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## Letters

### Science in Spain

We find to the point much of the information provided by A. G. Verduch [*Science* **130**, 126 (1959)] in his comments on the article by David Gates on basic research in Europe [*Science* **128**, 227 (1958)]. But, although Verduch's comment on Spain is brief, it conveys the idea that research in Spain is "free"; that there is a free exchange of theory, methods, and ideas, when it is common knowledge that there exists a censorship which makes it difficult to receive information in such fields as political economy, evolution, and sociology.

Although much good work is actually done in Spain by our colleagues in the field of anthropology, this cannot, probably, erase the effects and norms of the regime. A few examples will suffice.

From *Manual de Antropología* (1946), by Pérez de Barradas, professor in anthropology: "There are anthropologists who still cannot do away with the evolutionist load" (page 12); and, "We think anthropology as a science should abandon the theme of the origin of man, and gallantly confess that it is not known when, where, or how man appeared on the earth, and also the utter failure of evolutionism" (page 25).

In volume 1 (1954) of the ten-volume *Historia Universal*, edited by S. A. Espasa-Calpe (one of the most renowned editors of Spain, if not the most renowned), in the chapter "The birth of humanity," there is a strong criticism of Weidenreich in which it is stated (page 101): "Today no one believes that man comes from an animal world of ancestors," and so on.

B. Melendez, professor of paleontology at the University of Madrid, and director of the department of paleontology of the National Museum of Natural Sciences, in the prologue to the book *Hacia el Origen del Hombre* (1956), by Valeriano Anderez Alonso, writes the same sort of equivocal concepts: "Organic evolution *within certain limits* [our italics] is a historical reality," and so on.

Since this is "Darwin's year," it seemed pertinent to us to provide the foregoing information and to give examples of the regrettable conditions that unhappily prevail in much of Spain's scientific life.

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### The Green Ray

Gerhart S. Schwarz says that the green ray can be seen only from an elevation but does not say how great an elevation [*Science* **130**, 276 (1959)].

I formerly had a shore cottage on the northwest shore of Oahu, and I have seen the green ray several dozens of times when my eyes were about 16 feet above sea level.

I have also seen the green ray from shipboard between Honolulu and California, in the North Atlantic Ocean, and in the Adriatic Sea.

Most remarkable, however, was seeing the green ray once from the observation platform at the rear of an eastbound train somewhere on the Great Plains.

HAROLD S. PALMER

*Honolulu, Hawaii*

In his letter concerning the green flash, Gerhart S. Schwarz says, "the green ray can be seen only when one views the horizon from an elevation, and this . . . plus factors of latitude, season, and weather . . . probably explains why few sea captains are familiar with it." This comment is misleading, for I have seen the green flash many times while sitting or standing on a beach or the deck of a ship. I have seen it also from planes and from mountain tops, and in latitudes from the Tropic of Capricorn to the Arctic Circle.

From an elevation, the green flash may at times be seen *before* the sun sets. On such occasion, two notches may be seen on the sides of the sun, and these two notches coming together at the top of the sun result in a green flash.

One may reasonably look for the green flash at sunset whenever one has a clear horizon to the west, and sea captains should have many opportunities to observe it.

CHARLES H. SMILEY

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### Stratospheric Fallout

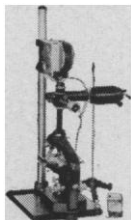
Fry and Kuroda have recently studied stratospheric fallout, using the ratio  $Ba^{140}/Sr^{89}$  as a parameter. Their report [*Science* **129**, 1742 (1959)] appears open to serious objection. The basis of the authors' thesis is their assumption: "Since the fission products remain in the troposphere for only a month or two, the fallout since December or January must have originated almost exclusively from the stratosphere." This assumption is not estab-



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lished; Machta, for example, in testimony before the Joint Congressional Committee on Atomic Energy (5-8 May 1959) states that *half* (my italics) of the tropospheric activity falls out every month. The clearing of the troposphere, with a 30-day half-life, cannot be considered complete while there is still measurable  $Ba^{140}$ , with only a 13-day half-life. Equation 1

$$[Ba^{140}/Sr^{89}]_t = ke^{-(\lambda_{140}-\lambda_{89})t}$$

which the authors use to describe their experimental results, can be derived solely on the basis that the contributions of the October-November 1958 test series, whether stratospheric or tropospheric, were in very large excess over pre-existing pools of  $Sr^{89}$  and  $Ba^{140}$ .

The extrapolated value of the  $Ba^{140}/Sr^{89}$  ratio (5.5 on 25 Oct. 1958) must be considered fortuitous, inasmuch as there is no particular reason to extrapolate to that date. An equally reasonable extrapolation might be to the middle of the U.S.S.R. test series, say 18 Oct. 1958, which would give an extrapolated  $Ba^{140}/Sr^{89}$  ratio of 7.3. The  $Ba^{140}/Sr^{89}$  ratio (5.5) in freshly produced  $U^{235}$  fission product mixture is not necessarily appropriate to weapon test fallout, which may well be derived from fission of  $Pu^{239}$  or  $U^{238}$ . The  $Ba^{140}/Sr^{89}$  ratio in fission product from either of these materials would be greater than that from  $U^{235}$  [S. Katcoff, *Nucleonics* 16, 78 (1958)].

A. S. GOLDIN

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Goldin claims that the basis of our thesis is the assumption: "Since the fission products remain in the troposphere for only a month or two, the fallout since December or January must have originated almost exclusively from the stratosphere." First of all, we wish to set the record straight by pointing out that such an assumption is neither necessary nor was it used either in deriving Eq. 1 or in arriving at the final conclusion.

As to the validity of the above statement per se, we might mention that Kuroda [*ANL-5920* (Oct. 1958), pp. 1-40] has estimated that 94 percent of the total  $Sr^{90}$  fallout was stratospheric during the period between 15 Oct. 1957 and 1 June 1958, at Lemont, Ill. In view of the fact that many nuclear explosions occurred during the above-mentioned period, whereas there was no explosion after the fall of 1958, it would be surprising indeed if the percentage of stratospheric fallout in the total fallout after December 1958 were not much higher than that in 1957-58.

In regard to Goldin's concern over "the clearing of the troposphere," we would like to emphasize the fact that our method is designed to work when "the clearing of the troposphere" is incomplete, as well as when it is complete. We have done this mathematically. Equation 2 leads to Eq. 1 when  $A_{T,0}^{*} \gg A_{T,0}$  and  $A_{S,0}^{*} \gg A_{S,0}$ —that is, when both the stratospheric and tropospheric contributions of the test series are in very large excess over pre-existing pools of  $Sr^{89}$ . It has to be noted that this occurs regardless of the value of the  $e^{(k_T-k_S)t}$  term in Eq. 2. We might add that Eq. 2 leads to Eq. 1 also, if either one of the following conditions is fulfilled:

$$A_{S,0} = 0 \text{ and } B_{S,0} = 0 \quad (I)$$

or

$$A_{T,0} = 0 \text{ and } B_{T,0} = 0 \quad (II)$$

However, none of these terms can be assumed to have been zero during the period between the fall of 1958 and the spring of 1959.

It is an oversimplification for Goldin to say that Eq. 1 "can be derived solely on the basis that the contributions of the October-November 1958 test series, whether stratospheric or tropospheric, were in very large excess over pre-existing pools of  $Sr^{89}$  and  $Ba^{140}$ ." Such an approach limits the usefulness of Eq. 1 and can result in additional confusion. We can demonstrate a good example of this in Goldin's next objection, concerning the extrapolation of the data.

To extrapolate the data, as Goldin suggests, to some earlier date would be meaningless because the stratospheric inventories of  $Ba^{140}$  and  $Sr^{89}$  have undergone considerable change between, say, 18 and 25 October, due to (i) new and undefined additions to the stratosphere during this time, (ii) radioactive decay, and (iii) fallout. After 25 October any changes were due to (ii) and (iii) only, which were used in deriving Eq. 2. The derivation of Eq. 2, however, does not take (i) into account, and any mechanical extrapolation of data not supported by theory only invites error in conclusions. Any extrapolation beyond 25 October will contain the error of disregarding the important factor (i).

Kuroda has explored the effect of fission of some materials other than  $U^{235}$  on the ratios of radionuclides, as suggested by Goldin. Our present data do not indicate that other materials were used, nor do they rule out this possibility.

LOIS FRY

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