SCIENCE

19 November 1976

Volume 194, No. 4267

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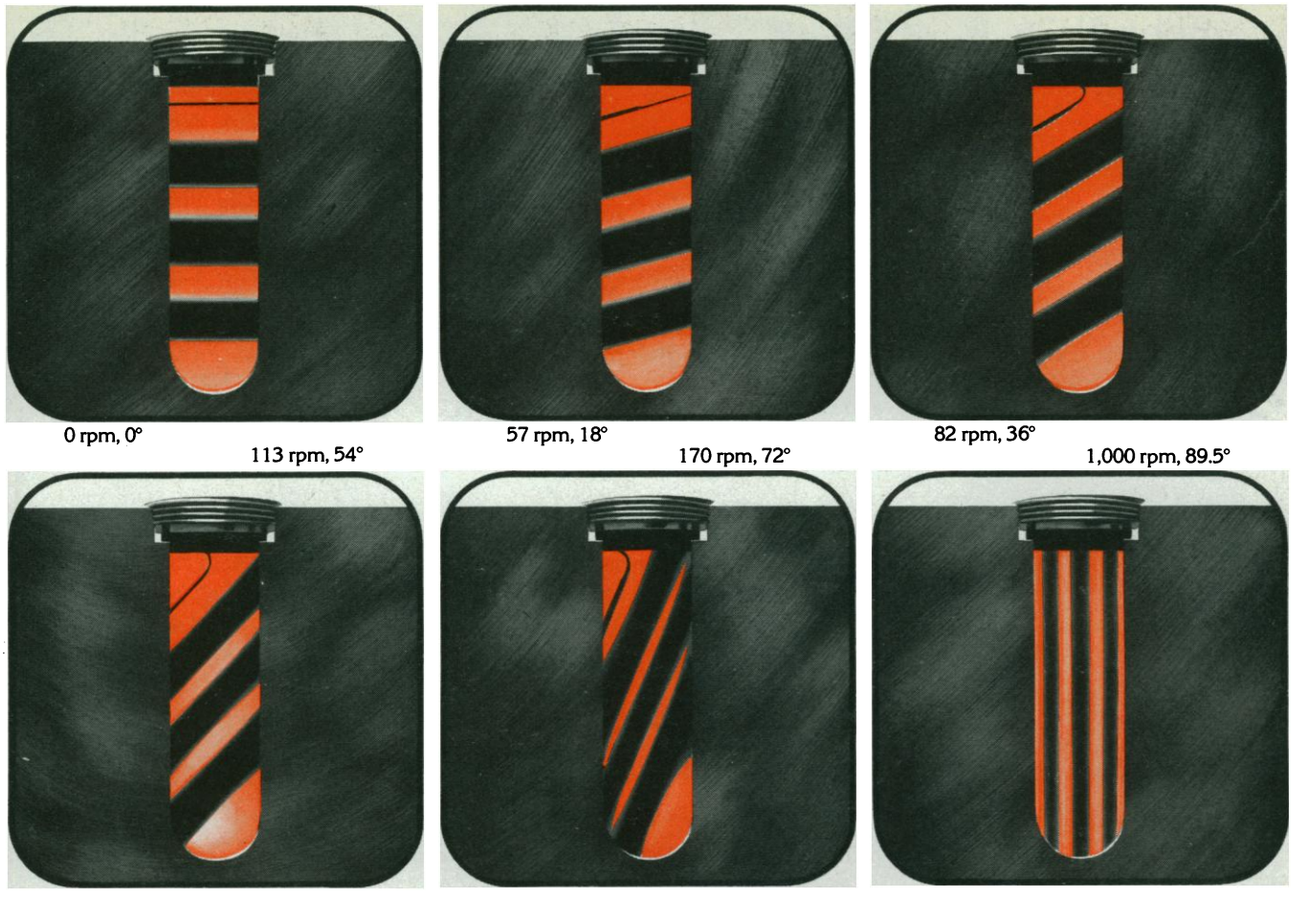

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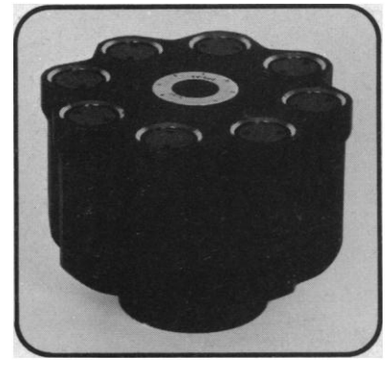

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COVER

Luxuriant bed of *Potamogeton robbinsii* at a depth of 5 meters observed during the winter ice cover in Lake George, New York. Plants are approximately 0.3 meter in length. See page 841. [Richard Sheldon and Tim Seamans, Rensselaer Polytechnic Institute, Troy, New York]

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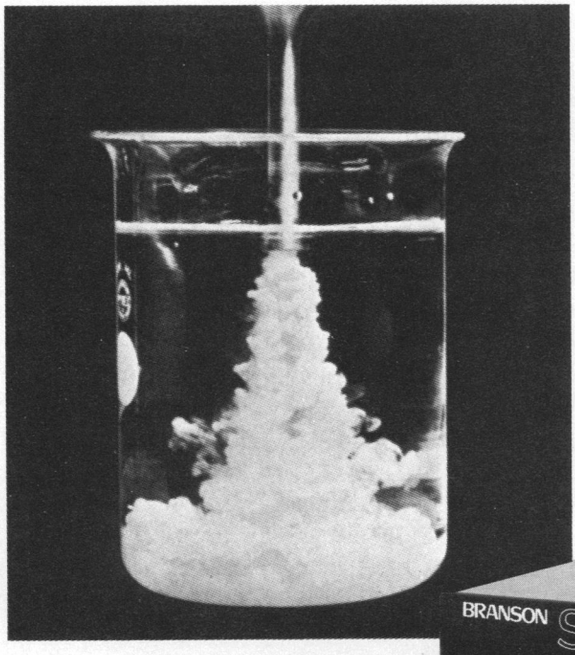

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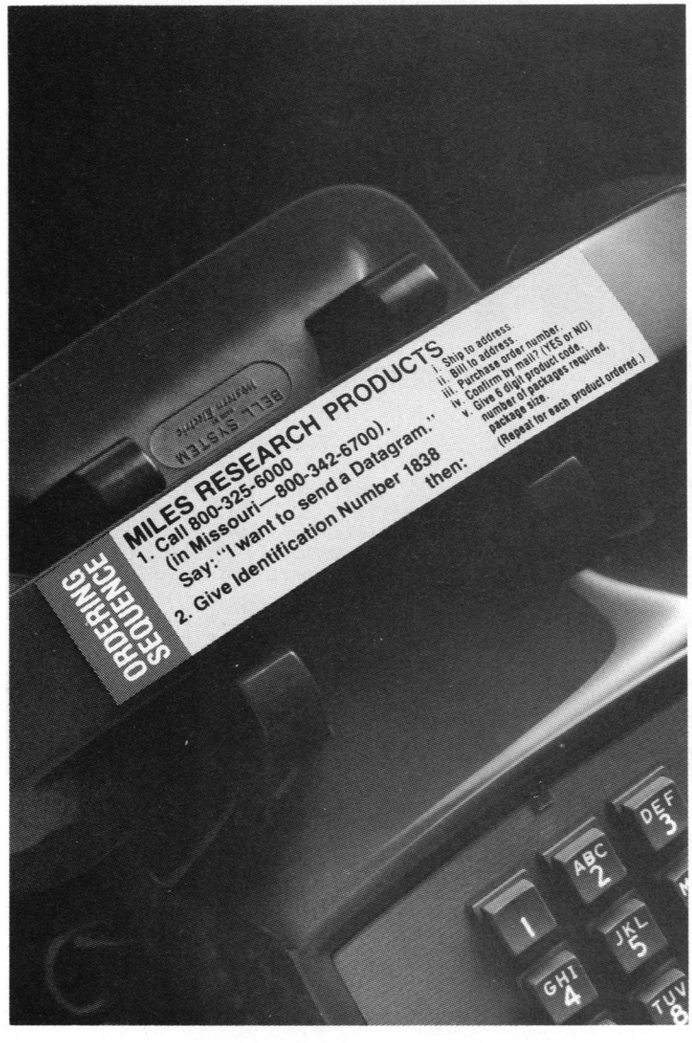

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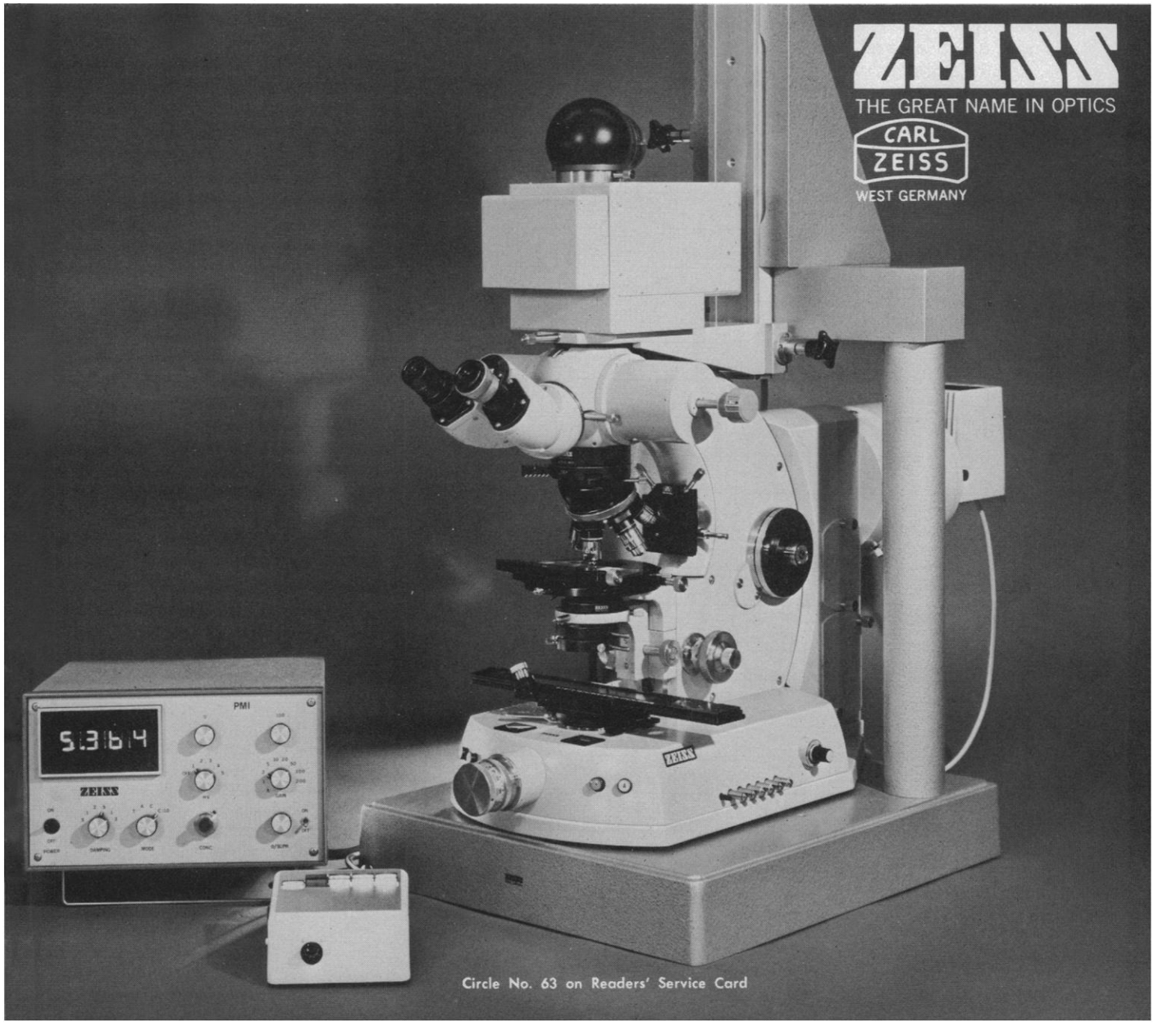

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The AAAS–Newcomb Cleveland Prize, which previously honored research papers presented at AAAS annual meetings, will henceforth be awarded annually to the author of an outstanding paper published from September through August in the Reports section of *Science*. The first competition year under the new rules starts with the 3 September 1976 issue of *Science* and ends with that of 26 August 1977. The value of the prize has been raised from $2000 to $5000; the winner also receives a bronze medal.

To be eligible, a paper must be a first-time presentation (other than to a departmental seminar or colloquium) of previously unpublished results of the author's own research. Reference to pertinent earlier work by the author may be included to give perspective.

Throughout the year, readers are invited to nominate papers appearing in the Reports section. Nominations must be typed, and the following information provided: the title of the paper, issue in which it was published, author's name, and a brief statement of justification for nomination. Nominations should be submitted to AAAS–Newcomb Cleveland Prize, AAAS, 1515 Massachusetts Avenue, NW, Washington, D.C. 20005. Final selection will rest with a panel of distinguished scientists appointed by the Board of Directors.

The award will be presented at a session of the annual meeting at which the winner will be invited to present a scientific paper reviewing the field related to the prize-winning research. The review paper will subsequently be published in *Science*. In cases of multiple authorship, the prize will be divided equally between or among the authors; the senior author will be invited to speak at the annual meeting.

Deadline for nominations: postmarked 15 September 1977

Nomination Form
AAAS–Newcomb Cleveland Prize

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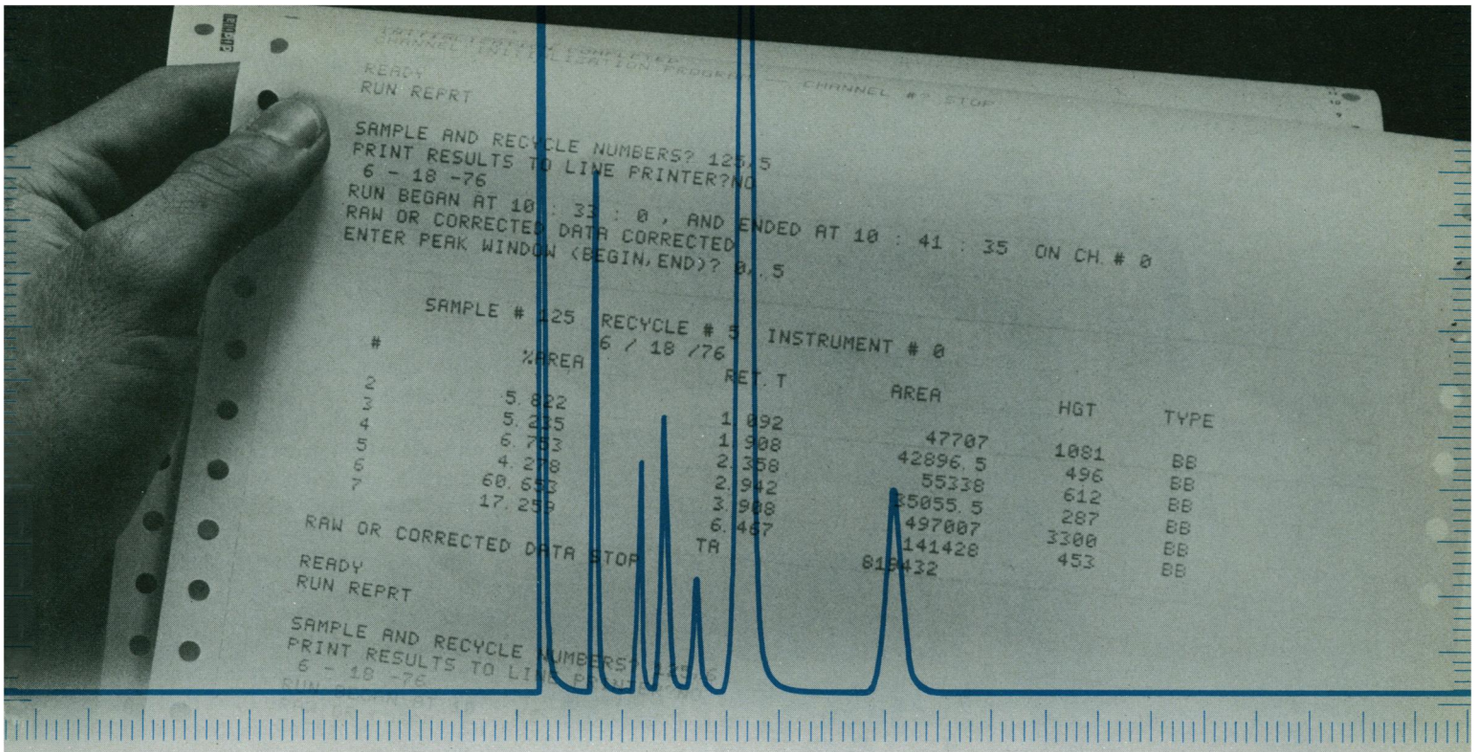

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S-106

LETTERS

Animal Welfare and Scientific Research

Nicholas Wade's article "Animal rights: NIH cat sex study brings grief to New York museum" (News and Comment, 8 Oct., p. 162) requires a response because of its profound significance for science, and also because it is necessary to correct several substantive statements. Wade states, for example, that when the National Institutes of Health (NIH) asked the American Museum of Natural History for a special review of its animal welfare procedures, "instead of getting an independent committee to write them a clean bill of health," the Museum "turned to a group which consisted only of people with ties to the Museum. . . ." Wade fails to say that, in constituting this committee, the Museum followed *exactly* the instructions of NIH. This procedure, which the Museum has followed regularly and for many years, was clearly approved in a special report prepared by the NIH Animal Welfare Officer, Roy Kinard. Further, in response to a request by the Museum administration, William A. Sadler, chief of the Population and Reproduction Grants Branch of the National Institute of Child Health and Human Development, appointed an ad hoc committee consisting of Sadler and two leading research veterinarians, one from Harvard University and the other from the Oak Ridge National Laboratory. A detailed report of this committee was inserted by New York Congressman Edward Koch into the *Congressional Record* of 24 August 1976 which gave unqualified approval to all of our research procedures.

Wade describes one of the leaders of the actions against our institution, Henry Spira, as ". . . not an all-the-way antivivisectionist." In Spira's writings, repeated references are made to "mutilations" and "butchering" by the "greedy vivisectors" of "millions" of "defenseless animals." Spira mentions specifically rats, mice, hamsters, rabbits, guinea pigs, birds, monkeys, and innocent beings, that are "being driven insane, suffocated, poisoned, battered, radiated, crushed, blinded, scalded." In none of his articles does Spira acknowledge that any animal should ever be used for any experiment, no matter how crucial it may be judged to be for human welfare or survival. How much further is "all-the-way"?

Moreover, Wade devotes roughly one-third of the article to some of the rationale of the antivivisectionist movement, such as the simplistic, reductionist idea

that "alternatives to live animals," for example, research with computers, test tubes, and tissue cultures, can be substituted for animal experiments, and to the quasi-moralistic claim that animals have "rights" equal to the sociopolitical rights of women and minorities. Wade does not indicate that many see such statements as being antiscience and, in fact, fails to present any of the opposing views.

In Wade's effort to judge the value of our research on sex behavior of cats, he brushes aside the repeatedly favorable decisions of several peer review committees ("it is difficult to second-guess their judgment") and chooses as his sole criterion of scientific worthiness the number of citations in *Science Citation Index*. Of the 21 publications to which Wade refers, the seven full reports, each representing 3 to 5 years of continuous experimental observation, have all been cited except for one which was published in Moscow. In addition, two doctoral dissertations by former students have been cited as such, and later as journal publications. The remaining 14 publications were abstracts of reports given at scientific meetings while the work was in progress, and even a goodly number of these have been cited. As Eugene Garfield, father of the *Science Citation Index*, has emphasized, the index can only serve as a valid criterion if its limitations are recognized and it is used properly.

We believe that the criteria for the morality and ethics of research must include its significance for human welfare, but the statements made by critics of the moral and ethical aspects of our work raise doubts about their values in this respect. How much suffering does one allow, how many human lives and how many pets does one sacrifice in the name of the "rights" of discarded and unwanted cats and dogs or laboratory-raised animals? On the order of 1 million unwanted cats and dogs are destroyed each year in animal pounds, their use denied to legitimate research institutions. Wade chooses not to discuss these fundamental aspects of the problem. Ironically, we find ourselves in agreement with the antivivisectionists on one point. They are distributing copies of Wade's article, which they view as supporting their cause.

The AAAS, as the most representative organization of scientists in the country, is obligated to respond to the antiscience, anti-intellectual, and inhuman position taken by our critics.

LESTER R. ARONSON
MADELINE L. COOPER
Department of Animal Behavior, American Museum of Natural History, New York 10024

The demonstrations against the animal behavior research at the American Museum of Natural History are potentially damaging at several levels. First, there is the potential damage to the public reputations of Lester Aronson and Madeline Cooper. Those who are unfamiliar with their research may wonder whether it justified the cost in dollars and animal lives. However, Aronson and Cooper will continue to be regarded highly by their scientific colleagues. Few scientists have contributed more to making us aware of the complex interactions between behavioral experience and the neurological and hormonal control of behavior.

Their recent work on the amygdala demonstrates that some widely held opinions on the function of this important part of the brain are wrong, partly because these opinions derived from research less sophisticated than that characteristic of Aronson and Cooper. Next, there is the potential damage to the Museum's Department of Animal Behavior. Since the late 1930's, this department has had a major influence on the conceptualization of human and animal behavior. Several of this nation's leading behavioral scientists were trained there, and the department's academic progeny are now teaching and doing laboratory and field research around the world. Under the curatorships of Frank A. Beach, T. C. Schneirla, and Lester Aronson, the department became a center of the "epigenetic" view of development, a theory which emphasizes that each stage of an organism's development results from a dynamic interaction between the organism and its environment. This view served as antithesis to the more preformationistic thesis of classical European ethology. If there is a current synthesis, it emerged from the interaction between the epigeneticists and the classical ethologists.

The broadest potential impact of the antivivisectionists is to reduce or eliminate the use of animals, or at least domestic animals and primates, in basic and applied biomedical research. Since these animals are often the most appropriate physiological models, the outcome would be to terminate research that cannot be initiated with humans. To the extent that this broad goal is achieved, human health and human knowledge may pay a terrible price.

The public's right to challenge the ethics and economics of animal research is unquestioned. The present peer review system, as fallible as it may be, has been largely successful in curbing unethical excesses and in fitting research priorities to available funds. Until a better system is developed, the peer review process should prevail.

One final note. Ten years ago, *Science* rejected, without review, a report by Aronson and Cooper because the editor felt that the sex research on cats, as described in that report, would offend the sensibilities of some *Science* readers, including antivivisectionists. Ultimately, *Science* had the report reviewed and published a modified version (8 Apr. 1966, p. 226) with no adverse repercussions. Despite the superficial sexual enlightenment of the last decade, the current reaction to the sexual aspects of the research of Aronson and Cooper and many of their colleagues indicates that, for many persons, basic research on sexual behavior is still beyond the pale. As Wade suggests, sex research tends to gain notoriety easily, no matter how ethically it may be conceived and executed.

BENJAMIN D. SACHS
Department of Psychology, University of Connecticut, Storrs 06268

(Continued on page 862)

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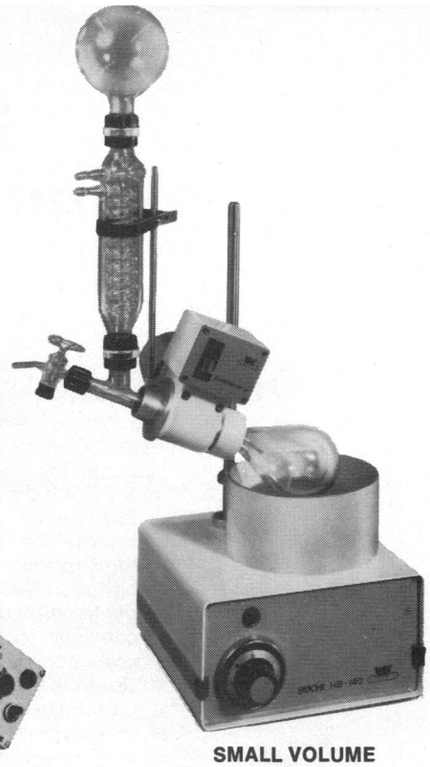

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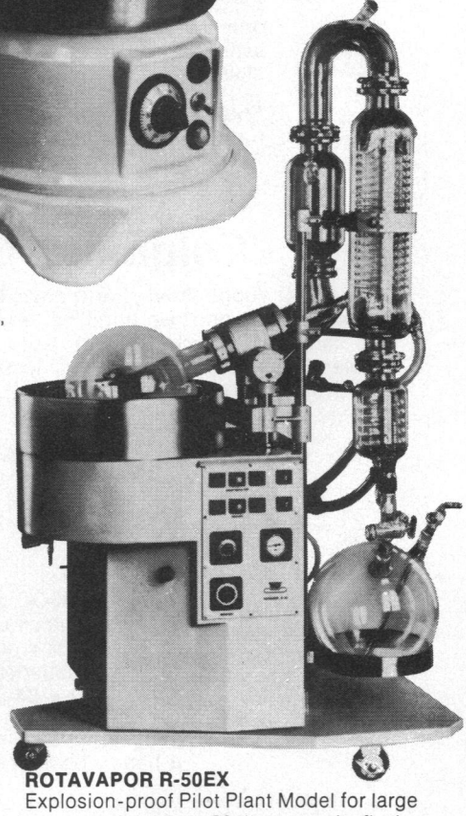

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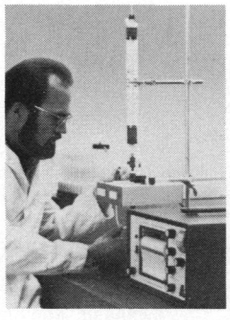

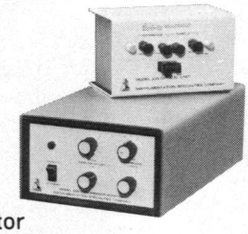

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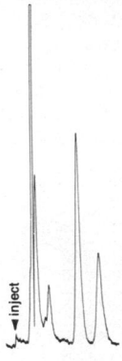

This typical curve was recorded at .005 Abs full scale on a Model 226.

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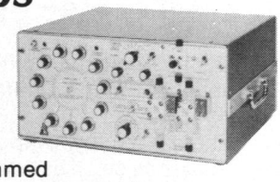

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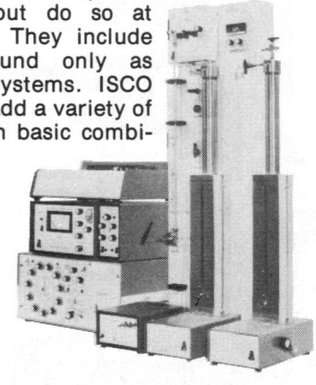
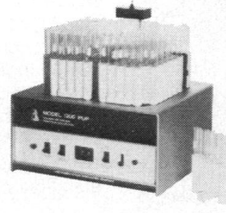


Fraction Collectors



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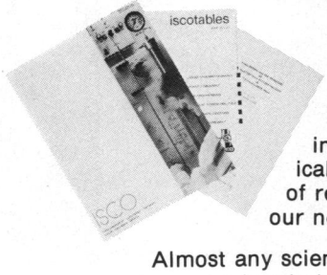

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19 November 1976, Volume 194, Number 4267 **SCIENCE**

On Working with the Soviets

Out of the Brezhnev-Nixon handshake cementing détente in Soviet-American relations in 1972 has come a brisk traffic of shuttle diplomacy in science and technology. A wide array of agreements for joint studies and exchanges of information are being carried out, each with counterpart Soviet and American working parties. As the original bilateral agreement comes up for renewal next year, how should the American scientific and technological community assess the value of the work accomplished to date, and the merits of Round Two?

One way to go at it is to total the runs, hits, and errors and announce which side is "ahead." This is how the politics of Soviet-American relations have been played since World War II, and it is questionable whether the world is safer or happier. If détente in scientific and technical affairs is to be treated as an arena for competition and scored by the rules of competition, then we have strayed from the announced motive of cooperation. Whatever may be said for competition in the spheres of defense and power politics, it is no standard for judging a *process* designed to find common ground between two powerful systems.

As one casts a critical eye over the universe of the cooperative projects in science and technology, it becomes clear that the majority have progressed quite well, while a few others are struggling. In some cases it has been hard to find a handle, in others there arose semantic and conceptual problems which took time and patience to iron out, and there were the difficulties which always arise when busy people on each side can give only fractional time to the work.

The basis for the agreements was equivalency. From the start, it was to be a fifty-fifty proposition, with each side getting comparable benefits. The U.S. side has not relaxed that rule, and the result is that the accounts appear to be balanced. But equivalency is in large measure a matter for judgment rather than precise scoring, and the bottom line can always be a subject for argument. Moreover, when two such different systems try to get together, they come to the table with different premises and constraints. These do not disappear with a handshake. Equivalency comes slowly, stage by stage. It is a point to be kept in mind by détente-watchers.

The exchange has been especially lively with respect to the comparisons of national science policies. While the Soviet practice is to build scientific research and development into their macroeconomic plans for periods of as much as 15 years, keying it to upwards of 200 priority problems in each 5-year planning segment, their R & D is not locked in so firmly that goals and strategies cannot be changed on short notice. The Soviet passion for planning provides more continuity and stability for science than we do, and their policy recognizes the investment nature of R & D in a way that ours never has. Where we seem to do better is in applying research results under conditions of market choice and risk, even without the help of an explicit emphasis on large doses of R & D in macroeconomic policy. With all the basic differences, both systems seem to produce very good research and innovation.

As the American and Soviet working groups have learned how to get along, the momentum has picked up. Experts are traveling in both directions, previously unavailable data are being gathered and evaluated, and case studies of comparative technologies, research and innovation, and problem-solving are being exchanged. Recent meetings in both countries have been unusually productive, suggesting rising confidence on both sides. It is not all a bed of roses, for there are some bare patches. But the prognosis is decidedly better than it was even a year ago. All things considered, the case for Round Two is a good one.—WILLIAM D. CAREY

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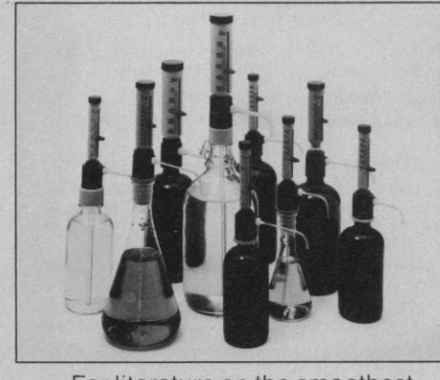

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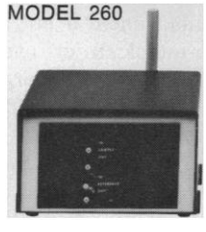

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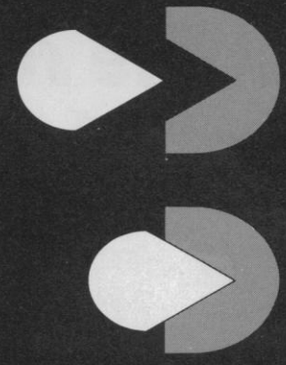

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LETTERS

(Continued from page 786)

Let me comment on the "historical trend . . . from minorities' rights, to women's rights, to animal rights" that, according to Wade, has given philosophical fuel to the controversy on animal experimentation. If the trend continues, it may well bring the controversy to an end. As recounted by Samuel Butler (*1*) this is precisely what happened a long time ago in the mythical country of Erewhon. A fussy old prophet, it seems, had stood up for the rights of animals with such fervor that for 600 years the Erewhonians were by law allowed to eat animal food only if the animal had died of natural causes, including suicide. Then along came a philosophically inclined Professor of Botany who argued convincingly that vegetables are only animals under another name. The Professor maintained, in fact, ". . . that animals and plants are cousins, and would have been seen to be so, all along, if people had not made an arbitrary and unreasonable division between what they chose to call the animal and vegetable kingdoms." He therefore proposed that the natural death rule should logically also apply to vegetables and their seeds. "Having thus driven his fellow-countrymen into a corner at the point of a logical bayonet," Butler continues, the Professor referred the confused Erewhonians to an Oracle, whose response sanctioned the eating of fresh vegetables and, by extension, of animals killed for the purpose. The Erewhonians then repealed the ill-tolerated and controversial vegetarian laws, and lived more or less happily ever after.

Thus, we'll just have to wait for the modern counterpart of the old philosopher who will stand up for the rights of vegetables. When vegetarians, botanists, farmers, and gardeners arouse the same outraged feelings as meat eaters, experimental psychologists, dogcatchers, and baby-seal clobberers are doing now, an Oracle may be found whose words will retrieve us, as Butler puts it, from wandering in the wilderness of philosophy.

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Is it utterly foolish to wish that people could get as steamed up about operations on scientists at Leningrad state prison (Letters, 8 Oct., p. 133) as on cats at the American Museum of Natural History?

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The scientific establishment is not, as many feel, under attack from overemotional antivivisectionists. Other organizations with a more balanced approach, such as The Humane Society of the United States, have also voiced their concern. The embarrassing scenario involving the Department of Animal Behavior at the American Museum of Natural History is symptomatic of the effect of a widespread and relatively radical change in societal values and attitudes. These values and attitudes impinge upon biomedical research and especially upon so-called basic research, which is (to quote the director of the Museum) the "freedom to study whatever it [the Museum] chooses, without regard to its demonstrable practical value." The scientific community that uses animals and whose basic research is supported wholly or in part by the taxpayer should be open to discussion of the validity of their work.

Three questions need to be addressed: (i) Should the "luxury" of basic re-

search, of seeking knowledge for knowledge's sake (and with no forseeable applied value) be supported by public funds? (ii) Is it fair, ethically speaking, to use animals in studies—to create and destroy life and sometimes cause suffering (if unavoidably "essential" for the research) for purely intellectual reasons? (iii) If relevance of such animal studies to an understanding of human diseases or other "humanocentric" problems is claimed, does not the basic researcher create his own trap? Relevance, after years of reductionism and nonapplied research on esoteric subjects, may be very difficult to demonstrate.

The complex area of basic biomedical research—including many studies in animal behavior and physiological psychology—needs to be looked at from a new perspective. Beginning with animal rights is only a start. We will get nowhere if the basic researcher remains locked in his own conceptual world of consensus values, approved standards of animal care, and so forth. We must all be free to look "objectively" at ourselves and avoid being defensive under the fire of outside criticism by others who do not share the same world view. Basic research, and biomedical research in general, may well benefit once open and constructive dialog on animal research is achieved—with ultimate benefit, one would hope, to the animals themselves.

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I regret that Aronson finds hostile an article in which his assailants' two most serious charges are dismissed as groundless or absurd and the third is discussed but not endorsed. It is only in general terms, not in their specific attack on the Museum, that I think the animal rights groups' arguments are at least worth considering. Their campaign has undoubtedly been hard on Aronson and on the Museum. Aronson, as the article concludes in discussing the campaign, "is an established and productive scientist whose work, in the aspects for which it is being assailed, differs in no way from the research carried on by a great many other investigators."—N.W.

Erratum: In the letter from William R. Havender (1 Oct., page 9, column 2, paragraph 1, the next-to-last-sentence), the word "not" was inadvertently omitted from the parenthetical phrase, ". . . (or else, the within-group heritabilities would *not* be high, as posited)."

Erratum: In the reply to the letter from Vladimir J. Konečni by Harry W. Power (5 Nov., page 563, column 2, paragraph 1), the last sentence should have read "To treat a functional dichotomy as a continuum is to do as great a violence to truth as to treat a continuum as a dichotomy."

RESEARCH NEWS

(Continued from page 826)

force as deuterium is to the nuclear force. The interaction appears to be quite complicated. There is a need for large spin-orbit and tensor forces which are as as yet incompletely understood. The central force is not an inverse-square or harmonic-oscillator one, but something intermediate; roughly a force independent of the quark-antiquark separation. But in any case great optimism remains that in the long run the charmonium system can teach much about whether, why, and how quarks of fractional charge are confined within hadrons.

Thus, rather rapidly, the evidence for ψ being a member of the hadron family as well as a bound state of a spin 1/2 constituent with its antiparticle became quite decisive. Just this much explains its production by gamma-rays, the charmonium levels and helps to interpret the large yield of hadrons at high energy observed by CEA and SPEAR. The crucial test came in the expectation that charmed quarks will bind to uncharmed antiquarks, forming overtly charmed hadrons. Their rest energy could be estimated at about 2 Gev, and in analogy to the behavior of strange particles they would be unstable with respect to the weak interactions. This leads to an estimated lifetime of order 10^{-13} second for a 2-Gev charmed particle. No time was wasted in embarking on the search. Ting was in an especially good position to attempt the search himself, using his spectrometers to detect charmed particles which decayed into two oppositely charged hadrons. Ultimately 10 million events were accumulated, but no positive evidence was found. A similar experiment at Fermilab extended his result to higher energies, and as yet charmed hadrons have not been detected as products of hadron-hadron collisions.

Actually, it was expected that electron-positron annihilation and neutrino reactions would be the best sources of charmed particles, but for a long time the search was inconclusive. Neutrino-induced reactions did provide positive evidence for charm, but not until the discovery of charmed mesons at SPEAR this year did the case for charm become highly persuasive. There is perhaps still room for the skeptic to doubt the existence of charm (not to mention the quark) but it is a severe uphill battle to do so.

The charm concept is more than just a label for a fourth quark; it has specific implications for problems of the weak interactions. The word charm was intro-

duced by Sheldon Glashow (*4*) as early as 1964. Given the introduction of a fourth degree of freedom for hadrons, a formulation of weak interactions was found that was more symmetrically balanced between hadrons and leptons (the family containing electrons, muons, and their neutrinos). The unique feature of this charm picture was the prediction that charmed matter should decay weakly and predominantly into states containing strange matter. This signature made the search for charm quite specific, and the prediction so far appears to be borne out by experiment—although much remains unclear. But in 1964 this was mostly speculation and esthetics.

In 1970 real operational use was found for the charm concept in dealing with problems of the high-energy behavior of the weak interactions. The basic description Fermi so brilliantly provided in 1932 (as refined by a few others in the subsequent 30 years) necessarily fails at high energies, of order 1000 Gev of center-of-mass energy. Corrections must exist. During the late 1960's, confidence in the technique of estimating such corrections grew (thanks to large extent to the work of the Soviet theorist B. L. Ioffe) and it was found that unless some new physics intervened at an energy scale of only a few Gev, it was difficult to understand why the two neutral strange mesons, K_L and K_S, should possess such a small mass difference (experimentally it is less than one part in 10^{11} of the electron mass). In 1970 Glashow, with J. Iliopoulos and L. Maiani, showed that the 1964 charm scheme did the job, provided the charmed quark was not too heavy.

This observation lay dormant for a couple of years, until some beautiful mathematical physics by a young Dutch physicist Gerard 'tHooft opened the way for a quantitative theory of weak interactions, where such corrections can be systematically computed. Not only is such a theory practical from the point of view of computation, but its starting point is modeled after quantum electrodynamics. Even more, quantum electrodynamics is subsumed into the larger structure, somewhat as electrostatics and magnetism are subsumed within Maxwell's electrodynamics. Finally, the simplest version of such a unified weak electromagnetic theory, as formulated by Steven Weinberg and Abdus Salam, requires the existence of muonless neutrino-nucleon interactions ("neutral currents"). These not only have been found but, most importantly, agree quantitatively with the theory. It is no wonder that this theory is by far the most serious candidate for a successful description of the weak force in its natural energy domain. But, for essentially the reasons discovered earlier, inclusion of the charmed quark appears to be an unavoidable ingredient in this theory as well.

Thus the existence of charm lends support of sorts to the present viewpoint on weak interactions and its potential synthesis with electrodynamics. Nowadays, with charm scarcely established, many are already looking far beyond the detailed study of charmed matter. Ting has followed Lederman to the CERN storage rings and is now mounting a large experiment, this time to detect muon pairs with rest energies up to 15 Gev, in the hope that history may repeat again: there is no particular reason why the number of quark types should stop at four. Richter is deeply involved in a large electron-positron storage ring project (PEP) which is under way at SLAC. Center-of-mass energies of more than 30 Gev will be available upon completion of the project in about 1980. Ting is also planning an experiment at a similar project (PETRA) under construction at DESY. Beyond that, Richter is a strong proponent of the eventual construction of very large electron-positron rings of center-of-mass energy up to 200 Gev. If the present viewpoint of weak interactions is correct, at these energies quantum electrodynamics, as Richter and Ting knew it in the 1960's, *should* break down, and with their 30-Gev experiments at PEP and PETRA they should be able to see the breakdown occur. If they do not, and instead the old quantum electrodynamics is verified, there will be almost as much impact on the theory as had Ting or Richter found a breakdown in their early experiments.

J. D. BJORKEN

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2. J. Augustin, A. Boyarski, M. Breidenbach, F. Bulos, J. Dakin, G. Feldman, G. Fisher, D. Fryberger, G. Hanson, B. Jean-Marie, R. Larsen, V. Lüth, H. Lynch, D. Lyon, C. Morehouse, J. Paterson, M. Perl, B. Richter, P. Rapidis, R. Schwitters, W. Tanenbaum, F. Vannucci, G. Abrams, D. Briggs, W. Chinowsky, C. Friedberg, G. Goldhaber, R. Hollebeek, J. Kadyk, B. Lulu, F. Pierre, G. Trilling, J. Whitaker, J. Wiss, J. Zipse, *ibid.*, p. 1406.
3. There is no general consensus on the ultimate name of J/ψ. With all due respect to Professor Ting, I choose to use the name ψ here. Not only is it a matter of habit, but also the ψ (3.1) appears to be a member of a family containing ψ' (3.7), ψ'' (4.1), . . ., whose names are not ambiguous. Given that J is married to these others, is it not appropriate to change its maiden name?
4. B. J. Bjørkén and S. L. Glashow, *Phys. Lett.* **11**, 255 (1964). The peculiar spelling of my name was used in order to maintain deniability, which I here affirm. in that paper our relative contributions to the development of the charm concept were in the same proportion as our subsequent contributions.

BOOKS RECEIVED

(Continued from page 834)

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