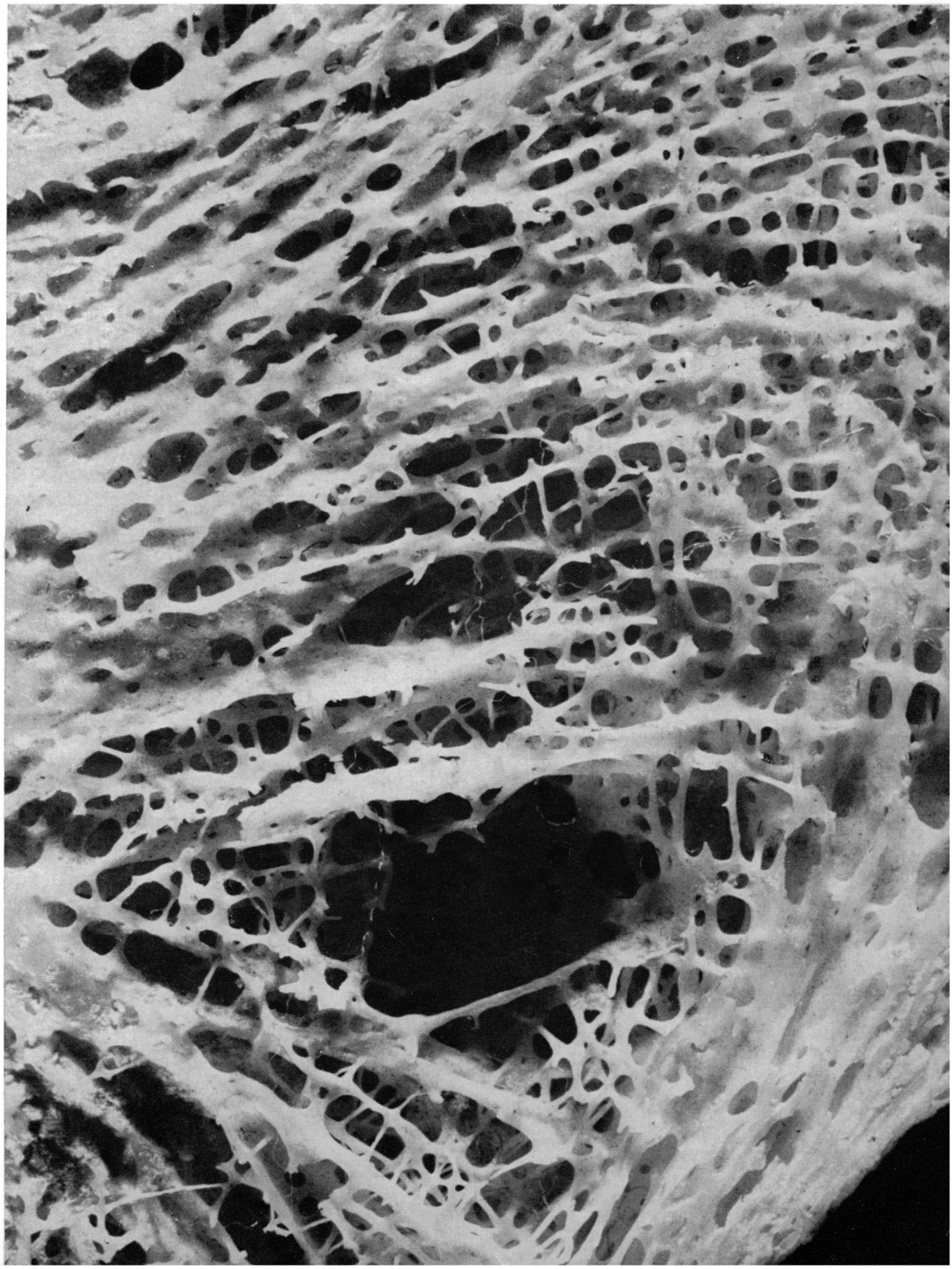
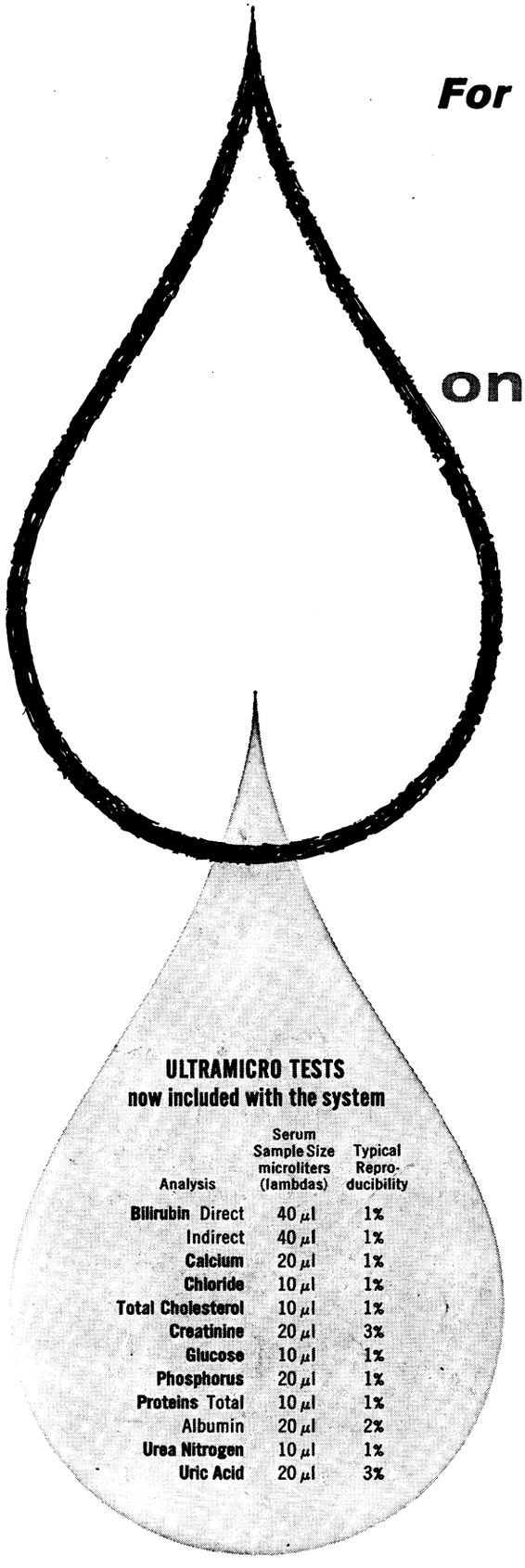
SCIENCE

8 July 1960

Vol. 132, No. 3419

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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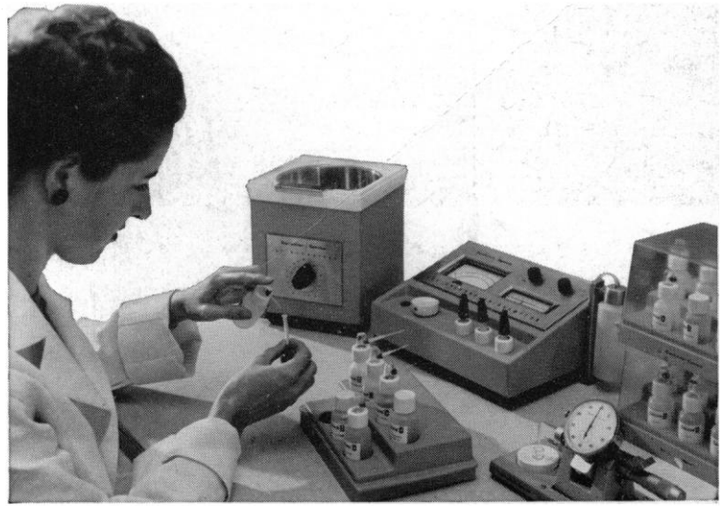

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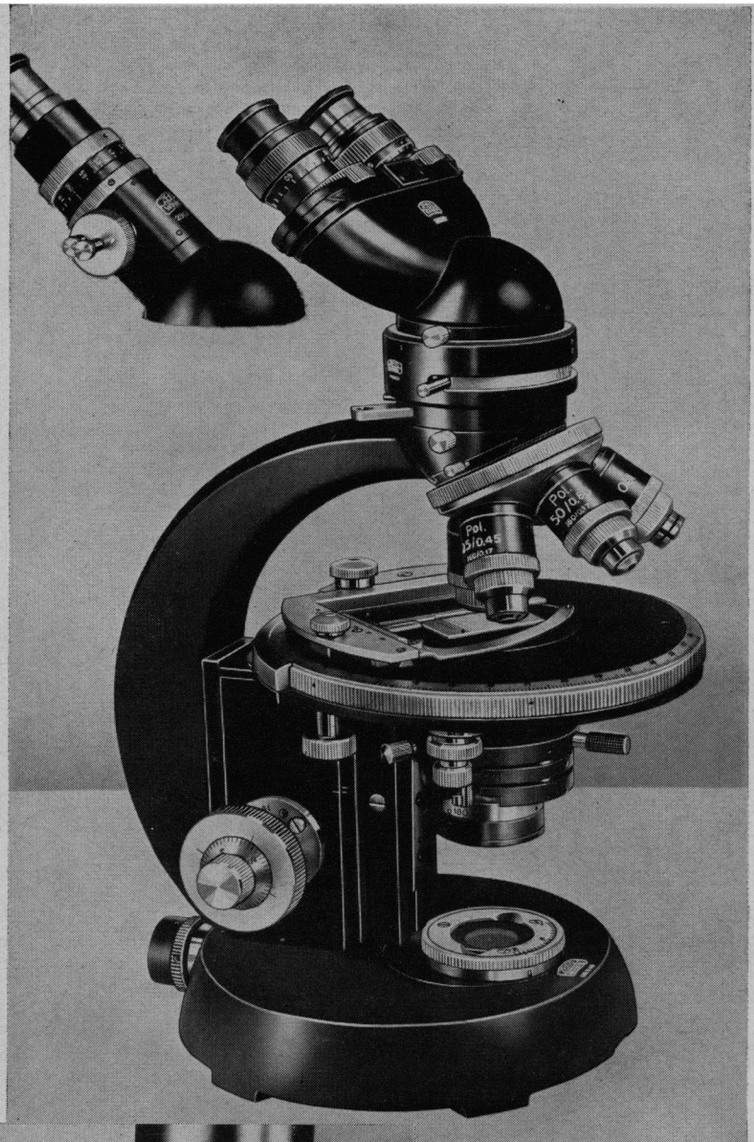

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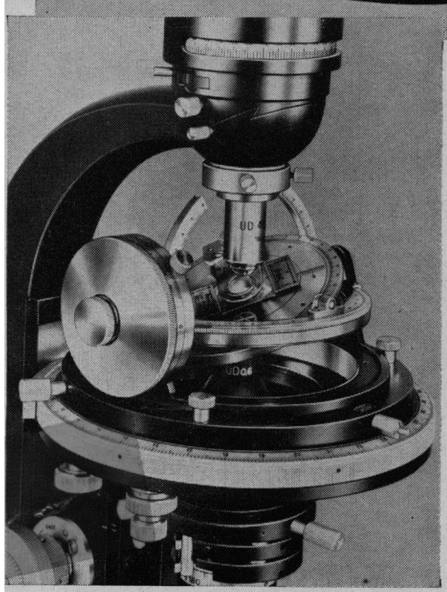

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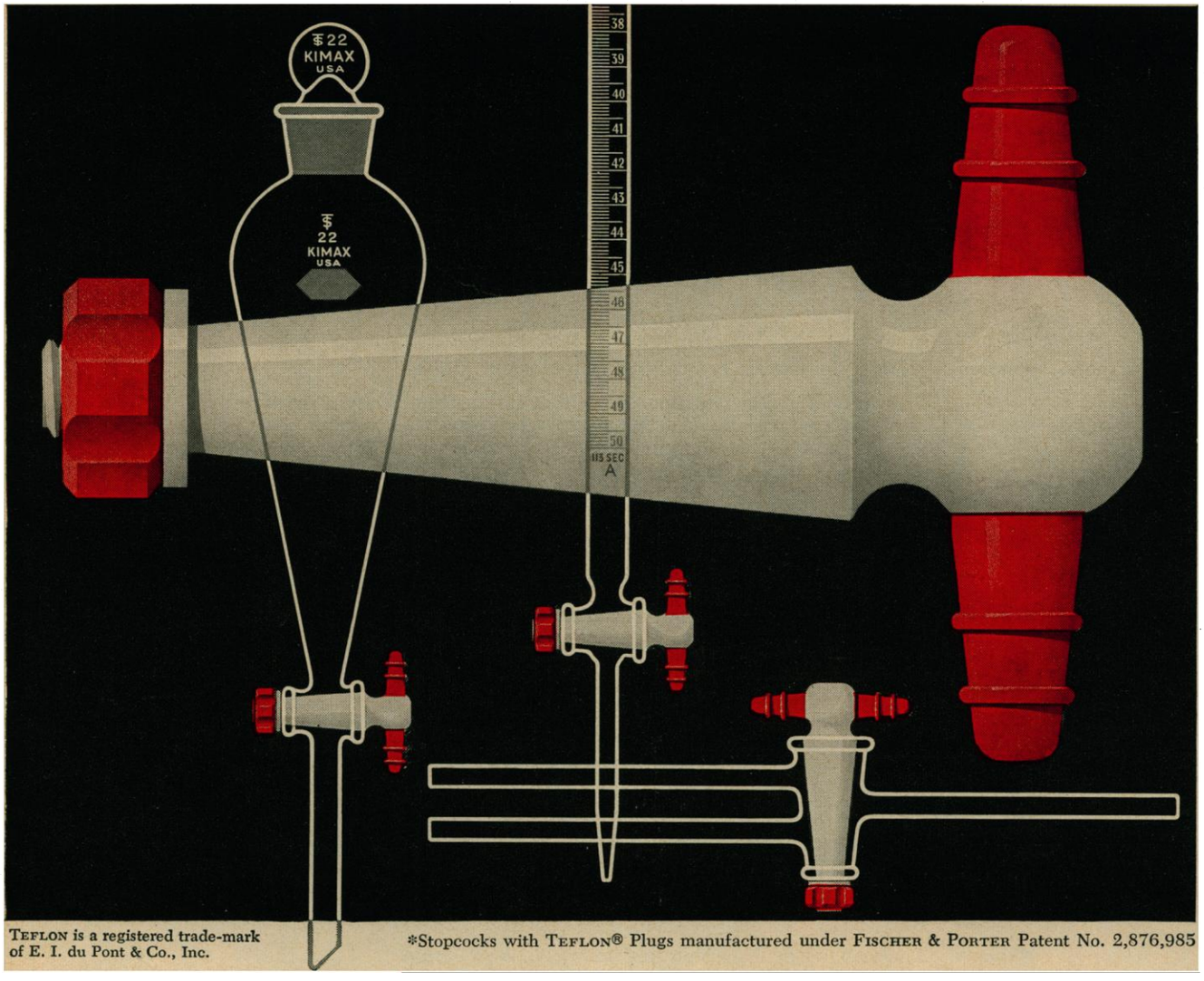

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SCIENCE

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Reverse in Geneva

One effect of the summit collapse in Paris on the negotiations for a ban on nuclear weapons testing in Geneva was the reversal by the Soviet delegation of its decision to go along with the United States proposal that more research be conducted on ways to control small underground explosions. Soviet scientists had said that although the Soviet Union would not conduct experiments with nuclear devices, there would be a three-year program using chemical explosives. But about three weeks after the summit collapse, the Soviet delegation announced at the political level that there would be no tests of any kind in the Soviet Union and that Soviet scientists had not been empowered to speak in the name of the Soviet government.

In seeking better ways to detect small underground tests, United States scientists had, for their part, proposed exploding a series of nuclear devices. Soviet scientists had objected to some experiments in the American series, but they agreed to others on condition that adequate safeguards be provided to assure them that no results of military value could be gained along with results of use in improving the design of a control system.

By way of assurance, the U.S. has subsequently offered to share all test results with the U.S.S.R. and to admit Soviet observers to the test site. The U.S. has also offered to put immediately on deposit, under international supervision, boxes containing all the bombs to be used in the research program, and so to demonstrate that results from devices exploded earlier in the series could not be used as the basis for design of the devices exploded later. The Soviets have demanded, however, precisely what the Americans have not offered and what is against the law barring the disclosure of nuclear secrets—namely, a look at the devices themselves. There may be ways for the United States to escape from this predicament, but any proposal is easily countered by a person determined to offer objections. A proposal to explode Soviet bombs instead of American bombs, for example, could be construed by Soviet negotiators as a thinly disguised plan to spy out the characteristics of Soviet weapons.

The U.S. is now mounting an effort to determine just what Soviet intentions are on a test ban. It is important, of course, to see whether we actually can specify safeguards for our research program that will prove acceptable to the U.S.S.R., but the course of the technical talks is ample evidence that the Soviets are as informed as we are about the limits of the present technology of test controls and about the advantages and disadvantages of a research program. Reversal of the Soviet stand on its own research program, with no explanation offered, suggests that the Soviets have found in their demand for assurances an opportunity to stall negotiations while making it look as if it is we who are doing the stalling.

Accordingly, in the new effort to determine Soviet intentions, the United States delegation should focus its efforts on the political level. We should push for answers on such familiar matters as the number of on-site inspections the Soviets will permit and the nationality of the staff at the control posts—questions which the summit meeting was supposed to have answered. It is on the political level that the more fundamental differences lie. And it is here that we are in the better position.—J.T.

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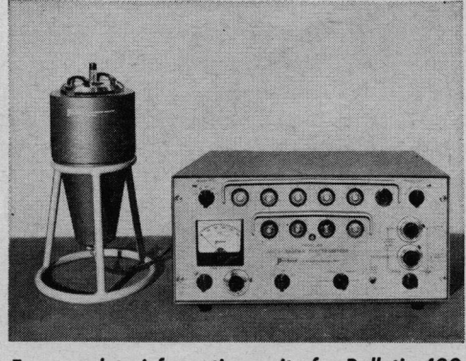

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Obviously, automatic sample counting is desirable when large numbers of samples are to be counted. It is just as useful, however, for counting small numbers of low activity samples. Blanks and standards can be arranged among the samples for background checks and calibration. The complete series of tubes can then be counted automatically as many times as required to give desired statistical accuracy.

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Kodak reports on:

photography for biologists . . . increasing the peak value of informational sensitivity . . . highly fluorescent chelated Mg ions . . . the parts-per-billion business

Four of us at Woods Hole

Four of us expect to put in a rugged week on Cape Cod beginning early on the morning of August 1. It's not exactly an exhibit or symposium or anything as formal as that. It's just that the management of the Marine Biological Laboratory at Woods Hole has given us to understand that if we want to hang around for five days dispensing free advice on how to use photography to communicate results of biological investigation, they will probably refrain from calling the cops to throw us off the premises.

One of us will be there to hold court on problems encountered in photomicrography and photomacrography of marine life—movies or stills, color or black-and-white. Another of us will take over when questions come up of photographing living small specimens at natural size or less, with or without flash. The third man knows a lot about photographic materials for electron micrography, autoradiography, and microradiography. The fourth, a biochemist, will make himself generally pleasant and helpful.

We figure any time spent feasting on Homarus americanus *is time lost from showing our movie of* Homarus americanus *hatching in glorious Eastman color.*

Nice sharp pictures

We must not lose our sense of proportion. We must recognize that some people take so little technical interest in photography that they have never even heard of *Kodak Tri-X Pan Film.* Others who do use it have noticed that since that "Improved Type" has appeared on the carton, results have somehow improved. Most of these folk will be content with a bland assertion that the recently added notation signifies a major advance in reduction of graininess and improvement in picture sharpness. A single congenial luncheon table could probably accommodate all who would press us to explain that what we have really done is to increase the peak value of the informational sensitivity I of *Kodak Tri-X Pan Film* from .0016 to .0036

where $I = \frac{g}{a_s} \cdot \frac{1}{\sigma_s(D)}$

g = gradient of the linearized characteristic curve

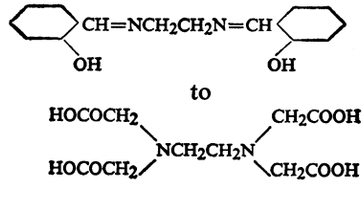
a_s = area of the spread function

$\sigma_s(D)$ = standard deviation of the granularity trace for a scanning aperture having the size of the spread function

If you want to talk like that you should first consult Journal of the Optical Society of America, 48, *926, but if you just want very sharp pictures from very fast film, ask the Kodak dealer for Kodak Tri-X Pan Film, Improved Type.*

Suggestion at Pittsburgh, Pa.

When we read the advance program for this year's Pittsburgh Conference on Analytical Chemistry & Applied Spectroscopy, we decided on a coup. One of the papers was to state an interesting fact about bissalicylideneethylenediamine, which stands in relation to the well known chelating agent EDTA as



When the former, in slightly alkaline *N,N-Dimethylformamide* (Eastman 5870), chelates Mg ions, it becomes highly fluorescent. The excitation maximum occurs at 355mμ, and emission maximum occurs at 439mμ. A sensitivity of 7×10^{-5} micromole of Mg per ml was mentioned.

This time, said we to ourselves, when they come charging out of the meeting room to our exhibit to ask whether we have this reagent, they will encounter no mere bland smile. We will be able to reply, "Yes, sir, here it is, labeled α,α'-*(Ethylenedinitrilo)di-o-cresol* and designated Eastman 8019."

And that is exactly what we were in a position to do that day. The only trouble was that while many people did flock to our booth and talked of many things, not one soul as much as mentioned bissalicylideneethylenediamine.

Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company) has some 3800 other Eastman Organic Chemicals. Many of them do excite a decent level of interest.

Spectroscopy in electronics

We wish we could inspire several dozen more persons to enter the craft of emission spectrography. They would become customers for *Kodak Spectrum Analysis Plates* and *Films* as well as occupants of a secure place in technical society, one of waxing importance.

To convince that the importance indeed waxes we could send copies of a disquisition written by one of our dealers, a gent willing to undertake some deep thinking and digging out of useful information in hope of the favor of an order.

Think of the old days, he suggests, when electronics meant radio and the man at the end of the radio assembly line was given plenty of adjustable resistors, capacitors, and coils on the chassis to adjust in compensation for the unpredictable characteristics of the vacuum tubes. Electronics isn't that way any more, he implies. Today electronics is supposed to assume that its solid-state devices and the cathodes of its vacuum tubes will behave predictably within very narrow limits.

And what sets these limits?

Among other things, the presence or absence of certain chemical elements in the range of parts per billion.

How determined?

By emission spectrography.

Is this easy?

Not particularly.

What's one way to start surveying the techniques?

Writing for a copy of "Spectroscopy in Electronics" to Eastman Kodak Company, Special Sensitized Products Division, Rochester 4, N. Y.

This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science

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Letters

Oxygen Transport

I found Scholander's article on O_2 transport [*Science* **131**, 585 (1960)] very interesting, and I think I can lend support to it since similar conclusions were drawn (though not published) by myself and J. P. Baumberger some years ago in a completely different experimental situation [*J. Gen. Physiol.* **36**, 255 (1952)].

We found that the polarographic diffusion current for O_2 given by whole blood was six times greater than that given by plasma when both were measured at O_2 tension of 100 mm-Hg at 37°C. This excess diffusion current depends not only on red cell concentration but on factors which increase the rate of dissociation of HbO_2: degree of O_2 saturation, pCO_2, pH, and temperature. The readiness with which HbO_2 releases O_2 determines (i) the rate at which O_2 is replaced in the immediate vicinity of the "consuming" cathode and (ii) the rate at which Scholander's "bucket-brigade" operates, at a greater distance from the cathode. (The rate of "passing the bucket" obviously depends on the dissociation rate.) The driving force behind this steady-state flow is the constant rate of O_2 "consumption" of the cathode, which maintains a constant pO_2 gradient in its vicinity.

The resultant of this situation is that, at a pO_2 of 100 mm-Hg, the cathode receives O_2 from whole blood at a rate at which it could receive O_2 from plasma only at a pO_2 of 600 mm-Hg. If we now substitute rapidly metabolizing cells for the cathode, the conclusion follows that in the presence of oxygenated red blood cells the effective rate of diffusion of O_2 to the consuming cells at a given pO_2 will be much greater than could be expected on the basis of the O_2 tension difference alone.

GABOR MARKUS
Roswell Park Memorial Institute, Buffalo, New York

Competitive Exclusion

As an economist, I have been delighted with the recent revival of the earlier view that ecology and economics are closely related. Hardin's article ["The competitive exclusion principle," *Science* **131**, 1292 (29 Apr. 1960)] is an excellent example of this revival, as would be expected in view of his previous work. Unfortunately, it contains an error in economics. From the principle that complete competitors cannot coexist he deduces the development of monopolies. The principle, however, applies to species, not individuals. Park's experiments did not show that one individual flour beetle grew so large that it eliminated all the others, only that one species grew so numerous that it eliminated the other. If the principle has any application to economics at all, it would indicate that one type of economic enterprise might, by multiplication of its members, replace another, but this would not lead to monopoly. The problem of monopoly is a real one, and an important one, but it has nothing to do with the competitive exclusion principle.

I rather dislike attacking an article which is, in general, as superior as Hardin's, but in this one area he is wrong.

GORDON TULLOCK
Department of International Studies, University of South Carolina, Columbia

Garrett Hardin points out that the "competitive exclusion principle"—that is, that "complete competitors cannot coexist"—is essentially a theoretical concept unlikely of direct proof in the field. I believe this conclusion is strengthened if it is considered that at least three common conditions, not mentioned by Hardin, must be *absent* if the replacement of one species by its complete competitor is to proceed in a direct fashion. However, at least two of these conditions are likely to be *present* in the field unless the replacement process is quite rapid.

If there exist two complete competitors, species A and B; if species A is slowly replacing species B; and if this process is to continue directly to the point of (local) extinction of species B; then the following possibilities must *not* occur:

1) A decrease in the genetically determined competitive ability of species A below the level of that of species B.

2) An increase in the genetically determined competitive ability of species B above the level of that of species A.

3) A change in the environment sufficient to shift the competitive superiority from species A to species B.

The first possibility is somewhat unlikely, on evolutionary grounds; the second is fairly likely if the replacement process is quite slow, a condition which also implies that the difference in competitive abilities of the two species is small and hence likely to be easily changed by genetic variation. The third possibility would appear to be quite likely under many conditions. If the particular environmental factor is one which oscillates between the condition favoring one species at one time and the other at another time, and if the rate of oscillation is sufficiently fast as com-

pared to the rate of replacement of either species by the other, then the two complete competitors may, in fact, continue to coexist for a long period of time.

WERNER G. HEIM
Department of Biology, Wayne State University, Detroit, Michigan

If one thinks of a corporation as an individual, Gordon Tullock's criticism is justified. But this "model," though sanctified by a century's evolution of U.S. legal theory, is not the only possible one. One can also think of a corporation as an aggregate of individuals competing with other aggregates engaged in the same line of business. We assume no interbreeding of the aggregates ("mergers"). The equivalent of biological reproduction may be taken to be the hiring of new personnel. The limit of possible income is the limit of consumer demand for the goods or services of the kind offered. If there is free competition and no ecological differentiation, the most efficient aggregate will necessarily displace all others.

A tendency toward this sort of displacement is seen also in the competing of any two "species" of cells within the same individual whenever there is a breakdown of the poorly known cybernetic controls that keep the various kinds of tissues within bounds. See, for instance, G. Crile's review of the cancer problem [*Perspectives in Biol. and Med.* **3**, 358 (1960)]. Within a multicellular body that must meet certain stringent demands of the external environment, the exclusion principle cannot, of course, be worked out to its conclusion; the multicellular envelope dies first.

Werner G. Heim's remarks point up some important points which were scarcely more than hinted at in the last section of my article. We now know of many competing species, or competing alleles within a species, that manage to coexist because their relative competitive efficiencies change with the seasons, and the seasons always change. N. W. Timofeef-Ressovsky [*Biol. Zentr.* **60**, 130 (1940)] has carefully described the seasonal alternation of genotypes in a beetle. Comparable studies have been made with other species by E. B. Ford in England and T. Dobzhansky and his students in this country.

In addition, our theory must take account of changes in the environment that are brought about by organisms themselves. M. J. Beijerinck's "enrichment culture" method [see F. Stockhausen, *Okologie, "Anhäufungen" nach Beijerinck* (1907)] is a direct application of the competitive exclusion principle to the problem of securing a nearly pure culture of the wanted species from a very mixed natural culture. But the method is limited by the fact that, in general, any species multiplying in a closed system will tend to make the environment less favorable for its own way of life, and thus more favorable for other forms. The result of a succession of such alterations is "ecological succession."

Facts such as these do not undermine the principle; rather, their explanation (when achieved) enriches the theory.

GARRETT HARDIN
University of California (Santa Barbara), Goleta

Names for the Sun and the Moon

The Future Scientists of America Science Club of Bergenfield, N.J., propose [*Science* **131**, 380 (1960)] the proper names *Sol* and *Luna* as substitutes for the better-known *sun* and *moon* on the ground that the latter words are common nouns and not proper ones. While the argument is plausible, I think it should be pointed out that logical reasons can be adduced for *sun* and *moon*, that the question is not a scientific one but one of English usage, and that some of the assertions are too strong.

To my knowledge, no English-speaking person ever refers (excepting in poetic or heroic utterance) to "sun" or "moon"; it is always "the sun" or "the moon." Use of the definite article indicates the object without ambiguity, whether the names are capitalized or not. We see the same usage in the terms *the nation, the president,* and especially *the earth, the world,* and *the galaxy*. It is equally possible to speak of "a sun" or "a moon" or "a nation" (but not so easily of "an earth"), and every person moderately familiar with English understands what is meant. The logic of the matter is that the definite article is often as good as a special name and is almost always used in place of the latter whenever it is not ambiguous—for example, "the sky," "the ocean," and (in the family) "the newspaper" and "the car." Furthermore, there is nothing wrong with capitalizing *Sun* and *Moon* if anyone wishes to do so; it is done in some astronomical publications, for consistency with *Venus* and *Jupiter*, and to avoid very frequent use of *the*.

The experience of centuries has shown that usage cannot be governed by fiat. Educated persons are going to be guided by dictionaries, and dictionary-makers, by literary usage, not by pseudo-scientific jargon. It is not possible for a group of scientists to reform the language to the extent proposed, even if they should be generally agreed on the desirability of doing so. I urge all serious students of science to devote their energy to the subject, rather than to hopeless causes.

Sol and *Luna* cannot rightly be said to be established names, excepting perhaps in science fiction; the "reference material" available to Vincent Massaro is not entitled to as much regard as are standard dictionaries and encyclopedias. Fowler, in his *Modern English Usage*, calls *Sol* a sobriquet, "a thing to be avoided"; he does not mention *Luna*. The words *sun* and *moon*, at least when prefixed by *the*, do not convey a "vague notion" to "people throughout the world." Such exaggerations are better avoided when one is exhorting scientists, for they have an effect opposite from the one intended.

G. M. CLEMENCE

Washington, D.C.

The Future Scientists of America Science Club decided to differentiate our sun and our moon from other suns and moons by giving them special names—*Sol* for the sun and *Luna* for the moon.

There can be no doubt that it is necessary to have special names for our sun and for our moon. It is true that "sun" in general must be distinguished from the sun that is the center of our planetary system.

The Future Scientists of America attempt to introduce the word *Sol* as a name for our sun, and they overlook the fact that this is a word not only from the extinct Latin language but also from the very vital Portuguese and Spanish languages. Not less than 20 nations on the American continent speak Spanish or Portuguese, and their *sol* corresponds to the English *sun* in all respects. So, when a specialized alternative for the English *sun* is necessary, then it is just as necessary for the Spanish and Portuguese *sol*.

I think that new words introduced in science must be acceptable to as many nations as possible. This cannot be achieved by giving a frequently used word from one group of languages a special meaning in another. It must be confessed that such specialized definition has occasionally occurred in the past. However, it seems to me unique that such a popular and common word as the Spanish-Portuguese *sol* should be suggested as a stopgap in an international science such as astronomy.

WILFRIED H. PORTIG

Department of Meteorology, University of Texas, Austin

Massaro regrets the lack of proper names for the sun and moon of the earth. He recommends, for *international* use, the words *Sol* and *Luna* as proper names for these two bodies in our system, the words *sun* and *moon* to be retained as common names for the center of a system and a satellite in general. Unfortunately, Massaro considers the problem solely from the point of view of his own language, in which *Sol* and *Luna*, being different words from the common designations *sun* and *moon*, may easily be established in the usage suggested. But what about other languages? For instance, *sol* is the common name for sun in Danish, Swedish, and Norwegian, and the Russian common name for moon is *luna*.

Being no authority on the question myself, I should not have written this letter save for the fact that Massaro is not the only person to take such an attitude to linguistic qustions of international interest. Thus, in the recent discussion in *Science* on the problems connected with transliteration of Russian texts, the proposals advanced were largely based on English pronunciation, irrespective of the fact that certain letters or combinations of letters sound quite different in other languages. In deliberating such problems with a view to putting forth recommendations for international usage, those concerned would be well advised to consider carefully the suitability of their proposals in different languages.

S. ROZENTAL

Nordisk Institut for Teoretisk Atomfysik, Copenhagen, Denmark

Meetings

Forthcoming Events

August

1–12. Modulation Theory and Systems, Cambridge, Mass. (E. J. Baghdady, Dept. of Electrical Engineering, Massachusetts Inst. of Technology, Cambridge)

2–5. Poultry Science Assoc., Davis, Calif. (C. B. Ryan, PSA, Dept. of Poultry Husbandry, Texas A & M College, College Station)

3–6. Gas Chromatography (Infrared Spectroscopy Inst.), Nashville, Tenn. (N. Fuson, Fisk Infrared Inst., Fisk Univ., Nashville 8)

3–6. Rarefied Gas Dynamics, 2nd intern. symp. (by invitation only), Berkeley, Calif. (Engineering and Science Extension, Univ. of California, 2451 Bancroft Way, Berkeley 4)

5–6. Pennsylvania Acad. of Science, summer annual, Grantham, Pa. (K. B. Hoover, Messiah College, Grantham)

6–12. International Geographical Cong., 19th, Stockholm, Sweden. (IGC, Postfach, Stockholm 6)

7–10. American Soc. of Clinical Hypnosis, Miami, Fla. (S. Hershman, 6770 N. Lincoln Ave., Chicago 46, Ill.)

7–12. Gerontology, 5th intern. cong., San Francisco, Calif. (L. Kuplan, Intern. Cong. of Gerontology, P.O. Box 2103, Sacramento 10, Calif.)

7–13. Industrial Research Conf., Harriman, N.Y. (Miss M. F. Garvey, Industrial and Management Engineering Dept., Columbia Univ., New York 27)

8–11. American Astronautical Soc., Seattle, Wash. (R. M. Bridgforth, AAS, Propulsion Unit, Boeing Airplane Co., Aero-Space Div., P.O. Box 3707, Seattle)

8–12. American Inst. of Electrical Engineers, San Diego, Calif. (R. S. Gardner, AIEE, 33 W. 39 St., New York 18)

8–13. World Federation for Mental Health, 13th annual, Edinburgh, Scotland. (Secretariat, WFMH, 19 Manchester St., London, W.1, England)

8–20. American Soc. of Criminology, London, England. (D. E. J. MacNamara, New York Inst. of Criminology, 115–117 W. 42 St., New York 36)

9–13. Hail Storms, intern conf., Verona, Italy. (H. G. M. Ligpa, American Meteorological Soc., Stanford Research Inst., Stanford, Calif.)

11–13. Rocky Mountain Radiological Soc., Denver, Colo. (J. H. Freed, 4200 E. Ninth Ave., Denver 20)

11–16. Canadian Teachers Federation, Winnipeg, Manitoba. (G. G. Croskery, 444 MacLaren St., Ottawa 4, Ontario)

14–19. American Pharmaceutical Assoc., Washington, D.C. (R. P. Fischelis, APA, 2215 Constitution Ave., NW, Washington 7)

14–19. International Cong. of Clinical Chemistry, Edinburgh, Scotland. (S. C. Frazer, Clinical Laboratory, Royal Infirmary, Edinburgh)

14–20. Cardiology, 6th Inter-American cong., Rio de Janeiro, Brazil. (H. Alqueres, P.O. Box 1594, Rio de Janeiro)

15–16. National Assoc. of Boards of Pharmacy, Washington, D.C. (P. H. Costello, 77 W. Washington St., Chicago, Ill.)

15–17. Heat Transfer Conf., ASME and

AICE, Buffalo, N.Y. (A. B. Conlin, Jr., ASME, 29 W. 39 St., New York 18)

15–17. Organic Scintillation Detectors, intern. conf., Albuquerque, N.M. (G. H. Daub, Chemistry Dept., Univ. of New Mexico, Albuquerque)

15–18. American Veterinary Medicine Assoc., Denver, Colo. (H. E. Kingman, Jr., 600 S. Michigan Ave., Chicago 5)

15–18. Radiation Biology, 3rd Australian conf., Sydney, Australia. (P. Ilbery, Dept. of Preventive Medicine, Univ. of Sydney, New South Wales, Australia)

15–20. International Astronautical Federation, 11th cong., Stockholm, Sweden. (Secretariat, Intern. Astronautical Federation, 12, Bessborough Gardens, London, S.W.1, England)

15–23. Soil Science, 7th intern. cong., Madison, Wis. (R. Bradfield, Dept. of Agronomy, Cornell Univ., Ithaca, N.Y.)

15–24. Crystallography, intern. cong., Cambridge, England. (W. H. Taylor, Cavendish Laboratory, Cambridge, England)

15–25. Chemistry of Natural Products, IUPAC symp., Melbourne, Canberra, and Sydney, Australia. (Convener, Symposium Organizing Committee, Box 4331, G.P.O., Melbourne)

15–25. International Geological Cong., 21st session, Copenhagen, Denmark. (IGC, Mineralogical-Geological Museum, Univ. of Copenhagen, Øster Boldgade 7, Copenhagen K)

15–25. International Paleontological Union, Copenhagen, Denmark. (J. Roger, Service d'Information Geologique, B.R.G.-G.M., 74, rue de la Fédération, Paris 15e)

15–25. Sedimentology Cong., 6th intern., Copenhagen, Denmark. [General Secretary, IAS, c/o Institut Français du Petrole, 4, place Bir Hacheim, Rueil-Malmaison (Seine-et-Oise), France]

16–18. Biological Effects of Microwave Radiation, 4th annual conf., New York, N.Y. (M. Eisenbud, New York Univ. Post Graduate Medical School, 550 First Ave., New York 16)

16–19. Society of Automotive Engineers, San Francisco, Calif. (R. W. Crory, SAE, Meetings Operation Dept., 485 Lexington Ave., New York 17)

17–19. Hydraulics Conf., Seattle, Wash. (W. H. Wisely, American Soc. of Civil Engineers, 33 W. 39 St., New York 18)

17–19. University Nuclear Reactors, Gatlinburg, Tenn. (University Relations Div., Oak Ridge Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.)

17–21. Ionization Phenomena in Gases, 4th intern. conf., Uppsala and Stockholm, Sweden. (A. Nilsson, Fysikum, Uppsala)

18–19. Submarine and Space Medicine, 2nd intern. symp., Stockholm, Sweden. (H. Bjurstedt, Laboratory of Aviation Medicine, Karolinska Institutet, Stockholm 60)

20. American Inst. of Ultrasonics in Medicine, Washington, D.C. (D. M. Stillwell, Dept. of Physical Medicine and Rehabilitation, Univ. of Colorado Medical Center, Denver 20)

21–24. Latin-American Cong. of Angiology, Rio de Janeiro, Brazil. (R. C. Mayall, Caixa Postal 1822, Rio de Janeiro)

(See issue of 17 June for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ RECORDER of potentiometer type measures 11 by 14 by 4¾ in. and is designed for use on a laboratory bench or for wall mounting. It monitors d-c signals in the 10 to 100 mv range. Repeatability is said to be better than ±0.35 percent and full-scale response time 1.0 sec. Pen travel is 5 in. The pen drive used permits use of over-size strip chart or of circular charts. (Beckman Scientific and Process Instruments Division, Dept. Sci594, 2500 Fullerton Rd., Fullerton, Calif.)

■ FREQUENCY METER is a dual-range instrument reading either 396 to 440 cy/sec or 360 to 440 cy/sec from the same meter. Accuracy is said to be ±0.1 percent over the temperature range −55 to +65°C. The instrument uses semiconductor devices with no magnetic components. (Vidar Corporation, Dept. Sci604, 2107 El Camino Real, Palo Alto, Calif.)

■ TIMING BOARD for actuating three stop watches simultaneously permits timing of sequential steps with zero time loss between steps and without necessity for reading timer hands while they are in motion. Timing is started with the first watch at zero, the second stopped and ready to fly back, and the third in motion. Thus each step is timed in with the same motion with which the preceding step is timed out. (Heuer Timer Corp., Dept. Sci618, 441, Lexington Ave., New York 17, N.Y.)

■ PYCNOMETER permits determination of density of granular, irregular, or porous solids without use of liquids. The device operates by compressing air in two identical cylinders, one holding the sample to be measured contained in a cup. A third cylinder and piston communicating with the sample cylinder permits compensation for the effect of the volume occupied by the sample. This third cylinder is adjusted until the pressures in the two main cylinders, after compression, are equal. The motion of the third-cylinder piston required to effect the compensation is directly proportional to the volume of the sample. A choice of 50 cm^3 or 100 cm^3 sample containers is available. Accuracy of the instrument with a 50 cm^3 cup is said to be ±0.1 cm^3. (Houston Instrument Corporation, Dept. Sci620, P.O. Box 22234, Houston 27, Tex.)