on the "electrohardwaregram," but that after one has observed the coincidence enough times he feels confident in making certain diagnoses, but it is entirely possible, if not probable, that merely by programming the machine just so one could simulate a tumor in a transistor or a lesion in a loop.

A medical student once testified to me, after listening to some of my propositions concerning a model for behavioral organization, "But you can't know anything about function without knowing structure!" In the brain, I suspect that the opposite is true: you can't know anything about structure (that is, organization) until you first know function (what the structure does). I suspect that the medical student sometimes uses machines of which he understands the function considerably better than the structure, and I know for fact that even some of the most sophisticated of com-

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* In April 1960 issue of Lab World.
puter users may well consider a study of 
flip-flops, diode storage times, and 
bit density on tape a waste of their 
time, leaving that to those who want 
to build, not use, computers. The best 
way to learn how computers work is 
certainly not to start by studying the 
malleurgy of copper wire, vacuum tube 
or transistor theory, and the quantum 
mechanics of conduction processes. 
One could know all that is to know 
about these “fundamental” aspects of a 
computer and still be completely, or 
least largely, ignorant of the various 
functions which a computer can carry 
out so usefully. One could know all 
there is to know about synaptic processes, 
cell metabolism, and the routing of im-
pulses through the brain and still not 
be able to make heads or tails of what 
the blamed thing is for. Only through 
a thorough analysis of the things which 
people can do, and orderly construction 
of a model representing these functions 
and their interrelationship, can one un-
derstand “brain activity.” Only then can 
one group the various structures he has 
found and has yet to find into meaning-
ful assemblies which tell him anything, 
which bear any comprehensable rela-
tionships to each other. I stood by once 
and heard a neurosurgeon comment to 
a colleague, during a cleaning-up proc-
cess where a brain tumor had been, 
“Look there—that’s all good brain.” 
This might be called the simplest con-
cept of function; as long as there’s 
enough good brain left to secrete be-
havior, you’re OK.

I don’t claim that psychologists or 
cyberneticists have yet published any-
thing that is very helpful in helping us 
understand brain structure, but I will 
assert that no neurologist has ever pub-
lished anything (limited sample admit-
ted) that helps us understand why our 
neighbors’ children are so inferior to 
ours.

Behavioral science and neurology are 
still a long way apart, and the attitude of 
many neurologists toward the psy-
chological approach is no help in getting 
them together. The childlike faith that 
sufficiently fine measurement and suf-
Ficiently thorough chemical analysis 
will explain behavior may sustain one 
through periods when nothing seems to 
explain anything, but it is a faith, not 
a fact, and ought not to be referred to as 
if it were proven and self-evident. The 
 essence of science is measurement, and 
measurements yield numbers; the num-
bers remain numbers until some human 
being fits them into his concept of a 
system which performs certain func-
tions; only then do the numbers mean 
anything at all. And if the human being 
had not started with some guess about 
function, he never would have made a 
meaningful measurement.

I argue, therefore, that not only is
the functional approach essential and prerequisite to understanding of any structure but that any scientist who does more than fill catalogs full of numbers uses the functional approach all the time, willy-nilly. He does so because that is one of the functions which human beings are built to carry out.

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William Powers states a viewpoint which we support and have done much to document. Our only regret is that he should have misinterpreted the statements made in our original communication and have attributed to us a position which we deprecate. Far from implying that the differences in "normal" and "anxious" subjects "must be due to brain damage of some sort," as Powers deduced, we stressed that "studies of the microscopic structure of the nervous system have revealed no significant changes in the brains of persons suffering from the common neuroses and psychoses." The remainder of our communication presented evidence that one might be able to measure electrographically dysfunction in this group of chronically anxious subjects who have no known demonstrable damage to structure. Although Powers purports to find us hinting that structural damage is the cause of neurosis or psychoses, there is no such statement or suggestion anywhere mentioned or implied; nor indeed do we subscribe to such a view.

Our brief report concerned one electrographic method which might be used to evaluate brain function or dysfunction, regardless of the basis of the impairment. We would reaffirm our original statement that "perhaps more sensitive methods of measuring responsiveness in the electroencephalogram may demonstrate other evidence of impairment [of function] in the 'functional' disorders of the brain."

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Conversions

Pembroke J. Hart in his letter on conversions (Science 132, 256 (22 July 1960)) uses a conversion factor of 1.1516 statute miles per nautical mile. This is the factor given in most current reference works, yet since 1 July 1954 the Department of Commerce and the Department of Defense have been using the international nautical mile, defined as exactly 1852 meters, for which the conversion factor is 1.1508 statute miles per nautical mile. Prior to 1 July 1954 the United States used a nautical mile of 6080.20 feet (1853.248 meters). The international nautical mile at the time of its adoption by the United States was equivalent to 6076.1033 U.S. feet, but effective 1 July 1959 the United States adopted the international yard, equivalent to 0.9144 meter. Therefore the international nautical mile is now equivalent to 6076.11549 international feet.

It is apparent that the term nautical mile is ambiguous and, when encountered in a scientific paper, is difficult to interpret. As Hart points out, conversions in the metric system are much simpler.

I suggest that the use of nautical mile be restricted to air and surface navigation, where it has real value, and that metric distance units be used in space flight and rocketry.

WILLIAM H. ALLEN
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Washington, D.C.

Pembroke J. Hart, in his letter on conversions, evidently used for his conversion the "old" nautical mile, which for the United States was 6080.20 feet and for the British, 6080.0 feet. The former would give his ratio of 1.1516 (1.15155).

The "new" nautical mile or international nautical mile, as defined by the International Hydrographic Bureau, was adopted by the United States on 1 July 1954; this length is 6076.1033 feet (7), and the ratio is 1.1507575.

H. ROBERT DURSCH
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Reference


P. J. Hart's letter complaining about two instances of imprecise conversions from metric to English units is meant to point up one of the advantages of converting to the metric (decimal) system universally. I wish to use it for a different lesson. We should rid ourselves of the pedants who translate a news item about a 4540-kilogram spaceship into 10,009 pounds, as well as the squares who round off the conversion factor and come out with 9988 pounds. I don't have access to the original report but suppose that it came out of the U.S.S.R. as a news item, not as a scientific datum. The aim was to command admiration, not to provide a basis for computing the burning time of the rocket motor. Rendered into English (U.S.), the weight of the satellite, as a news item, is 5 tons.

Now for the generalizations: (1) "If you wish to have what you say remembered, put it in the words that your
Humane Treatment of Animals

The bill S. 3570 recently introduced into the Senate by Senator Cooper and others, "To provide for the humane treatment of animals . . ." has been strongly attacked both in Science [132, 7 (1960)] and in the Bulletin of the National Society for Medical Research. These attacks have given what I think to be a false idea of the nature and intent of the bill, and of the motives of its sponsors, and prompt me to make a carefully considered statement of my own opinion.

The issue of humane treatment itself is a moral one: To what extent are we justified in inflicting pain and discomfort on other organisms in our search for knowledge? Bill S. 3570 takes the position "that living vertebrate animals used for scientific experiments shall be spared unnecessary pain and fear; that they shall be used only when no other feasible and satisfactory methods can be used to ascertain biological and scientific information for the cure of disease, alleviation of suffering, prolongation of life, or for military requirements; and that all such animals shall be comfortably housed, well fed, and humanely handled." This is a statement with which, I think, most biologists would agree in principle; personally I should feel more comfortable if the words potentially valuable were inserted after the words scientific information, but I think that the efforts of the National Society for Medical Research, the Animal Care Panel, and the American Physiological Society over the past several years have been directed toward the general aims stated above.

The second issue posed by the bill is a practical political one: Granted that humane treatment is desirable, is legislation, and in particular this legislation, the best means to assure it? The alternatives would seem to be voluntary action by the investigators or local control by individual communities. The charges recently brought against Stanford University and the College of Medical Evangelists in California show that local action under the influence of extremist pressure groups may still endanger medical research; it seems probable that the existence of federal legislation of the type proposed in S. 3570 would do much to protect laboratories against this sort of local attack. The question of voluntary action is a more debatable one. In my own experience I have never come across an instance of wanton cruelty to experimental animals, but I have encountered numerous cases of neglect due to callousness, inadequate facilities, inexperience, or carelessness; again, it would seem that S. 3570 would help to eliminate such instances.

The reasonable objections which have been made to the specific provisions of S. 3570 are well summarized in the Science editorial: "Advance approval of experimental plans by the Department of Health, Education, and Welfare, burdensome record keeping, annual or more frequent reports to HEW, additional costs . . . and a new and unnecessary amount of red tape." As I read the bill, it seems to me that the requirements are not greatly beyond those now in force. Every application for federal research funds requires submission of an experimental plan which is approved by a panel of scientists. I hope that all of us who publish results of animal experiments do at least the amount of record keeping specified by the bill. Every federal research grant now requires an annual report. The only additional features are that the experimental plan must specify what animals are to be used and what
type of experiments are to be performed; there is nothing in the bill requiring advance approval of every minor change in experimental procedure. The report, also, must specify the animals used and the procedures employed, but there is nothing in the bill to say that this must coincide exactly with the plan proposed. Compliance with the provisions of the bill will cost more, insofar as the existing laboratories do not provide adequate facilities for the animals used, but this should result in better experimental results as well as more humane care.

The National Society for Medical Research has devoted much attention to the provision for inspection of facilities and for certificates of compliance with regulations to be laid down by the Secretary of Health, Education, and Welfare; this is presumably the red tape with which Science is concerned. At present, every institution receiving grants from federal agencies is visited—or if you wish, inspected—by officers of those agencies. On the basis of past experience, I think that we have nothing to fear from these officers, who have abundantly demonstrated that their main aim is to further research of the highest quality. Any regulations which HEW might lay down under an act of the sort proposed would, I think, not depart from this aim. In any event, the bill gives no police powers to HEW or anyone else, so that work sponsored by any but federal agencies would not be in any way affected.

In sum, I cannot find in this bill the evils which the National Society for Medical Research or Science profess to see, and I would urge my colleagues who are interested in animal experimentation, humane treatment, or both, to read the bill with care, to make their own appraisals on the basis of their own judgments, and to communicate these judgments to their representatives in the Congress.

Bradley T. Scheer
College of Liberal Arts,
University of Oregon, Eugene

I read with considerable interest your article on the Cooper bill, S. 3570 [Science 131, 1659 (1960)]. It seems a holy and ennobling thought that our animal friends should be generously accorded some measure of protection from our mighty and benevolent government. Your article, however, referred simply to experimental "animals"; I should hope that the sponsors of this bill were more explicit in their definition of the organisms to be protected. Do they include viviparous mammals only? mammals only? warm-blooded vertebrates only? vertebrates only? chordates only? vertebrates and invertebrates, including protista and bacteria? vertebrates and invertebrates,
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including all protista and bacteria which are not primarily or facultatively photosynthetic? Surely our senators will not endorse the statements of a mammologist acquaintance of mine: "If it isn't warm, it's dead," or, in his more extreme moments, "If it doesn't have red blood, it's a plant."

In a democratic society, it does not seem an unreasonable analogy to suggest that all organisms (or at least non-photosynthetic and/or nonparasitic organisms, though such exceptions might hint of an occupational elite) are created equal. If the criterion for inclusion under the bill is to be a phylogenetic one, it would seem somewhat arbitrary to propose protection up to and including tenth cousins, three times removed, and imply that tenth cousins, four times removed, are beyond the pale, and that eleventh cousins are of course unworthy.

I am confident that herpetologists and ichthyologists, entomologists and protozoologists would strongly protest any regulation which would not afford the full measure of governmental safeguards to the organisms of their interest. Such a slight would imply that these animals are, because of their immediate ancestry, unworthy of the full concern and protection of the government. I myself would emphatically oppose any such derogatory implication about the crustacea, which are my current interest. Such prejudgment, based on uncontrollable accidents of birth and ancestry, seems contrary to the American spirit of recognition of individual abilities and character.

As a further extension of this bill, it seems wholly logical that if a housewife (whose husband is at least partially dependent on federal money) plans to trap an intruding mouse (Mus musculus), her premises and the proposed trapping device should be inspected and certified for compliance, and that she be then licensed as an authorized animal experimenter. Then, of course, the Secretary of Animal Health, Education, and Welfare would have to review the proposal in detail. Likewise, any insecticide should receive the seal of approval from the SPCA—or its governmental equivalent—as the certified, least painful means of attaining the desired goal. Undoubtedly, properly motivated congressmen will see the myriad other logical, and thus necessary, extensions of the Cooper bill: animal husbandry, commercial fisheries, hunting and fishing as sports, to mention only a few of the more obvious.

J. T. ENRIGHT

3256 Luna Avenue,
San Diego, California

The Cooper bill (S. 3570) is concerned with "live vertebrate animals used for scientific experiments."—Ed.
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