Comments and Communications

Gravimetric Thermal Precipitator

I was interested to read the account by Kethley, Gordon, and Orr (*1*) of their thermal precipitator for aerobacteriology, since it is similar to one that I have been using in this laboratory for some years as a gravimetric dust sampler for animal inhalation experiments. The design of this instrument is shown in Fig. 1, from which it will be seen that it is of very simple construction. The aluminium hot plate (H) is secured to the aluminium case (N) by the steel sampling tube (A) (steel is used to reduce heat conduction) and nut (B). The heater coil (E) of nichrome tape is covered by an asbestos disc (D), which is secured by the plastic sleeve (C). The ends of the coil are silver soldered to copper wires (F) which are soft soldered to the terminals (G). The aluminium collecting plate (J) is secured by the knurled screw-ring (K) and rubber washer (L), the purpose of which is to take up small dimensional changes resulting from thermal expansion. The gap (O) between the hot and cold plates is about 0.015″ but need not be exact. It is best made by assembling the instrument and then facing the edge of the case and hot plate in one process on a lathe to ensure accurate parallelism.

Details of the performance of the instrument have not previously been published because there are a number of points yet to be worked out, but the following general observations may be of interest.

The performance of a thermal precipitator is governed almost entirely by the power input, and is independent within wide limits of temperature, air gap, or any features of design except those which ensure that as much as possible of the power input is transmitted as heat across the air gap, and that the air flow is uniformly distributed in it. These points can be readily demonstrated by making use of a heater plate with three adjustable projecting screws, and precipitating magnesium oxide smoke on to a glass surface so arranged that it can be watched from outside. It can then be seen that, for a given volumetric flow rate, the diameter of the deposit is fixed by the power input; so long as the deposit is smaller than the hot plate it may be presumed that precipitation is complete. The width of the gap can be varied from 0.005 to 0.0625 in. without appreciably affecting the diameter of the deposit, but if the plate is not parallel to the glass, the deposit will not be circular and dust will escape beyond the edge of the plate at one side. The diameter of the plate affects the diameter of the deposit, but does not affect the power required for complete precipitation at a given flow rate, which for a flow of 100 ml/minute is about 10 watts. It is interesting to compare this with the power required for the thermal precipitator of Green and Watson (*2*) which is about 1 watt for a flow of 6–7 ml/minute, and it would be valuable to have the corresponding

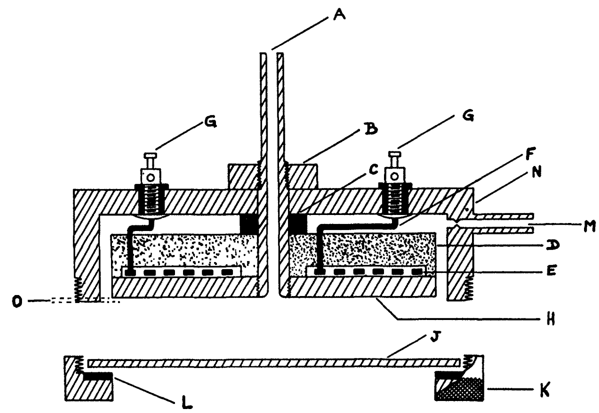

Fig. 1. Gravimetric thermal precipitator.

relationship for the instrument described by Kethley *et al.* and also that previously described by Bredl and Grieve (*3*). I have not attempted to measure the temperature of the hot plate since I found that the performance of the instrument was largely unaffected by whether it was water or air cooled. The temperature of the hot plate is controlled by that of the "cold" surface, and since the effectiveness of thermal precipitation varies inversely as the absolute temperature of the system, water cooling is not worth while unless, as in the case of Kethley *et al.*, it is desirable to keep the temperature of the whole system as low as possible.

I am indebted to C. N. Davies and W. H. Walton for advice on the theoretical aspects of this problem.

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Cosmic Cloud Hypothesis of the Origin of the Solar System

Under the above title Palmer (*1*) has recently very severely criticized all present-day theories on the origin of the solar system. As Palmer quotes from one of my papers and from a personal letter from me to him, I felt it worth while to consider the questions which he raises in his letter.

As I see the issues involved, there are two separate problems. The first one is a factual one: Are the existing theories able to account for all the observed facts? The second one is of a more philosophical nature. Is it scientifically justified to consider in detail a theory of which one is aware that it cannot satisfactorily account for all the data for which it is trying to account? Palmer's answer to both questions seems to be an emphatic "no." Personally, I feel that the answer to the second question should be "yes" and that although the answer to the first question is "no," the situation is not as black as it is pictured by Palmer.

To take the last question first, I feel that only by thoroughly discussing and exploring all possibilities, and thus also the not so very profitable and even the incorrect ones, it is possible to arrive at satisfactory theories. If science is to advance at all, it must needs be by the suggestion and criticism of theories, and it would in my opinion not be in the interest of science, if only "final" theories could be published—even if it were possible to judge prior to publication whether a paper could be considered to give a "final" theory. Palmer's criticism seems therefore to me to be far too severe and to be unscientific.

Regarding the first problem, I fully agree with Palmer that at this moment no completely satisfactory theory exists, and I hope within the near future to give a more detailed account of the reasoning by which I have arrived at this conclusion. However, none of the points raised by Palmer play a role in arriving at this conclusion. His points are mainly concerned with (i) the distribution of angular momentum in the solar system, (ii) the condensation process leading to the planets, (iii) the loss of material from the solar envelope, and (iv) the inclination of the axes of rotation of the outer planets. I do not wish to enter into a detailed discussion at this moment but I may just briefly mention a few points which to my mind are relevant and which seem to have been overlooked by Palmer.

(*a*) The distribution of the angular momentum in the solar system is difficult to understand, but only in as far as the sun is rotating slowly (*2, 3*). It is, however, likely that this problem is not connected with the origin of the solar system, but rather with the more general question of the relation between spectral class and rotation (*4*).

(*b*) If one assumes condensation in the solar envelope to be due completely to supersaturation of part of the constituents (*3, 5*), none of the problems mentioned by Palmer in this connection remains serious.

(*c*) In the discussion of the loss of material from proto-planets or from the solar envelope, Palmer does not seem to take turbulence into account. This would have changed his estimates considerably, as turbulence is probably the most important factor in the development of the solar envelope (*3, 6, 7, 8*).

(*d*) If one takes into account that the height of the solar envelope at the distance from the sun corresponding to Jupiter is at least 10^5 km (*3, 8*), one sees that the problem of the development of the protoplanets is really three dimensional rather than two dimensional and an inclination of the equatorial plane of the outer planets is no longer such an important problem.

Summarizing, it seems to me that even though no completely satisfactory theory has been developed, the situation has certainly been greatly improved by the publication of various theories such as those of von Weizsäcker (*6*), Kuiper (*8*), and Urey (*9*), to name only a few.

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The Somatic Mutation Hypothesis of Cancer Genesis[1]

COPIES of SCIENCE issued April 24, 1953, contain an article entitled "A Reconsideration of the Somatic Mutation Theory of Cancer in the Light of Some Recent Developments." No original data are presented and quotations only from selected papers appearing in various journals are included, one dated 1951 and the remaining 33 from previous years. The author, Mr. John C. Fardon, says: "In view of the experimental evidence collected in recent years, it may be concluded with some degree of confidence that the somatic mutation theory of cancer does not oppose the facts that have so far been brought to light." With this statement and opinion we most heartily disagree. In support of this contention we wish to call attention first to a later paper by Demerec and coworkers (*1*) which negates the papers by Demerec quoted by Fardon in support of the somatic mutation theory and, second, to personal work (*2–10*) appearing in the literature, reference to which was entirely omitted.

Demerec, Wallace, Witkin, and Bertani (*1*) reported in 1949 that earlier reports on the increased lethal mutation rate in *Drosophila* after administration of carcinogens could not be confirmed. "The variability from experiment to experiment became alarming and only occasionally was it possible to obtain confirmation of previous experiments. The fourth period, encompassing all the past year, has been characterized by uniformly negative results, except in those experiments using nitrogen mustard, methyl-bis

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