Thursday, 14 October 1920.

Dear Curator:

Please read this note from the Doctor even if you consign to your vest pocket for future reference the succeeding volume from Marjorie Daw.

The work on your material is progressing on schedule time. I allowed one month for rough cleaning and all the heavy work, and my first month (ending 20 Oct.) will see all of the specimens cracked out. I have never put in such a continuous period at hard physical labor as I am now putting in. I have six big callouses and a blister on my right hand; yesterday I trimmed a chunk of flesh out of the thumb on my left hand with the trimming hammer – it was so stupid of me; I pride myself on never pounding my thumb or fingers or missing a stroke on the chisel, but I foolishly tried to trim a small specimen which wasn’t big enough to hold properly and I trimmed myself instead of the specimen. Though you may not think it, I am deriving great satisfaction from the work. There are times in life when the hardest kind of physical labor is a real mental boon. The present is such a time in my life and I am digging more than ammonites out of these nodules. Today I tackled the last big concretion – that one with your prized and solitary *Nautilus* from Constancia. I have the *Nautilus* all worked out and he looks fine.

The next month I shall devote primarily to the final cleaning under binoculars and to the preparation of sutures getting specimens ready for photographing. I expect also to do some identifying along the way just as I have already done. You must not expect to see any really wonderful specimens for all are more or less broken and incomplete but I think that they make a good showing considering the nature of the matrix and the destructive influences at work in fossilization.

It is very important that I have by the middle of November the list of localities – you know, your own numbers H-6, C-41, etc. Did you perchance take the list with you along with your other Cuban material? It will help me a great deal of know the geographical position of the material, for I am thinking of the stratigraphy as well as the palaeontology.

On your labels and in speaking to me you have referred to lower, middle and upper Jurassic by which meant, of course, only the three divisions of Jurassic as recognized in the field without reference to actual correlation with the standard Lower, Middle, and Upper Jurassic. So far everything has fallen into one or another zone of the Upper Jurassic – Oxfordian, Kimmeridgian or Portlandian. But the other day I found a specimen which I recognized at once, generically and specifically, as a typical Middle Jurassic ammonite of western Europe, so common is it, indeed, that it has given the name to the palaeontological zone in the Jura Mts. This was a surprising discovery because I had had no indication of anything so low, nor had Burckhardt found any fossils in Mexico below Upper Jurassic. It is, I believe, a new horizon for the eastern hemisphere, and it marks the beginning of the advance of the Atlantic waters of that time upon the American continent. The embayment from the Atlantic covered western Cuba in Middle Jurassic time and continued to spread out over that end of the island during the Upper Jurassic by which time it also had reached Mexico. The Atlantic waters with the European fauna did not touch the N. American continent anywhere else except in eastern Greenland. You note that palaeontologic facts always at once transmute themselves in my mind into palaeogeographic interpretations.

Pal this Cuban problem is growing bigger and better every day. Aren’t you going to join me in the paper? It will be impossible for me to publish a second faunal paper with no discussion of field relations, general stratigraphy and correlation. A faunal paper without the geological data is not acceptable to anyone and I feel that my first paper will be criticized because there is nothing about the geology or correlation. I did not in that paper want to mention any of your data because I wanted you to give it yourself. From the beginning I have hoped for a joint paper. Of course, the descriptions of fossils I have to do alone. But to me the describing of species is a bore. I get interested only when it comes to restoring the conditions as they were – the relations of lands and seas, the origin and migrations of faunas. I want the Cuban paper to contain a number of good columnar sections and, with these as a basis, several restored cross-sections such as I roughly outlined for your London report. I want a good correlation table with a detail zonal correlation for Cuba, Mexico, and Europe. I want one or two palaeogeographic maps and a good discussion of the palaeogeographic condition. There should, of course, before all this be a clear and accurate account of field relations and I believe that we (p. 2) can construct an outcrop map for the different divisions of the Jurassic. (That’s why I’m anxious for a list of localities). Pal, I want this Cuban paper to be a classic for the Jurassic of this side of the world. We can make it a finely rounded, finished piece of work, but I can’t do it alone and neither can you do it alone. We can get out something far surpassing anything that the Survey has done on the West Indies. We can go far ahead of the Survey machinery. Of course you might send material to Washington but you know what the identifications would be like. I may say to you what I wouldn’t say to anyone else, knowing that you won’t attribute it to boastfulness nor to the conceit of youth, that I can beat Stanton1 all hollow, for I have had a training in the philosophic principles of palaeontology and he has not. His descriptions of species are pathetic. I want our report to be a model of the way in which a new area should be worked up. I must confess to you that I like everything that I do scientifically to be so perfect that it sets a milestone in whatever field I am working. Professor Merriam told me that he regarded my doctorate dissertation as a model of what a thesis should be and he had so held it up to his students. I have had that type of criticism of my work often, namely, that my method of attack and method of treatment are good.

Already I can see how I would treat the Cuban problem. Please don’t think that I’m trying to gobble up everything or get a monopoly. But I’d like to tell you my plan and then we could talk it over. We gained much before from mutual discussion. But the time is short, Pal. I shall be at the Museum only four months more – it isn’t as though my stay were indefinite.

I would never have entered upon this Cuban thing but for you. I had sworn off writing scientific papers. I would not even have returned to the Museum for these few months except that I want to finish this work for you. My return was for personal reasons only no matter how it looked to outsiders. Had Dr. Matthew offered me $200 a month to come back to work up material for him or anyone else I’d have refused. I accepted because I was already working on your material and had in my own time cleaned out the best material; all that he did was to pay me for something I would have done anyway. Pal, you really are interested in the Cuban work aren’t you? You didn’t pretend you wanted me to do it just so that I could get a job. I could have plenty of scientific jobs which paid better if I cared about them; I refused three just about the time that I accepted the Museum offer.

Pal I shall really be terribly disappointed if you don’t work with me to help finish this Cuban report and make it worthwhile. I know that I can be of great use to you; whatever I have of intellectual gifts is at your disposal. I won’t use them for myself but I will gladly do all in my power to help you. You know that I do not care if I never have another title in print myself but if I can help you on any of your work I shall be only too happy.

You probably regard me as a perfect plague in the matter of writing a paper. The writing of papers is usually a stupid bore. I think it would be good for the world if nobody wrote anything for ten years. How restful not to feel that you had to read somebody else’s contribution on some subject you were expected to be posted on. There are too many papers; most of them are never read, and are written only to add to the author’s list of titles. Pal, if I had your opportunities for travelling I’d probably never write another paper. I don’t blame you for going exploring instead of cooping yourself up with pen and paper. Often, oh so often, I rebel against the financial conditions which keep me a prisoner in study and laboratory while the whole great, glorious world stretches out all around me. This vegetating in one spot like an oyster attached to a reef, this knowing nothing of the world except what I may learn from books, is well-nigh unendurable at times. And so, Pal, I do not blame you for accepting all these splendid opportunities to see new countries. I would do just the same if I had the chance and if I were married – I would not care to travel alone as you do. Thus I can understand your preferring field work to writing. I wouldn’t urge you on this Cuban matter only that it is something already begun and we ought to see it through creditably. If you had not urged me I would not have returned to take it up again, but you did seem so to want it that I felt it was really your wish to have a paper worked up. But I can’t do it alone. (The letter ends here, at the bottom of the page, with no closing remarks).

1. Stanton refers to Timothy William Stanton (1860-1953), was a paleontologist who studied Cretaceous invertebrates and worked for many years for the United States Geological Survey (USGS), beginning in 1889. He retired in 1935 as Chief Geologist for the USGS. His papers are archived at the Smithsonian Institution. https://siarchives.si.edu/collections/siris\_arc\_217481