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Letter from Dr. Luis Alvarez

February 14, 1969

Professor Josiah Thompson
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Haverford, Pennsylvania 19041

Dear Professor Thompson:

We really don't seem to be very polite to each other, do we? You insulted me in your first letter, and I insulted you a bit back. Now you really lay it on pretty thick in your last letter. I don't want to go on with our correspondence, for reasons you will understand when you read the enclosed series of letters to the editor, etc., that Bill Shockley recently sent me. As far as I can tell, Bill is spending all his time in controversy in an area that is not central to his own interests and training, and I think he is making a serious mistake. He has a fine brain, and I would like to see it used in areas where it has been shown to have real competence. You will remember that the reason I asked CBS to bring EG and G into the picture was so that I would be spared the time required to engage in correspondence of this nature.

In spite of the very objectionable tone of your whole letter, particularly your last paragraph, I will answer it in as clear a manner as I can, and this will be absolutely my last communication on the subject. Let me quote from your last paragraph: "You may have felt you had an explanation for the fourth period of oscillation, but what notion of scientific responsibility is it that justifies the misrepresentation of the data itself? May I suggest that it is the same notion that substitutes patronizing invective for argument on the issues, the same notion that refuses to admit error when it is pointed out, and that may decide this letter will receive no answer."

I have discussed your letter in some detail with Paul Hoch, and we both feel that for reasons that we think we understand, you believe that the scientific process is equivalent to the logical process. In other words, I believe that you feel that if there is a bit of evidence that goes counter to a scientific conclusion, then that conclusion must automatically be rejected. If this were so, one would not need scientists, he would simply need an IBM machine which solves logical problems in a jiffy. As a matter of fact, the scientific method is not at all this way. The main thing that a scientist does, in my judgment is to decide which of the evidence he sees must be wrong, and must be ignored. Let me give you a concrete example of this in the form of one of the greatest achievements in physics in the last decade or so. I will not explain any of the

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words that I use, but I will instead simply give the history of the long search for the correct interaction for the universal weak decay. If you are interested in any of the details or what the words mean, you can ask anyone in the physics department at Haverford College, and he will corroborate what I am saying. My purpose in giving this example is simply to show that a good scientist is one who carefully considers the data, and decides that certain experiments are wrong, even though the excellent scientists involved have spent years trying to eliminate all errors from their experiments. As Enrico Fermi showed in about 1932, there were 5 theoretically possible forms of interaction that could lead to the beta decay of atomic nuclei. Later on it was shown that the muon underwent a similar decay, and probably involved the same kind of interaction. Many experiments over several decades were done in order to decide which of the 5 basic interactions was responsible for beta decay, and of course it was possible that a combination of two of them could equally well be responsible. It was certainly impossible that three or more could explain this basic decay mode of nuclei. But as the data became more and more refined, it became clearer that there was no possible unique solution to the problem. The only possibility that was consistent with all the experiments was that sometimes beta decay involved the A and V interactions, and sometimes the S and P interactions, but there was no universal interaction, according to that idea. Since there was good reason to believe that there was a universal interaction -- that certainly made nature simpler -- many theorists spent countless months trying to reconcile the experiments with a single interaction or pair of interactions. This proved completely impossible. Finally, Richard Feynman of the California Institute of Technology simply said, "I believe that the interaction is V-A, and this means that two highly regarded experiments are in error; Anderson's determination of the branching ratio of the pion into electrons is wrong, since it does not fit the predictions of the V-A theory, and Rubby's experiment on the recoil spectrum in the beta decay of helium is also wrong, because it says that the interaction is P or S. (I can't remember which). This was an extraordinarily bold step on Feynman's part, since Herbert Anderson, who did the crucial experiment on the beta decay of the pion was Fermi's chief collaborator over a 15-year period since the war. Anderson had been Fermi's student, and he and Fermi worked throughout the war on the chain reactor, and he is a member of the National Academy of Sciences, and a very respected physicist. For a theorist like Dick Feynman to say that the only sensible interpretation of the data required that Herb Anderson be wrong was a most serious step to take. The work of Rubby and his collaborators had, like Anderson's, occupied at least two years of intensive work on their part, and everyone who looked at the data agreed that it was better than anything else that had ever been done in the field. None the less, Feynman said that it must certainly be

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wrong, and he would assume from then on that it was wrong. Let me say, just to be absolutely clear, that I am not positive that it was Feynman who did this, but he was certainly deeply involved, and if it was not Feynman it was one of his associates. The thrust of what I am saying is quite independent of whether it was Feynman or someone else, so I will continue to talk as if it were Feynman.

Theorists all over the world took up the V-A theory with enthusiasm, and no one seemed to mind that it was in absolute disagreement with two beautiful experiments by two groups of very competent physicists. Anderson's work was repeated at the CERN Laboratory at Geneva, and the CERN people found that their results were in disagreement with the V-A theory. Anderson then repeated his work with several improvements in technique, and confirmed the CERN work, and the V-A theory. The recoil spectrum of Helium⁰ was then remeasured by two other groups of nuclear physicists, and they found a difficulty in the procedures that had been used by Rubby, which explained his incorrect result, and these two other groups both found that their experimental results were in agreement with the V-A theory.

Now this is the way physics actually is done, even though it is not at all the way you think physics is done or should be done. In my analysis of the Zapruder film, I have used exactly the same technique, only I have simply thrown out some observations that were made by people who were under extraordinary stress, watching the President of the United States' brain explode before their eyes. It seems to me that if one can throw out, as a responsible physicist, the considered and often repeated work done over a period of a year or two, by some of the best physicists in the world, and come to the right conclusion, then he is certainly within his rights and is doing the correct thing, if he throws out the recollections of some untrained observers under very shocking conditions. I did that with my eyes absolutely open; I did it because I thought it was right, and I still consider that that was the proper thing to do. You must remember that I was the first person to see that the automobile decelerated, and I required an explanation for that. I had also found not just a fourth set of wiggles, but what I noted was a "weak train of pulses". It seemed to me that the third set of oscillations was weaker than the others, and did not last so long, and needed a different explanation. I was certainly not prejudging which trains of pulses were associated with bullets and which were not, except for the obvious one in frame 313, which started the very instant that Mr. Kennedy's brain exploded. The second one came at a time when almost everyone seems to agree that a bullet was fired, give or take a few frames. The Warren Commission and Governor Connally and my analysis all agree on the firing time of that bullet, within what I consider to

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be a small limit of error on the frame number. The set of oscillations that I attribute to the first bullet is very much like the second and fourth train, and there is in the film no earlier set of wiggles, according to EG and G. (I never made measurements on the frames not shown in the Warren Report Supplement.) I simply used my best judgment as a physicist, and that judgment has proved to be quite good on some occasions in the past. I believe that my reasons were good; they tied together a number of loose ends -- the deceleration of the automobile, which was an extraordinary thing in my opinion, the weak set of oscillations of Mr. Zapruder's camera, and the siren that went off some place in that period of time. That is the best I can do, and I do consider myself to be quite competent at this type of analysis, and I absolutely reject your insinuations that I was doing anything unscientific or underhanded or any of the unpleasant phrases you have used. It is beyond my comprehension that the man driving the President and who knew that one and probably two bullets had been fired at the President and further that at least one of these had hit the President, would continue to drive at a constant speed of 12 miles per hour -- the constancy is the extraordinary thing. Then when I found (and I was the first to find this) that the driver suddenly slowed down from 12 miles per hour to 8 miles per hour, I needed an explanation for this even more incomprehensible act. The mention of the siren in the report reminded me that perhaps no one -- not even a secret service agent who was insensitive to guns shooting at his president -- could resist the impulse to lift his foot off the accelerator when a siren went off 25 feet from his ear. This hypothesis explained the simultaneous triggering of Mr. Zapruder's oscillatory system into a "low grade" set of pulses -- the only other unexplained phenomenon I noted in the film. When a scientist finds a single rational initiator of two otherwise puzzling phenomena, he treats it as a serious explanation of those phenomena. I am still satisfied with my conclusion, since all I have to do to bring it into agreement with all other observations is to move the siren time by a few seconds. I find this more rational than any other explanation I have heard, and I am not completely unlearned in this area. I hope you will reread what I say in my example of the way science is really done, in contrast to the way it is taught in books on logic.

I am aware that philosophers are often logicians, as for example Lord Russell. I am aware that he is a distinguished philosopher, and he certainly wrote the most exhaustive book on mathematical logic. I don't know whether you are a logician in addition to being a philosopher, but I have the impression that you think that science is somehow or other a branch of logic, and that if there is one piece of evidence that argues against a certain conclusion, then that conclusion must definitely be wrong. This, as I have gone to considerable length to explain to you in recalling the universal Fermi interaction story, is just not the way science is done. Dick Feynman is an exceedingly fine scientist; he did win the Nobel Prize for his work in quantum electrodynamics, two years ago. He doesn't do science by any set of rules of logic.

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I do not expect you to be convinced by what I have said, but at least I am on the record and I'm sure that any scientist who reads what I'm saying will understand why I did what I did, and will applaud it as in the very best traditions of science. As a matter of fact, a great many scientists have read what I have done, and without exception they have said (approximately), "Gee whiz, I wish I had done that. That was a really beautiful bit of work that you did." I will continue to listen to my scientific friends, when I want to know whether or not I did a good analysis of the Zapruder film.

I respect my young associate Paul Hoch, and I do not see anything wrong in our disagreement about details of this argument. There is nothing democratic about Science, with "One man, one vote." We judge a man's work by his demonstrated competence, and not by his "promise." Enrico Fermi knew more than ten times as much physics as I do, and I know more than ten times as much physics as Paul does. That doesn't mean that I'm a better scientific detective than he is, but I've done several important pieces of scientific detective work in the past, so I know I have some real "demonstrated competence" in that area, and I will continue to believe that my analysis of the Dallas film is better than Paul's, until he shows he is capable of proving his points in some difficult analytical situations of a practical nature. I believe he has real "promise," but no actual performance in this area to date.

I think that probably the best thing is for you to stick to your philosophy and I'll stick to my physics. I had an interesting interlude using my training as a physicist to examine evidence from the Zapruder films. I found things that the FBI photo interpreter said it was impossible to ascertain, for example, where was the car when there was nothing in the background? (In fact, I solved this in two completely independent ways.) The FBI photo interpreters also said it was quite impossible to tell (when the film was blurred) what was moving, whether it was the camera or the automobile, and again I showed this to be quite fallacious. Everybody with whom I discussed the problem of the frame rate said it couldn't be solved. All the photo experts and all of the Warren people said it was impossible to tell at what rate Mr. Zapruder's camera was running. I showed quite conclusively that it was not running at 24 frames per second, as Mr. Zapruder said, when he was really trying to say simply that it was going at its normal speed. (35 millimeter film goes at 24 frames per second, but 8 millimeter film goes at 16 or 18 frames per second at "normal rate," and 24 frames per second for "slow motion.")

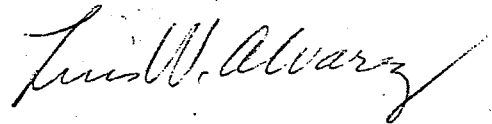
Here is another case where one should ignore the actual words in the testimony, and listen to the meaning. Mr. Zapruder simply was trying to say "I wasn't pushing the button on my Bell and Howell down to

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the 'high speed' position -- it was running at the normal rate." He incorrectly remembered 24 frames per second as normal for all movie film, and replied "24 frames per second." That made a lot of people believe that he was running his camera on the "high speed" setting of 24 frames per second, rather than the normal of 16 or 18. This reduced the time available for shots by the same ratio, and increased the speed of the car by the inverse ratio. No one else but me could put any limits on the speed of the camera. I showed it was not in the "high speed" mode.

It is certainly quite possible that I made errors, and if someone can demonstrate to me that I made such errors I'll be very happy to change my mind, and tell anyone you wish that I have made such errors. But I have not seen anything in any of your writings that would convince me that anything that I have done was incorrect. There are cases where we differ, and neither one of us can prove he is right, and I happen to favor my own interpretation. In spite of what you say, that is the way science is done, and since I am a professional scientist, that is precisely what I intend to keep on doing.

Very sincerely,



Luis W. Alvarez

LWA:am
encls.

cc: Paul Hoch
Walter Menaker