SCIENCE

8 July 1977

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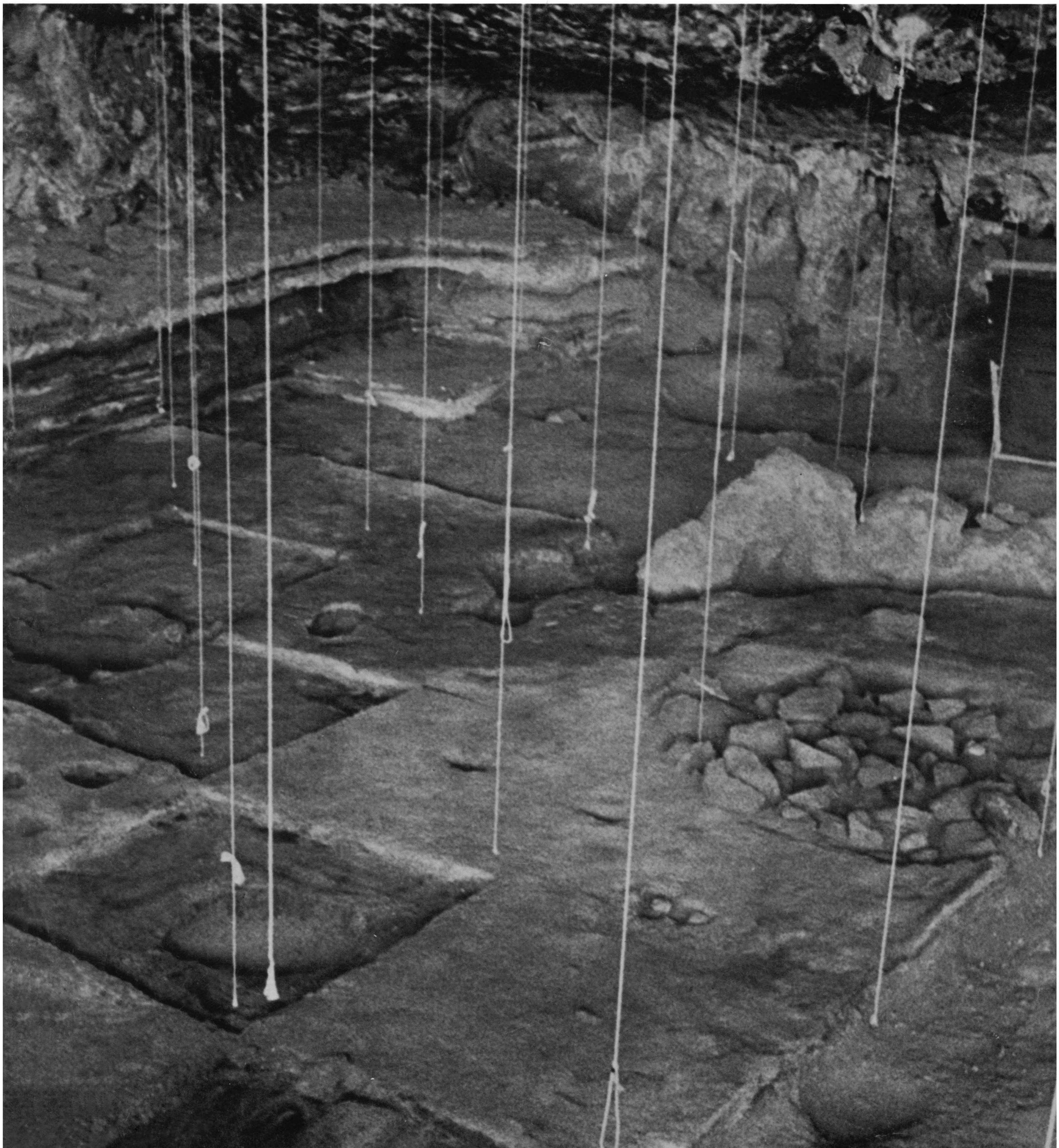
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE



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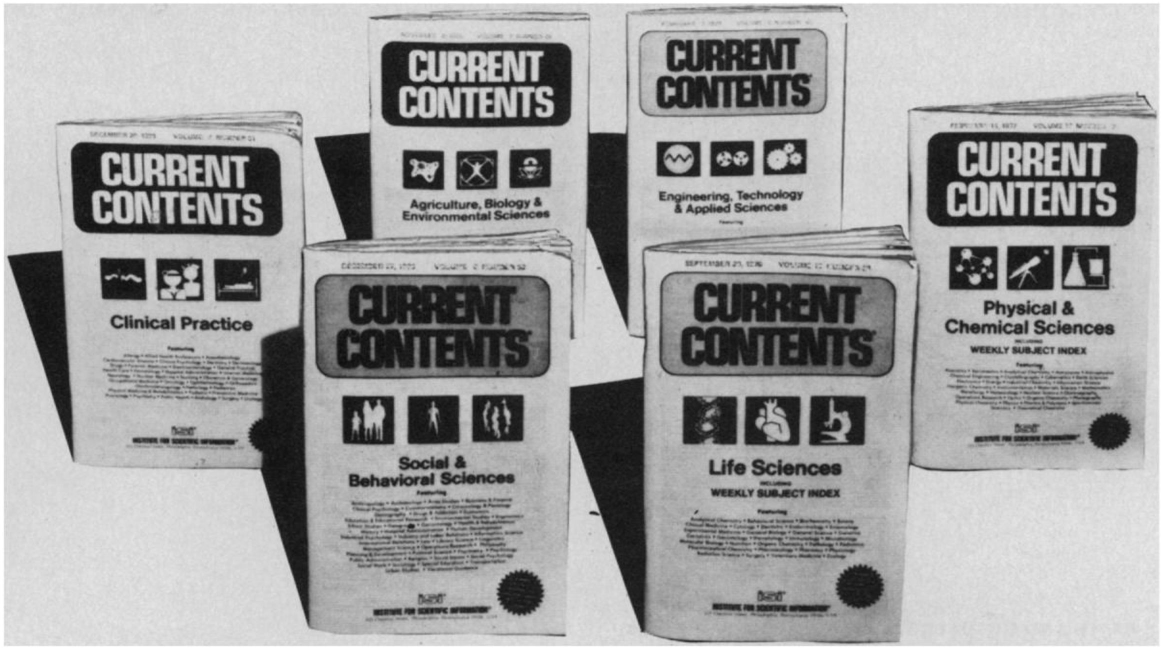

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SCIENCE

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AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

COVER

Excavation in Boomplaas Cave, Oudtshoorn District, South Africa, showing an occupation floor with fruit storage pits and stone hearth features which have been radiocarbon dated to between 1900 and 1700 years ago. The hanging strings, attached to the ceiling of the cave, demarcate 1-meter squares. See page 115. [H. J. Deacon, University of Stellenbosch, Stellenbosch, South Africa]

LETTERS

Leopard Frog Supply

The northern leopard frog *Rana pipiens* has been for many years the principal amphibian used for biomedical research and teaching. Unfortunately, such frogs of northern origin are no longer available in significant quantities and biological supply houses have turned to other regions, notably northwestern Mexico, as a source of frogs. The biology of these Mexican frogs is clearly different from that of the northern *R. pipiens*, a matter that was extensively discussed at a symposium on the laboratory frog (*1*), which was attended by major commercial frog suppliers. Recently it has been shown that, in the region of northern Sinaloa, Mexico, where most of these leopard frogs are collected, no *R. pipiens* are to be found. Instead, there exist two sympatric species of leopard frogs, *R. berlandieri forreri* and *R. magnaocularis* (*2*). Commercial collections made in this area from October through March contain about 80 percent *R. berlandieri forreri* and 20 percent *R. magnaocularis*. During other times of the year even fewer *R. magnaocularis* are found in these collections. Northeastern Mexico has now become an additional source of leopard frogs, and collections from this region are composed of *R. berlandieri berlandieri*.

For reasons unclear to us, commercial houses are now supplying Mexican leopard frogs designated as "*Rana pipiens berlandieri*." This is an unsatisfactory circumstance, not only because of the erroneous nomenclature, but because articles are beginning to appear about research, the meaning of which will be questionable, since the actual species on which the work was done is not known. Of more serious consequence is the fact that some investigators may think that they are working on *R. pipiens* when actually they are not. If there were no significant physiological differences among the various species of leopard frogs, the error in nomenclature would present no major problem; however, this is not the case. It has been pointed out in one demonstration of such physiological differences that melanophores of frogs of southern origin (*R. berlandieri forreri*) contain a preponderance of β-adrenergic receptors, while those from northern frogs (*R. pipiens*) contain mostly α-adrenergic receptors (*3*). Other physiological differences among leopard frog species include variations in reproductive cycles, in temperature tolerance, and in enzyme composition. The last is well manifested in the heterogeneity of isozyme patterns among leopard frog species (*4*).

We wish to emphasize that the various leopard frog species possess different biological characteristics that must be considered when they are used for experimentation.

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2. J. S. Frost and J. T. Bagnara, *Copeia* (No. 2), 332 (1976).
3. M. E. Hadley and J. E. Goldman, *Am. J. Physiol.* **219**, 72 (1970.
4. J. E. Platz, *Copeia* (No. 4), 660 (1976).

Engineering Ph.D.'s

A statement is made in John Walsh's article "The state of academic science: Concern about the vital signs" (News and Comment, 10 June, p. 1184) which I feel requires correction. Walsh says, "Engineering is in the midst of one of its cyclical booms in undergraduate enrollment but finds the opposite effect in its doctoral programs, apparently as a result of industry's current coolness to engineering Ph.D.'s."

It is true that engineering doctoral enrollment has been declining in recent years, but the evidence does not support the reason suggested by Walsh. Certainly there are some employers who are critical of Ph.D.'s, but surveys conducted by the Engineering Manpower Commission since 1970 show that 92 to 97 percent of engineering Ph.D.'s have been employed (or had other personal plans) by the time of graduation. This employment record is even better than that for B.S. holders in engineering, who in recent years have been 86 to 96 percent employed (or had other personal plans) by commencement time. Since only a third of engineering Ph.D.'s go to educational institutions and only 3 percent into postdoctoral positions, it is difficult to find in this fine employment record any evidence of industrial coolness.

A more likely explanation for the numerical decrease in engineering Ph.D.'s is to be found in the federal government's restrictive attitude toward training grants and a very negative report of the National Science Foundation (NSF) in 1971 (*1*). In that report NSF predicted that there would be a 40 percent surplus of engineering Ph.D.'s by 1980. However, the prediction was partially based on the assumption that the production of engineering Ph.D.'s would continue to increase during the 1970's. Overlooked was the fact that engineering doctoral enrollment had already begun to decline 3 years earlier, in 1968. Doctoral enrollment declined even further after publication of the NSF report. No surplus has yet developed, but it could not realistically be argued that there is a shortage, either. Perhaps the adverse prediction of the report, even though erroneous, prevented the occurrence of the very event it warned against.

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1. *1969 and 1980 Science and Engineering Doctorate Supply and Utilization* (NSF 71-20, National Science Foundation, Washington, D.C., 1971).

"Kerfuffle" Identified

The word "kerfuffle" questioned by Frank M. McMillan (Letters, 3 June, p. 1041) is recognized as a noun in volume 2 of the supplement to the *Oxford English Dictionary*.

"Kerfuffle" is also "curfuffle" (1813) and "gerfuffle" (1943). Kerfuffle was first spotted in 1959.

PHILIP B. JORDAIN
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McMillan appears to suffer from a deficiency of dictionaries. If he looked, he would find "kerfuffle" in the supplement portion of Eric Partridge's *A Dictionary of Slang and Unconventional English* (Macmillan, New York, ed. 7, 1970).

McMillan's brilliant analysis is merely another example of an unnecessary hypothesis based on a faulty premise.

MURRAY L. LESSER
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McMillan discusses the word "kerfuffle" as if it had been invented by Nicholas Wade. While I cannot give any definitive origin of the word, its meaning, to me at least, is perfectly clear and very expressive. I first heard it while visiting a friend in Uganda over a quarter of a century ago. The friend was British, an official in the Uganda Survey. He was describing what happened when a leopard got into his chicken coop. To me, it suggested a picture of chickens flying every which way amid great cackling and confusion. This is the picture evoked by the use of the word by Wade. A language grows through current use, and I would welcome this addition.

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The U.S. Birthrate

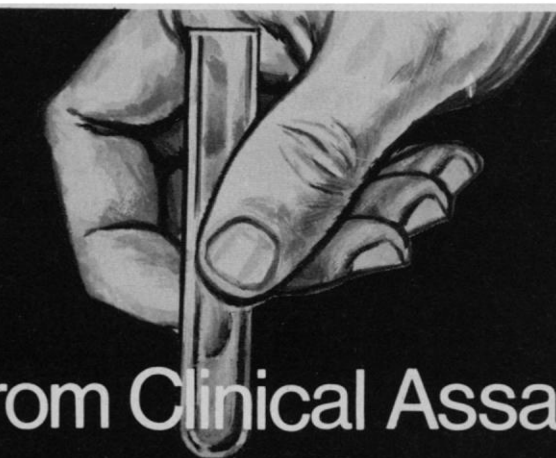
In our *Science* article of 29 August 1975 (p. 693) we set forth a number of considerations which led us to conclude "that the American birth rate may have bottomed out and that the country is likely to see a rise in reproduction." Campbell Gibson (29 April, p. 500) undertakes "to review [our] interpretations and to examine data pertinent to more recent fertility trends." For one, Gibson argues that our use of California data to predict the direction of national trends was invalid. While we indeed relied heavily upon California data, we did not, as Gibson suggests, merely extrapolate California's aggregated fertility rate to the country as a whole. On the contrary, we examined disaggregated rates for the state (by legitimacy status and race, age, and parity of mother) to gain insight into the internal dynamics that might be working to push up the birthrate. Because California frequently is a forerunner of national social and economic trends, we thought the state offered a significant clue to future demographic events in the country.

Gibson's finding that California's fertility experience has not been predictive of national trends comes from his use of a model that assumes the relationship between California and the rest of the country should have been the same before and after legalization of abortion. This procedure throws away important information about the advent of legal abortion in California. As Gibson's data show, between 1960 and 1970 California and the rest of the nation experienced similar annual changes in fertility; and after 1970, when a time lag developed between California and the nation in the availability of legal abortion, a time lag also developed in the decline of fertility. We recently showed that still another time lag had developed, this one in the renewed rise in illegitimacy, which now has appeared nationally, as it did earlier in California, as the influence of legal abortion has been overwhelmed by other social forces (*1*). These trends indicate that California's experience can indeed be useful for anticipating changes in national fertility.

Gibson agrees in general with one of our major premises, namely, that postponed childbearing among young married women is likely to be made up. He offers two different birth-timing models prepared as Census Bureau projections to show how much increase would occur in the total fertility rate assuming that the currently reported lifetime fertility expectations of these women are fulfilled. Both sets of projections, series II and series II-L, assume a relatively late mean age of childbearing. Series II generally projects current timing patterns, while II-L assumes an even greater postponement. Using II-L, Gibson shows a negligible increase in fertility. His results with II, however, indicate an increase in the total fertility rate of 10 percent between 1975 and 1980. This is in fact an increase of 17 percent in the crude birthrate (*2*). Gibson characterizes this increase as "moderate," which it may be when compared with the dramatic rise of the 1950's. Even so, the magnitude of increase shown by the series II projections has important economic and social implications which cannot be dismissed.

Moreover, the series II assumptions appear incomplete in that they do not allow for the possibility of a reversion to a pattern of earlier childbearing. But in our article we presented evidence from California showing that in addition to the making up of postponed births by older women, just such a pattern of more youthful childbearing was emerging in 1974. In California the detailed 1976 data show an estimated rise in the legitimate birthrate of 2.3 percent, with rises occurring among women over and under age 25 (*3*).

The final point in Gibson's article concerns the influence of economic conditions on fertility. He argues that "a substantial increase in fertility in the face of adverse economic conditions is unlikely." In our article we reported that California's birthrate rose in 1974 despite an unfavorable financial climate, and we cited this as empirical evidence that adverse economic conditions do not necessarily prevent women from going ahead with childbearing. We recognized that economic conditions influence fertility, but we considered that this influence has limits because of the unique role, discussed by Judith Blake and Kingsley Davis (*4*), that children play in people's lives. All told, how far the birthrate will rise or fall depends on the balance of a



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The Tris Controversy

The manufacture of infants' and children's sleepwear from fabrics containing the flame retardant tris(2,3-dibromopropyl) phosphate ceased in January 1977. The sale of such garments was banned in April 1977. The fear had been raised that hundreds of thousands of children who have worn Tris-treated garments have sustained damage leading ultimately to cancer.

The actions that led to the large-scale use of Tris and to its subsequent banning were taken in comparative haste. Disposing of the aftermath of the affair could occupy the courts for decades. Moreover, what has happened with Tris could be a forerunner of similar troubles for other products.

The next controversial issue with respect to Tris is deciding who pays for garments (worth $125 million) left on the shelf after the ban. Far more difficult might be the legal problems that may arise later when some individuals who, having worn Tris-treated garments, develop cancer and, blaming Tris, seek redress in the courts.

The large-scale use of flame retardants for infants' and children's sleepwear began in 1973. To meet new federal regulations for flammability, most manufacturers chose polyester into which the retardant Tris could be dissolved by a heat treatment. Tris was cheap, effective, and tests seemed to show that it was safe.

About a year ago, however, biochemist Bruce N. Ames, using his *Salmonella*/microsome test, found that Tris is mutagenic. Subsequent experiments by others with animals showed that Tris could cause kidney cancer in mice and rats. Other experiments with rabbits showed that Tris could be absorbed through the skin. In one instance, ^{14}C-labeled Tris was painted on a shaved rabbit's skin. In the next few days, a substantial fraction of the ^{14}C was found in the rabbit's urine. In another test, labeled Tris was placed on polyester fabric and later in contact with rabbit skin in the presence of unsterile human urine. Subsequently, the rabbit excreted ^{14}C in its urine.

It was on the basis of this chain of evidence that Tris was banned. There was no proof that Tris would cause cancer in humans or that the chemical when baked into a fiber would migrate from polyester through the human skin. However, considering the number of infants and children involved and the misery that cancer brings, the Consumer Product Safety Commission has no alternative but to stop the sale of Tris-treated garments.

When the controversies about Tris shift to the legal arena, some of the weaknesses in the chain of evidence are likely to be spotlighted. One that surely will receive attention is the applicability to humans of animal experiments. The strains of mice and rats employed had been bred especially to be cancer-prone. The untreated male mice employed as controls had an incidence of spontaneous liver cancer of about 50 percent. However, the incidence of spontaneous kidney cancer in these control animals was zero, and with large amounts of Tris, kidney cancer was observed. It was the male rat that was most susceptible to Tris-induced kidney cancer and this at Tris levels about 1 percent of those necessary to induce cancer in the male mouse. In calculating risk to humans, the incidence in the male rat is used. Does the susceptibility of humans resemble that of rats, mice, or neither?

Another question is, How many efforts have been made to detect transfer of Tris from garments through the human skin? The answer is that at the time the Tris ban was promulgated, observations on only two humans had been attempted. In neither case was transfer detected.

On the basis of the data available in May 1977, the hazards for humans of wearing Tris-treated garments are unknown. The hazards could be quite negligible. They might be enormous. Narrowing such a gap quickly will not be easy. There are too many unknown factors, such as the carcinogenicity of Tris for humans. We may have to await the passage of years to observe whether the incidence of kidney cancer remains unchanged or increases. And if there is an increase, we could not be certain of its cause, for other agents might be responsible.—PHILIP H. ABELSON

Deadline for Nominations: 15 September 1977
AAAS–Newcomb Cleveland Prize: Contest Year Is Nearly Over

The deadline for nominations of papers for the AAAS–Newcomb Cleveland Prize is fast approaching. Readers are invited to nominate papers published in the Reports section of *Science* from 3 September 1976 to 26 August 1977. The prize of $5000 and a bronze medal is now given annually to the author of an outstanding paper that is a first-time publication of the author's own research.

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 prizewinning 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.

Reports

Relation Between Earthquakes, Weather, and Soil Tilt

Abstract. *Two years of local earthquake, temperature, and rainfall data taken near a tiltmeter site were used in a study of the numerical relation between these phenomena and the recorded tilt response. A least-squares shaping and predictive error filter approach was used. The relations were ranked in part according to the root mean square (r.m.s.) error of fit across the entire sample space. The tilt data with an annual range of tilt of approximately 10 microradians were fitted to the combined weather data of temperature and rainfall with a 0.75-microradian r.m.s. error. The best fit of earthquakes to these same tilt data is the subclass of events with magnitude* (M) > 2.5 *within 30 kilometers of the tilt site. The filter that mapped earthquakes to tilt yielded a 1.03-microradian r.m.s. error. The most unusual tilt anomaly over the entire 2-year period has the best fit of rainfall to the data for any single month of the entire data set. This unusual anomaly was the basis of an erroneously predicted earthquake* (M ~ 5). *These data indicate that if there are premonitory earthquake signals, they are buried in local meteorlogical noise. Separating an earthquake anomaly from the response to surface phenomena becomes more difficult as the earthquake anomaly lead time approaches the rise time of the soil to weather and seasonal variations.*

The essential problem of earthquake prediction is to identify and monitor those physical characteristics of the failure process that uniquely foretell the magnitude, location, and the origin time of an earthquake. Identification of these parameters has tended to be heuristic. However, evidence is steadily accumulating toward the support of a sound physical basis for earthquake prediction. Models of the failure process are becoming sufficiently detailed to predict the spatial and temporal character of geophysical results found by measurements conducted in the epicentral region and thus to allow effective testing of these models against observations.

A popular instrumentation strategy for earthquake prediction is to deploy large numbers of relatively inexpensive tiltmeters, strainmeters, creepmeters, and other geophysical instruments (*1*). Most of these instruments are implanted at shallow depths in soil regimes near active faults. In some cases the sites are within the fault zone. Despite careful site selection, site preparation, and instrument emplacement (*2*), instabilities associated with the response of the soil to a variety of meteorological events can overwhelm the response of the site to premonitory earthquake signals of small events [magnitude (M) ≤ 5]. This is a particularly difficult problem if the rise time and duration of a meteorologically induced effect are comparable with the lead time signature for an earthquake.

Of all the meteorological variables, rainfall effects can be the most easily confused with anomalous earthquake precursors of small events because of the episodic nature of both phenomena. In an area of extremely low annual rainfall ($R < 20$ cm per year) a sudden burst of rain ($R \sim 1$ cm) has produced recognizable effects for more than 2 months duration with a lag between the input and response of the order of days (*3*). In areas of moderate to heavy rainfall ($R > 40$ cm per year), such as central California, detailed description of the tilt response to separate bursts of rainfall with separation time less than 2 months is difficult to achieve. The impulse response of the site to a spike of rainfall is different for different times of the year simply because the soil system has different properties at the time of each rainfall (*4*).

Figure 1 illustrates the variability of site response to rainfall at the Presidio site in central California. Clearly the instrument is responding to rainfall, but the incompleteness of the rain-tilt correlation suggests other influences as well. Early seasonal rains occur when the site is still in a state of partial desiccation after the annual 6-month period of no rain.

Other instruments besides tiltmeters respond to rainfall. Furthermore, responses differ from site to site and instrument to instrument. Figure 2 is a collection of geophysical data from a set of instruments sited along the central section of the San Andreas fault. They all exhibit anomalous behavior over the time bracketed by the dashed lines; this

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—RICHARD G. SOMMER

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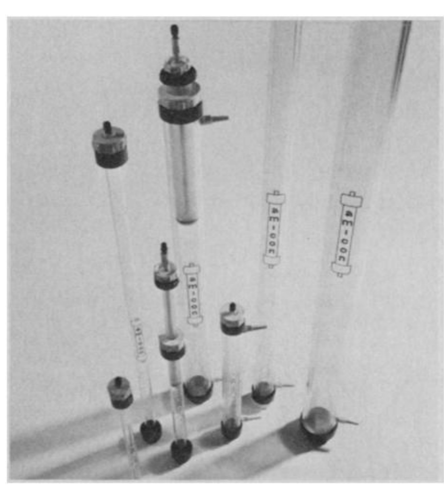

Iodine-Labeled DNA

Radiolabeled double-stranded DNA is prepared enzymatically by incorporation of iododeoxycytidine-5′-triphosphate, 5[^{125}I]. Single-stranded material is removed by treating the product with a single-stranded-specific nuclease, column chromatography, and Millipore filtration. The product is then tested for binding with antiserum against double-stranded (native) DNA. No single-stranded (denatured) DNA is detected using antiserum specific for single-stranded DNA. New England Nuclear. Circle 670.

Literature

Ultrafiltration Membranes and Systems includes apparatus for concentrating, desalting, and fractionating macromolecules and viruses. Millipore. Circle 677.

The Hologramaker lists a line of products for educational uses of holography. Holographics. Circle 678.

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