Berkeley, 8 December 1970

Dear Krishna,
I carried out hydrolysis using 1 N instead of 6 N HCl and then gave the sample for amino acid analysis. There were amino acids detected and the peaks were fairly close to the standard peaks for amino acids. For example, we detected glycine at 42 minutes and the standard comes at 40 min. The amino acids aspartic, serine, a trace of glutamic, glycine, alanine. However, they were present in very small amounts about 0.002-3 μ moles. This may still be a result of the hydrolysis procedure and the presence of salts in the hydrolysate. To check these results, I gave the analyst a sample of the same preparation but hydrolyzed in 6 N. It gave no detectable amino acids. Therefore it seems that the strong acid destroyed the acids. Perhaps 2 N would give more acids than 1 N, or perhaps desalting is necessary. The amino acid man said that he thought it may not be necessary for his apparatus.
In order to get around this possible salt problem and get faster results, Lemmon and I are thinking about using gas chromatography. By means of some new techniques, the amino acids can be prepared for the g.c. in just a few minutes. At least, that is what the advertisements say. One makes a N-trimethysilyl-(TMS)- Amino acid.

I am now trying runs with the mineral water and also with sea water. The run mentioned above used a $MgSO_4$ solution (0.80 gms/100 ml). Maybe the mineral water and sea water will yield bigger amino acid yields. But the trouble may have been with using too dilute a hydrolysate.

I am confident that good results will be forthcoming but it takes a lot of patience.

Did you notice that Fox published a paper in Science in which he reports amino acid formation from heating of formaldehyde and ammonia. The yields reported are extremely low and perhaps even lower than the yields we obtained above. I don't see how referees can accept such a paper from him.

When the procedure is tied down, I am going to get an elemental analysis of the particles. The guy who was supposed to make a scanning electron micrograph has not done anything yet. Same od story probably.

In order to get some interest in the subject, we have to address ourselves to problems people are really interested in. I don't know if I told you that Dean Kenyon and I have written a review of ~~the~~ neobiogenesis= the present formation of life. It actually seems like a much easier problem to get life going from highly evolved biomolecules than from simple molecules like we use. We are going to send in the paper for publication. If it is accepted, then it means that there is interest and we will start experimentation. Neobiogenesis has great medical importance. We hypothesize that many different types of small organisms such as viruses, mycoplasma, and some others are probably being formed de novo in our bodies. Sterile sera itself gives rise to a great many microstructures which can even be passed from one plate to another.

I have a feeling that this work will get support because the applications are immediate in the medical field. Along these lines, why don't you continue the work on formation of peptides. The work you did with Perti. Incidentally, I don't have Perti's papers with me here so I would appreciate it if you could send me a brief descritpion of the work you did together. I am sure that

if your peptide work can be linked in something in molecular biology , you will get good support. Something like determining the structure of the peptides. This will interest the molecular genetics guys. What do you think of all this ? Regards to Rangan.

Adolph

P.S. I am tired of banging my head against an apparent stone wall so that is why I am throwing out these suggestions to interest people. After all, origin of life work in the 1860's really formed the foundation for modern medical practice, sterility. ~~Mxxxxxxx~~ When I say origin of life work in the 1860's I mean the spontaneous generation stuff of Pasteur. It went too far in its generalizations but did good anyway. Now it the time to go back to reexamining this fundamental premise of medicine.

SIR GEORGE WILLIAMS UNIVERSITY
DEPARTMENT OF PHYSICS
1455 de Maisonneuve Boulevard, Montreal 107, Quebec, Canada
Telephone (514) 879-5915

September 8, 1973

Letters
New Scientist
128 Long Acre
London WC2 9QH

Dear Sir,

In their article "Is anyone out there ?", Mitton and Lewin (16 August) use the importance of the metal molybdenum to the functioning of biological systems as an argument for Directed Panspermia. The authors seem to be unaware of the work of the Indian chemist Krishna Bahadur who has used molybdenum oxide as a catalyst for the photosynthesis of amino acids under assumed prebiotic conditions (Nature 182 , 1668 (1958)). Therefore since molybdenum has been shown to act in a natural way to promote formation of amino acids, the argument for directed pansperimia using molybdenum cannot stand in its present form.

Sincerely,

Adolph E. Smith

Adolph E. Smith

Dear Krishna,
I sent this to New Scientist. Thanks for the reprint from the fourth origin of life congress. Please send me the recent Zbl. reprints.
Adolph.

Physics Department

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Tel.

concordia university

May 20, 1977

Dear Krishna,

Congratulations on the marriage of your daughter. Time flies. I remember her as a adolescent.
I enclose a reprint which will probably interest you. It shows that Mo concentration could be made may times higher by means of air-sea bubbles. This article appeared when you were imprisioned so you may not be aware of it.

How is your asthma doing ? I hope that you are feeling better. Please try to cut down on smoking if you have not done so already.

Recently, I am getting back to origin of life work. This field is really the most important single topic I can think of. As you know, I don't have much facilities here. In fact, nearly none. Now I am trying to see how primitive photosynthesis began. The reason is also practical. If I can get some good results, I will apply to"Energy Research and Development Administration" in the U.S. to get some money. Do you have any ideas along these lines. Am working with $K_3Fe(CN)_6$ under ultraviolet radiation.

The poltiical scene is really bad and I am looking hard for a new job.

The best,

Adolph

Adolph

239-9

National Aeronautics and
Space Administration

Ames Research Center
Moffett Field, California
94035

NASA

29 Sept. 1978

Reply to Attn of:

Dear Krishna,

I received your letter in which you enclose the Jeewanu particles, and started the procedure for preparing some more. In the directions for the mineral solution I ran into some difficulty. Is the ferrous sulfate added to the mineral solution. When I did so, the solution turned cloudy and could not be made clear by heating. So I assume that the $FeSO_4$ is added later and made the mineral from just the first five salts. You say that these particles contain only iron. What happens to the molybdenum from the ammonium molybdate solution ? A 1 gram yield is quite good.
Right now I am wondering about the best way to test the particles for photochemical activity. There are two things which come to mind. 1) Suspending the particles in sea water of a mineral solution and irradiating with UV or visible light and looking for a pH change. 2) Suspending the particles in a strong buffer (probably at 7) and irradiating and looking for a change in the redox potential. Do youhave any thoughts along these lines ? In preaparing the particles, I don't think that I will do any autoclaving or worry about aseptic addition of formaldehyde unless you think that the heating from the autoclaving can aid the chemical reactions.

I spoke to Dean Kenyon about a publisher for your book. He suggested Freeman in San Francisco and was going to think about some other possibilities. The problem is that the whole field is going stale because of lack of progress. And the lack of progress is due to the influence of the establishment characters. The very name"chemical evolution" is bad. It implies that a series of chemical reactions alone will lead to the formation of a living organism. We have to think along the lines of functioning. The important questions whould be, what can all these particles do ?" and not "what are they made of ?"/ There is a man at the University of Hawaai , A Dr. Clair Folsome of the Department of Microbiology who is thinking along our lines. He is also having a book of origin of life published and is espousing the functional approach. I enclose his address so you can write him.

Clair worked at NASA here on the microstructures which are formed in the Miller-Urey experiment and he is coming to visit here some time in October. Incidentally, when I spoke to Clair on the phone a few weeks ago, he recommended adding acetate to the sea water so as to get some lipids. Is that why you use the calcium acetate in the mineral solution?

Now that I am back in origin of life work, I expect that the letters between us will start flying fast.

~~In regard~~

In regard to the vitamin E, it is best to take them right after a meal because they are fat soluble and best digested with the stomach has food in it. I will send you some more vitamin E from here.

I received tha chapter on neobiogenesis and Dean Kenyon and I are going to go over it together. I will then send you our reactions.

When you write to Dr. Hall, it might be a good idea to point out the possible application of the ferrodoxin-like activity to the solar energy program. As you know, the solar energy program has a high priority in the science programs of every country so any relation to that would be of great interest. I feel that the relation of oirigin of life work to solar energy is the way to go.

I am looking into the possiblity of getting a cheap fare and maybe I will find one. I am getting paid in Canadian dollars from my universit in Montreal and unfortunately that is a near disaster. In just 6 weeks, the Canadian dollar has fallen from $0.88+ to $ 0.85 . That is a big drop on a thousand dollars.

My best,

Adolph

Dr. Clair E. Folsome
Dept of Microbiology
Univ. of Hawaii
Honolulu, H.I. 96822
U.S.A.

239-9

National Aeronautics and
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Ames Research Center
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NASA

Reply to Attn of:

17 October 1978

Dear Krishna,

Since last writing to you I started repeating your 1958 work in which molybdenum oxide sol and paraformaldehyde give amino acids with atmospheric nitrogen fixation. To make the sol, I saturated a water solution of molybdenum trioxide and then passed the sol thru a 0.45 micron filter. The resulting solution or rather colloid was used. Then I also used several light sources. ONe was an ordinary incandescent light, 150 watts at about 15 cm distance, a 365 nm source obtained by using a medium pressure mercury light and putting the mixture in a Pyrex flask, and the third as radiation at 254 nm using a low pressure mercury penlight. Then after four days of irradiation I took samples. The control kept in the dark showed only the faintest ninhydrin reaction. Then the visible showed posttive ninhydrin but this was less than the 365 nm which in turn was less than the 254 nm. In the case of the last two, the products of the 254 reaction flask advanced more rapidly on the chromatogram than the other two. I put back the mixture and it is now being irradiated further.

The next step is to set up a test system whereby the products can be rapidly identified in a quantitative manner. I will start using gas chromatography after making derivatives of the amino acids. This work is new to me and I have never done it before so I expect that it will take a few weeks, with luck. Around here, people work by themselves. But I hope that when the results start coming in, people will get more interested and other people start working with me. Anyway, I look at the good side.

While setting up the test sytem, I am going to start using a combination of metal oxides. Since there is a lot of work on the Mo-Fe-protein complex being important in nitorgen fixation, I am going to add the ferric oxide colloid to the mixture next. I would also appreciate directions on how to prepare this colloid. Right now, I plan to do by filtration.

I am enclosing a copy of an abstract of a German patent issued in 1957 on the fixation of ammonia. Since MoO_3 was used it may be interesting for you. The whole question of nitrogen fixation from N_2 has commercial possibilites so it is a phase of theorigin of life question that may gain favor . Also is NH_3 the first product formed?
Why did you and Perti drp the topic of atmospheric nitrogen fixation ? Where is Perti today ? OK,I hope.

Aside from the hard work, I enjoy the warm climate of California very much. It is such a pleasure to have the warmth. Right now, it has been snowing already in Montreal and they are shivering. I wonder how I will be able to go back to the cold north after enjoying the climate here.

On the financial side, I have a bit of trouble, The canadian dollar has fallen precipitously in the past two months. When I cam here in August, it was worth 0.88 US dollars and now it is worth only 0.84 and falling fast. On a $ ¾ 1,000 a month, that means $ 40. Oh well, if the atmospheric nitrogen fixation comes thru, maybe my finances will improve. Did you ever consider getting a patent on that MoO_3 work ?

Right now I am also looking for some naturally occuring mineral(containing Mo and Fe and preferably some TiO_2 also) that will function in the fixation. If this can be found the process would be cheaper than using reagents from the bottle. Molybdite, MoS , is supposed to be common. I will ask some geologists about this.

I just bought some vitamins and will put them in the mail today.

With all my best

Adolph