Feb. 10, 1919

My dear Dr. Cashman:

My visit to Boston was a fine one in every way and I learned much. I was much pleased to see you, your family and your home workshop. You are nicely fixed, and I hope you will continue your studies of the Foraminifera from the Recent to the Cambrian.

Please remember me to your wife and to thank her for the good hospitality.

Yours truly,

Charles Schuchert.
Dear Professor Schuchert,

your note is here this morning, and I assume, you that the enjoyment of our meeting was mutual. The impression you made on my family was such that they are looking forward eagerly to seeing you again in the near future for a longer visit.

I am wondering a bit as to one or two things.

First of all as to the remark you casually made as to my thinking of the Bishop Museum. Secondly the effect on your mind perhaps of my apparent fixity in Sharon and Boston. And again the effect of the second on the first.

If by any chance you had really in mind the possibility of my being able to measure up to the requirements I want to seriously consider it. Here I have had varied outlets in museum work but have not had until lately a real opportunity to carry out my own ideas and plans. As a result the energy of which I seem to have an unusual supply has gone into my research and the developing of the place in Sharon. I really believe that I could make good in the Bishop work especially after reading through yesterday the series of Annual Reports. Their great lack has seemed
to be in the development of marine zoology which is I suppose the one thing I am most about. With such a wealth of material at hand and means to carry through comprehensive plans I can see the possibilities both for exhibition, accumulation of study collections and research, especially for research on living faunas and endemys problems, etc. Hawaii and the adjacent Indo-Pacific have wonderful opportunities. It is there that conditions should be studied to correlate conditions of deposition of Cretaceous and Tertiary for many of the genera of the Cenozoic and Oligocene Crabs and shrimp as least are not living only in the Indo-Pacific.

One of my great desires since the short opportunity of work in Jamaica has been to see some of the semi-tropics. The reefs at Humacao Bay next an em-increasing undeveloped to me, and I regretted having to leave them. One of the greatest gaps in the fauna work is the very slight knowledge of the living animals, especially the large so-called "colonial" forms of the tropics, Ophiuroidea, Nereididae, etc.

So if you were at all serious in considering me please do not let any impressions you may have had turn away in other directions without further consideration. If you think it worth while I should be glad to come to New Haven and get more light on the whole subject.

With kind regards,

Very truly yours,

[Signature]
My dear Cushman:

Balloway sent me a copy of his interesting paper "The Change in Ideas about Foraminifera", and I have been reading it. I was surprised to see nowhere a reference to you, and all the more since I have always held you were the natural textile method to the study of forams. Our Balloway seems to think it appears as if Schuchert takes the honor. In any event you deserve equal credit in the forams. I should have been taken pride in by an American! Is Balloway or jealous of you that he can't speak straight? I see this is at least true, that you are a foraminifera stand head and shoulder above all others!

Yours truly,

Charles Schuchert.
CUSHMAN LABORATORY FOR FORAMINIFERAL RESEARCH.

October 28, 1928.

Dear Doctor Schuchert:

I am afraid your diagnosis of the case is correct. There seemed to be a studied attempt to leave me out of the picture in regard to work on the forams, so much so that I had several indignant letters from those who heard the paper in San Francisco. I have a very good sense of humor so it reacts on the author in anything.

Just now the same source is trying to discredit the theory that the arenaceous forams are primitive and dominate the Palaeozoic. It all fits so nicely into my theories of descent that they must be knocked out if possible. It is going to be difficult to change the facts however and more and more light is coming all the time to show the characters of the early forams. Douville's theories which I have followed are very close to the truth as is evidenced by the study of the Palaeozoic especially.

Autogenous studies were made by Munier-Chalmas and by Schlumberger in the 80's, and by Schubert after 1900, but only in small groups, and I believe no attempt was made to apply such studies to the whole group of forams. The new book has taken exceedingly well both here and abroad, and I believe is being used as the text book in all the universities teaching micro-paleontology except Columbia. Also most of the Columbia graduates have it, and so I have no fault to find. Some of the best European workers have taken it over bodily and papers will soon appear over there using it in toto. That is really more than I expected. So you see I really can see a ludicrous side to the ignoring process on the one hand and the efforts to discredit on the other.

All this is not the sort of cooperation I have given and received from other workers at home and abroad, but I am putting up with it patiently.

Dunbar is coming up this week to see the laboratory, and what we are doing, and I do wish we might have you up sometime.

With my very best regards,

Sincerely yours,

J. A. C.

(A copy of the letter to Schuchert written by J.A.C. by hand.)
Dr. J. A. Cushman,
Sharon, Mass.

My dear Doctor Cushman:

The July 14 number of Nature has on pages 43-45 a long review of Galloway's book, and incidentally you are brought into the picture, by Heron-Allen. It again shows what is so well known outside Great Britain, namely, that anything emanating from a source other than British is no good!

The object of the review is to show that Americans are crazy in making species and genera, and that the British old-fashioned view of classification is being sadly neglected. It is evident to me that the British students of foraminifera are out of date and decidedly old-fashioned.

Heron-Allen could say the same of recent brachiopod work, since new genera are now being made at the rate of about 100 a year. But here the British are also in the game, as is evidenced by Buckman, Sahni (an Indian educated in England), and Muir-Wood.

But the ammonites are the best illustrations of genera making no the minutest characters, and here the Europeans and British have long been the leaders in showing the new way to the best methods of classification.

I hand these suggestions to you, since you may want to reply to Heron-Allen, and he richly deserves to be told that he is a very back number and that we Americans can take care of ourselves.

I have learned of this British attitude in my work on the Permian, and now Galloway and Cushman are shown where they are out of step.

Cushman recognizes 558 genera, 79 subfamilies, and 47 families
Galloway " 542 " 61 " 35 "

This is not much difference among Americans, but horrors! it is American!

Yours truly,

Charles Schuchert.
August 10, 1934.

Prof. Charles E. Schuchert,
Yale University,
New Haven, Connecticut.

Dear Doctor Schuchert:

How I can always depend upon you to come to my aid! It is one of the most precious possessions that I have. My appreciation of it is only equalled by my very inadequate ability to express to you how much it has meant to me many times in the past and now again.

I have waited answering your letter until I had a chance to see the article in "Nature" which I have now read. Other letters meanwhile have come, all in the same view. One from an Englishman of the generation after Heron-Allen blows him sky-high and apologizes for the incident as entirely un-English, and also condemning "Nature" for publishing it.

What made my resentment particularly bitter was the fact that I have had the most cordial relations with Heron-Allen I supposed. He has lost much of his wealth, I suppose. About two years ago his brilliant daughter who had won double honors in Zoology at Oxford was killed in an automobile accident, and in the last year, he has broken his friendship with Earland after twenty-five years, and is evidently very bitter. All this is however no real excuse for bringing in such an attack in the review of another person's book. However, except for the cause to shudder in a few quarters, it amounts to little. I had rather have your letter and some of these others as a net result.

As I look back, most of what I am is due in a large measure to English opposition. I have twenty ancestors in the group who came to Plymouth on the Mayflower in 1620. Some addition came from Huguenot dissenters as well. My inbred New England ancestry was thus largely a result of English opposition. Various Revolutionary soldiers carried on this same tradition, and again in 1812. Dr. Richard Rathbun once told me years after about the appearance of my first part of Bull. 71 on the Pacific forums. He received two scathing letters from England, attacking the Smithsonian for publishing any such paper which had upset some of the Challenger Report which "was a standard for all time." He told me he threw both letters in the waste basket unanswered, and then and there resolved to back my work just as far as he was able. So English opposition again gave me a decided boost rather than otherwise, I used what influence I had to get the Smithsonian to publish Heron-Allen's paper on Lister's observations of the life history of the forums when it was turned down by the Linnean Society of London. And now this gratitude!

Well, these things should be a stimulation I suppose. After all, one has to live with oneself, and must set one's own standards of scientific and other truth, and so criticism that seems unwarranted shouldn't hurt.

Thanking you again for your kind interest, past and present,

Very sincerely yours,

[Signatures]