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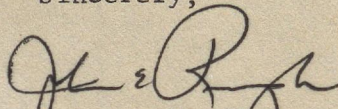
Dr. Krishna Bahadur
Reader, Chemistry Department
University of Allahabad
Allahabad, India

Dear Dr. Bahadur:

We have decided not to publish your paper on "Jeewanu".

The manuscript is enclosed.

Sincerely,


John E. Ringle
Assistant Editor

JER/so
encl.

From

Dr. Krishna Bahadur
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JUN 17 1969

Present Status

Canadian Commonwealth Research Fellow
Attached to the Physics Department
Sir George Williams University, Montreal, Canada.

To The Editor,
"Science",
Washington, D.C., U.S.A.

Dear Sir

Through your journal I wish to bring to the knowledge of the scientists interested in the problem of origin of life a few facts about our research on JEEWANU, the synthetic particles with properties of biological order, and thereby to clarify the misunderstanding which obscures a real picture of the life synthesis on the earth.

In 1963 we reported the synthesis of a type of particles which we named as Jeewanu (1,2). These particles grow from within, multiply by budding and have metabolic activity. These have been synthesised by exposing sterilised aqueous mixtures containing formaldehyde, citric acid or amino acids, biological minerals, a source of fixed nitric or ammoniacal nitrogen and molybdenum, iron or other metallic ions as catalyst, to sunlight or artificial light.

The photochemical formation of Jeewanu by us was confirmed by Dr. M.H. Briggs of England in a paper which he presented in the 4th International Congress on Photobiology held at Oxford in 1964. Another of his confirmations of some more of our experiments appeared in *Spaceflight*, 7, (4), 129-131 (1965).

I did some experiments on formation of Jeewanu from thermal peptide of Dr. Fox and it was not agreeable to him. He gave the work for repetition to his research assistant and to two of his friends Dr. R. Young and Dr. C. Ponnamperruma. The research assistant of Dr. Fox did observe the increase in size and number of the particles when she repeated the experiment as I suggested including the exposure part. She did not observe these properties when earlier she had tried excluding

the exposure part of the experiment. Dr. Ponnampereuma did not do any experiment at all. Dr. Young observed an increase in the number of the particles but thought it to be due to splitting during shaking.

The properties of growth and multiplication by budding in microspheres have been reported by Dr. Fox in 05 report of his institute at Miami in 1966. Professor A. I. Oparin reported these properties in coacervates in 1967.

In 1967, Dr. Ponnampereuma, without doing a single experiment on our work and reporting his findings, criticized our work in NASA Technical Memorandum-XI439. The memorandum is a misquotation of published literature, wrong reporting of our published data and is a complete misrepresentation of our work. The memorandum was brought to my notice by Professor A. I. Oparin in 1968 when he visited India. He insisted on my writing a detailed reply to the memorandum to clear the misunderstanding caused by this publication.

I wrote to NASA authorities objecting to the publication of the memorandum and requesting that my reply to this memorandum be published in the same periodical. I received a letter from Dr. R. Young stating that only the work of NASA scientists is published in the memorandum and as I am not one of them, my reply to the memorandum cannot be published and that he considers the memorandum satisfactory and that he has also received appreciation from some scientists. In December, 1968 I sent a copy of my reply to the memorandum to Dr. Ponnampereuma asking him to tell me where I am wrong but in spite of my reminder I have so far not heard anything from him. I, therefore, wish to clarify the points raised by Dr. Ponnampereuma in short through this letter in your journal.

It is reported in the memorandum that I believe life is inherent in matter. I do not (3,4,5). I think that matter has inherent property of duplication which can be observed under suitable conditions and a system of matter in equilibrium has the property of making such changes within it, if possible, so that a mild

constraint on it is partially annulled. This I express as adaptability. Under suitable conditions we can observe these properties of matter. These helped in the formation of particles capable of growth, multiplication and metabolic activity. Such particles are able to adapt to the milder changes in the environment and so could evolve (2,6,7).

It is mentioned in the memorandum that I define life in terms of growth, multiplication and metabolic activity. I do not define ~~as~~ living system or life. I simply enumerate a few properties which if present in a system together may entitle a system to be included under the category of living things, though one may have established living systems on other considerations and not having all these properties (8).

According to the memorandum I have not provided convincing evidence of growth, multiplication and metabolic activity. For growth, increase in the dry weight of the particles has been desired as evidence. Dry weight of 0.1157 gm of the particles has been reported in the very paper referred by the authors of the memorandum (9). For multiplication there is an inquiry whether it is by budding. It is by budding as is evident by even the title of the paper discussed in the memorandum in detail (9) which is "..... multiplication by budding...." The time lapse micrographs showing increase in size and multiplication by budding (10, 11) and data indicated by actual statistical counting have been published. Metabolic activity is indicated by the difference in amino acid pattern of the environmental materials and the particles as published in the paper referred by them (9). As the particles have a distinct boundary wall and an internal structure, the seat of reactions cannot be in the environmental medium or the outer side of the boundary wall of the particles but can only be inside the particles (13, 14).

It is stated in the memorandum that the amino acid analysis has been done on one way paper chromatography only. This is not so. The identification of amino acids was done by simultaneous running of known amino acids with different running solvents in one way chromatography, by two way chromatography (15, 16, 17, 18, 19) and also by chromatographic and chemical separation (18, 20) and identification of the separated fractions chemically.

Then there is a reference to our work on the formation of Jeewanu from thermal peptide. The background of this work has been published (21, 22, 23) and interested persons may read these. It is mentioned in the memorandum that increase in mass is not rigorously demonstrated. The increase in the dry weight has been recorded as mentioned earlier.

The memorandum mentioned that Young and McCaw did not observe an increase in size. Young definitely observed an increase in the number of the particles. Did he observe a decrease in size of the particles? The size of the individual particle does not increase beyond a certain limit. Then it buds and the bud increases in size reaching the maturity size. The mature particles do not increase in diameter indefinitely. This reported result of Young has neither been published anywhere nor been shown to me so I cannot say what he observed and what he did not.

Young suggested that the increase in the number of the particles is due to shaking. What about the increase in ~~the~~ dry weight of the particles? The increase in the number of the particles is definitely not by shaking because in the time lapse micrography where the particles are kept stationary for several days, increase in the size and multiplication by budding have been observed. Particles can be formed spontaneously but conditions for this type of formation had been suppressed to the maximum and as reported in the paper chiefly referred to by the authors of the memorandum (24) the increase in the number of the particles and their size take place only in the seeded mixtures and identical non-seeded mixtures do not show the presence of a single particle in subculture experiments.

It is said in the memorandum that Dr. Fox reported that proteinoid microspheres can form buds which are detachable by agitation, and whether this mode of multiplication also occurred in Jeewanu is an open question. Multiplication by budding only has been reported in Jeewanu as indicated by even the title of the paper (24).

It is further reported in the memorandum that no convincing evidence for metabolic activity has been produced. If the materials of the particles are formed inside each particle and these substances are not present in the same form in the environment, the chemical transformations effecting their formation could be called metabolic activity. The particles grow in size and increase in number by budding. Where can the seat of reactions be? Not in the environment because how can the insoluble substances go inside the particle which is encircled by a boundary wall? Not on the outer side of the wall because it would have only thickened the boundary wall which is not the case in the growth of the particles. The seat of reaction leading to the formation of the materials of the particles is inside the particles and these reactions may be taken as metabolic activity.

Once reproduction is established, i.e. younger units coming in existence through the parent ones and their increase in size to maturity and subsequent reproduction is observed, metabolism is there, as suggested by Professor J.D. Bernal in the following words (250)

" They are not so to speak metabolism and reproduction; metabolism is reproduction " .

Evidence of energy metabolism as indicated by the decrease in the total weight of the organic constituents of the mixture due to combustion with time has been reported (26).

The memorandum reports it is not apparent why the substances in the nutrient medium were included or why they varied in relative amounts from experiments to the next. The reasons for including each and every chemical in the nutrient medium have been given in the very paper which has been referred to by the authors of the memorandum (24). The paper also mentions the reasons why the relative amounts of the chemicals were changed in the experiments (27). I wonder how they missed these.

The memorandum has mentioned that though we sterilised the mixtures we did not sterilise the seed mixtures. The aqueous mixture in which the seed for subculture experiments were prepared was sterilised and the peptide which was used for this preparation was sterilised by dry heat at 120 C (28). Can one imagine what is meant by sterilising the seed mixture in subculture experiments! How can one use it as seed after it has been sterilised?

The abiogenic aspect of the problem of origin of life is almost clear. The study of the molecular associations which may have the properties of biological order is of considerable interest. In 1967, Professor A. I. Oparin has summed up the situation thus. When peptide and protein-like substances are formed in water, they combine with other compounds and come out of the solution in the form of small particles. These can be observed through a microscope. They have internal structures and they grow bigger from small size. They multiply after becoming big in size. They show adaptability and cellular organisms were formed as a product of evolution of these particles.

With regards

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4.3.69.

Yours sincerely

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