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August 18, 1982

Prof. Richard L. Garwin
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Dear Prof. Garwin:

Thank you for your letter of May 28, about the work of the NRC committee on the acoustical evidence in the Kennedy assassination.

I understand that Prof. Alvarez has distributed relevant pages from my newsletter, "Echoes of Conspiracy," which he asked me to send him. That material might have given you a general idea of my reaction to your report. Also, I have corresponded with Prof. Chernoff on some statistical points; I can send you those letters, if you wish.

My major questions include the following:

(1) When all is said and done, what do you think the HSC experts were looking at?

(2) On what grounds was the Committee initially negative about the BBN/W&A analysis?

(3) What do you really think of the FBI's rebuttal to the HSC experts?

(4) How thoroughly did you look for evidence not in the BBN/W&A reports which would support their conclusions?

(5) How exhaustively did you deal with the hypothesis that the recording had been copied and changed?

Let me first focus on question (1) as applied to the supposed knoll shot. Prof. Ramsey touched on this point when he talked with George Lardner of the Washington Post. "'The chances of its being static or other noise are higher than previously estimated,' Ramsey said. (This reflects your Appendix A-3, which increased to 0.223 the P value for a match to random noise.) Lardner then pressed for a more definitive answer: "Higher than gunshots? 'Yes, okay, higher than gunshots,' he [Ramsey] said."

Of course, given the timing discrepancy, if you are certain that the Dictabelt is an unaltered original, then the impulses in question must have been something other than a gunshot signal - that is, some kind of noise. But I don't think that is what Prof. Ramsey meant to say. Perhaps you can clarify for me what quantitative basis there is for a statement about the probability of any specific hypothesis other than random noise. Did the panel study, or consider studying, the actual distribution of noise on the tape? Would that have to be done before one can assign a numerical probability to the hypothesis that the signal was caused by static or other noise?

Your own calculation, which increased the P value from 0.05 to 0.223, was described in your report as "possibly overconservative." In fact, the word "conservative" appears three times in Appendix A-3. I have discussed this with Prof. Chernoff. As I understand it, there is some question whether one full degree of freedom should have been subtracted because of the speed of the car. As far as I know, this parameter was omitted from W&A's original calculations, and then added, but the speed may not have been allowed to vary freely. (A fixed value may have been taken from other information.) Perhaps you could explain why you characterized the calculation as possibly "unduly conservative," and what weight I should give to the absence of a clearly non-conservative calculation.

The adjusted P value (0.223) seems to reflect not a match to random noise, but a match to two bursts of random impulses, spaced to match the two separated parts of the signal and the test shot. As I understand what Prof. Chernoff told me, BBN's decision to omit the empty middle interval from their calculation, which seemed to me unnecessarily conservative, was in fact a reasonable way of looking at a slightly more elaborate hypothesis than random noise, namely two spaced bursts. The P value for noise randomly distributed over the entire interval would be much less than 0.223 - in fact, 0.06 or less.

In other words, the P value calculated your way is at most 0.223, for two bursts of random noise with the proper spacing. So, I wonder about the basis for any claim that noise, even if nonrandom, can be shown to be more likely than a gunshot, on the basis of probability calculations and/or flaws in the calculations of the HSC experts.

Turning to the other supposed shots: your report dealt with the original BBN argument that a moving microphone had been detected, which was based on a chi-square analysis of the matches over a certain correlation threshold. I understand your objections to that analysis, which reduced the probability corresponding to a real detection from over 99% to 93%. I never did find this chi-square analysis particularly convincing, in part because of the unsettling dependence on choice of a correlation threshold for a match.

I was, however, quite impressed by the presentation of the correlations for the three non-knoll shots which Dr. Barger submitted in his letter of February 2, 1982, to Prof. Ramsey. To refresh your memory, I am enclosing the graph from this letter. I was surprised to find no mention at all of this presentation in your report. I would very much appreciate hearing your explanation of how an artifact could have caused the spurious detection of these three shots, and of the reasons for omitting this information from your report.

Leaving aside the probability arguments, how should I explain to the readers of my newsletter what, in the committee's opinion, caused the BBN/W&A results?

Your report noted that, after the Committee's first meeting, "no member was convinced" by the HSC analysis that there was a knoll shot. I have some knowledge of the basis for Prof. Alvarez' rejection of that analysis. He told me in October 1980 that one of the HSC reports was some of the worst work he had seen in many years. I understand that he wrote a memo saying that it would be a waste of money for the committee to meet before the HSC experts cleared up the points he had raised.

I have not yet seen the Alvarez memo, but I suppose that one objection was something we discussed. Prof. Alvarez told me that, in the display of BBN's test shots, the points marked as corresponding had to fall on a straight line, but did not. As I now understand it, there is indeed an error in (for example) BBN's Figure 15 (8 HSC 86): the points are too far apart to correspond to a single echo received at microphones spaced 18 feet apart. Did this error in a graph of test-shot data bother you as much as it bothered Prof. Alvarez?

Prof. Alvarez took a position on the acoustical evidence as early as June 1979, when he wrote that he didn't "think the new 'evidence' should be dignified by the use of that phrase. The police officer whose radio was recorded insisted for years that he wasn't even close to Dealey Plaza at the time the President was shot. I am simply amazed that anyone would take such 'evidence' seriously." Prof. Chernoff has also made his feelings clear, in a recent reference to "the abuse of statistical reasoning (blasphemy)" by the HSC experts. I have attended enough meetings with Prof. Alvarez to know what a forceful adversary he can be. I would like to know to what extent the points raised by Alvarez and Chernoff formed the basis for the initial non-acceptance of the HSC results by the other members of the committee.

Turning to the FBI report: Prof. Chernoff told me (speaking for himself, not for the committee) that it was "simply dismissed as irrelevant." That would be a reasonable position - although, as an outsider concerned with many aspects of the Kennedy assassination controversy besides the acoustics, I hesitate to consider completely irrelevant the fact that the FBI chose to release its report to the public while your committee was working, and while the Justice Department was publicly taking the remarkable position that "the only indication of a conspiracy is the HSCA expert opinion regarding the acoustical evidence."

My reading of the single paragraph in your report on the FBI analysis is that the author went out of his way to emphasize his agreement with the FBI's conclusion, while minimizing his criticism by restricting it to one particular FBI argument. This formulation appears twice in that one paragraph. It is hardly the same in tone as saying that the FBI report was irrelevant, even though it gave the right answer.

I have two substantive questions about the FBI report. First, was there any FBI argument which was valid enough to justify its conclusion? My own impression is that the FBI report was totally unpersuasive. The probability associated with the Greensboro match was apparently not even calculated, but given (as 95% or more) on the basis of an erroneous (but unessential) statement in the W&A report.

Secondly, you said that "39 Greensboro shots were available from which the most favorable could be selected." Did you determine whether the FBI did in fact select the most favorable match? The documents I have seen do not reveal if this was done (either by calculating all 39 correlations or by qualitatively selecting the most likely candidate).

If I am wrong in concluding that the NRC committee deliberately minimized the weaknesses of the FBI analysis, please correct me.

My fourth question concerns the committee's efforts to validate the HSC results. I understand that the committee's mandate was to review the reports of the HSC experts, and the data and methodology used. You did a lot of work on voiceprint analysis and other studies to confirm Steve Barber's observation of crosstalk. Although I personally was convinced that the crosstalk matched simply on the basis of what I could hear, I recognize that the work you did was ingenious and certainly appropriate. However, I was struck by the absence of reported committee work on the raw data of the purported shots themselves.

For example, your calculation of P=0.223 for a match to noise was based on the number of coincidences reported by BBN. As I pointed out to Prof. Chernoff, the W&A report claimed one additional coincidence. Using that data, and reducing the coincidences to account for the degrees of freedom as you did, one gets a P value for a spurious match of only 4%, consistent with the 5% HSC figure. From what Prof. Chernoff told me, there was no special reason for not using the W&A figures; he chose the BBN figures "mainly because they were the easiest for me to read and understand, and their calculations were based on these." I am not trying to excuse the inconsistency between the two reports. However, I would have expected the committee to resolve the discrepancy either by talking to the HSC experts or by looking directly at the raw data. If this was not done, I do not know why.

I have not raised the above points simply for the sake of arguing with the members of the committee. I believe the crosstalk is real, but I remain unconvinced that the statistical and other flaws in the HSC analysis are bad enough to make the issue moot. In the absence of a specific mechanism for producing not one but four spurious shots, and in light of the possibility that your P value of 0.223 for a chance match to the knoll shot is indeed unduly conservative and should be closer to 0.05, I still wonder whether the Dictabelt contains real shots at the wrong time. In other words, because I was not unconvinced by the BBN/W&A analysis at first, I think that the possibility of reconciling the shots and the crosstalk, by means of a hypothesis in which the Dictabelt is not a continuous original, has to be dealt with exhaustively.

In letters to Prof. Ramsey, I made several specific suggestions along these lines. A visual check of the belt was indeed an essential step, but I would not rely on Doris Schwartz's statement that the belt is the original. Did the committee question her directly, under oath, or was her statement relayed to you by Capt. Bowles?

The Secret Service made a copy of the Dictabelts within a week of the assassination. Some years later, that tape could not be located for me. It could be important evidence of what was on the original recordings. Were you able to locate and listen to that tape?

Also, I argued that certain messages were of dubious authenticity. These messages, within half an hour of the shooting, relate to Dallas Police Officer J. D. Tippit (who was killed later that afternoon). Although these messages appear on later copies of the recordings, and on transcripts, the information they contain about Tippit's movements apparently was not known for some time to certain Dallas authorities, at a time when there was some doubt about why Tippit was where he was. In addition, some of the messages from the police dispatcher struck me as unusually formal in phrasing - unlikely for the totally routine exchanges they purportedly were, but not surprising if they had been read and added to the recording later. Did anyone even listen to the relevant Dictabelts to see if these messages are there at all?

I would, of course, be glad to send you another copy of the material on the Secret Service tape and the Tippit messages which I submitted through Prof. Ramsey.

Studies like that could not have proved that the HSC experts were wrong, but they might have established that some of the Dictabelts you had were not the unaltered originals. Are you personally satisfied that the possibility of re-recording has been dealt with thoroughly enough?

Dr. Rader told me that he did not want to make this investigation into an entire career. Neither do I, and I expect you feel the same way - especially when faced with a letter this long. I am planning to get much of what is available in the NAS's public access file, but that file reportedly does not include much technical material from the committee's working papers. Any information you or the other members can provide to me will be greatly appreciated.

Sincerely yours,

Paul L. Hoch

Paul L. Hoch

cc: Dr. Rader
Dr. Elkind

FIG. 1
FROM BARGER TO RAMSEY, 2 FEB 82

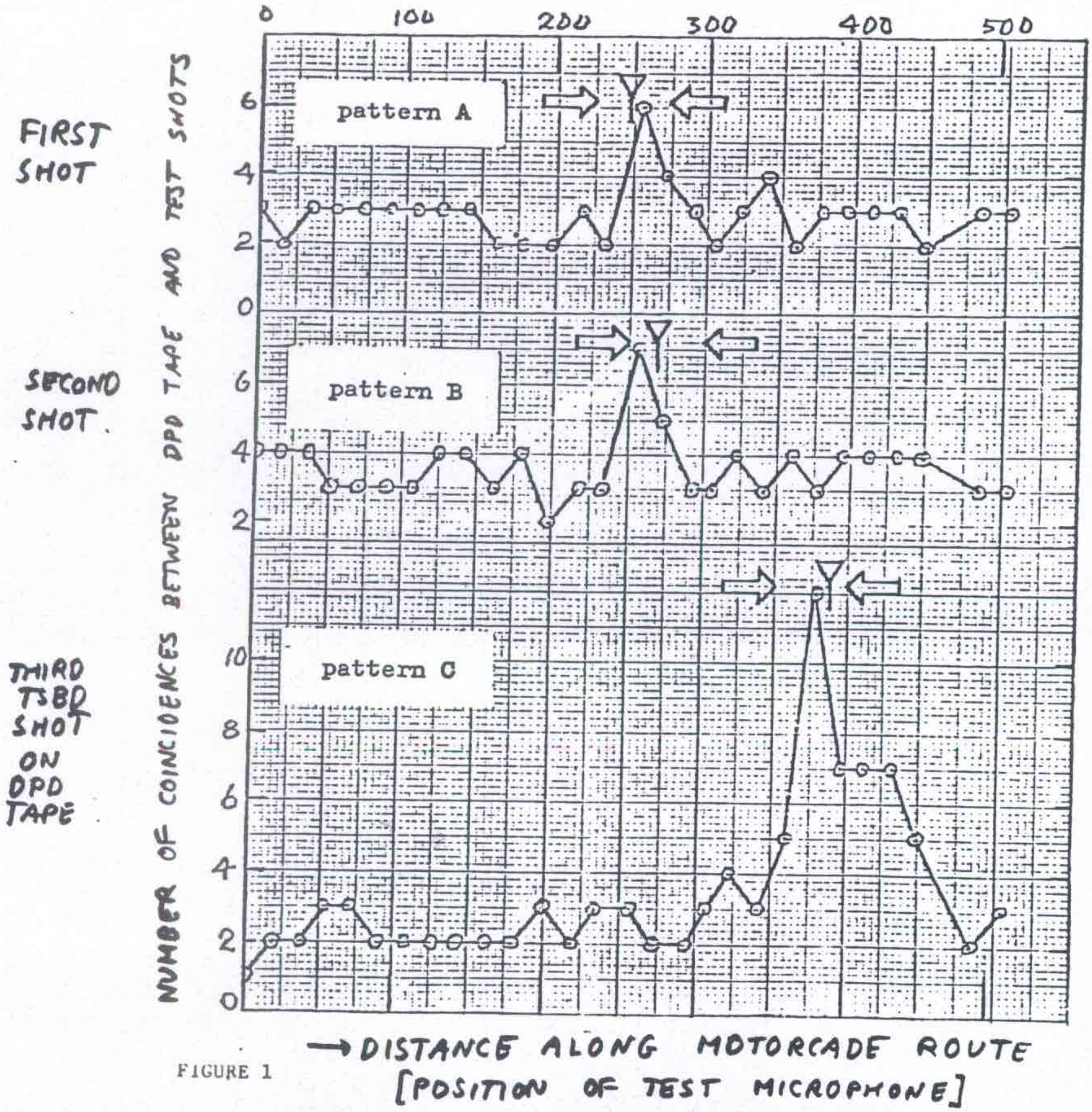

FIGURE 1

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September 16, 1982

Dr. Paul L. Hoch
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Dear Dr. Hoch:

This responds to your long letter of 08/18/82.

I don't know what the House "experts" were looking at. I do know that they weren't looking at a portion recorded at the time of the shots. I know also that their analysis was a good idea carried out with less than adequate care and skepticism.

There is really no reason to compare the probability of the hypothesis with the probability that the recording was due to random noise. That is the easiest comparison to make, but there is no reason a priori (and certainly none from observation) to characterize the noise on the recordings as "random." In order to compare with any other hypothesis, one would have to assume some "burstiness" about the noise, or to derive that from observations of the recording. The recording is anything but "stationary" so that would not be an easy job.

It is by avoiding such experimental designs that I have accomplished what I have in physics; I'm not about to start analyzing somebody else's poor experiment (which we have disproved on other grounds) to find out how to do the tedious and complex analysis demanded by a poor experiment. For your information, I enclose the paper I gave to the Fifth Cambridge Conference on Relativity at MIT, dealing with Joe Weber's results.

As for your question "On what grounds was the Committee initially negative about the BBN/W&A analysis?" I don't characterize the Committee's view in that way. In fact, I don't think the Committee had a view until it wrote its final report. I personally thought the idea of matching echo patterns was quite clever, in view of my experience in acoustic location of artillery, and it was not until I had read the reports and then tried to understand from those reports and from the conversations with Barger and W&A that I recognized their case was really very weak. In fact, my earliest substantial concern arose from the fact that BBN and W&A initially found different regions of the recording as representing the third "shot." So I do not accept your premise.

As to "what I really think of the FBI's rebuttal to the House experts," I think they also went astray. They tried to match the patterns with another pattern of assuredly non-random noise, but they made a faulty analysis of the statistics!

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*Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

I can't reply to every statement or question in your letter, but I did note your sentence "If I am wrong in concluding that the NRC Committee deliberately minimized the weaknesses of the FBI analysis, please correct me." You are wrong. The FBI report was irrelevant to our work; had we been asked to analyze the FBI report, we would have put a lot more effort on it and given it more space in the report.

As for how thoroughly we looked for evidence "not in the BBN/W&A reports which would support that conclusions," I personally certainly kept my eyes open for everything I could see. If I had thought or anyone in the community had informed me of any acoustic evidence which might support the BBN/W&A conclusions, I would have looked at it.

As for the "hypothesis that the recording has been copied and changed," I thought about it and looked at it, but I can't go beyond the conclusions and analysis stated in the report. I do think that we did pretty well in obtaining a copy of Channel II without "repeats," which made possible the improved timing analyses.

Within a few weeks we will put into the Academy file a more technical report of the work done by me and my associates at IBM Yorktown Heights. That report will of course be available to the public, including you.

You understand that the Committee on Ballistic Acoustics was not asked to review the question of conspiracy, whether Oswald really shot Kennedy, whether Oswald was a paid agent, or any of those questions. Of course, had we found answers we would have reported them, but we did not. We did establish in several ways that the portion of the recording in question was made long after the actual shots, so that even if the analysis of "95% probability" were valid, that hypothesis would have to be rejected and one would have again to admit that a 5% probability comes up once in 20 times! Actually, we did enough work on the probabilities to show that the "95%" figure was unwarranted, although not enough (I don't know that <u>anybody</u> could do a proper analysis) to know what the best figure is.

I hope this reply is of some use to you.

Sincerely yours,

Richard L. Garwin rsa

Richard L. Garwin

Encl:

06/10/74 "The Evidence for Detection of Kilohertz Gravitational Radiation," presented at the "Fifth Cambridge" Conference on Relativity (MIT). (061074EDKG)

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R 9/23/82