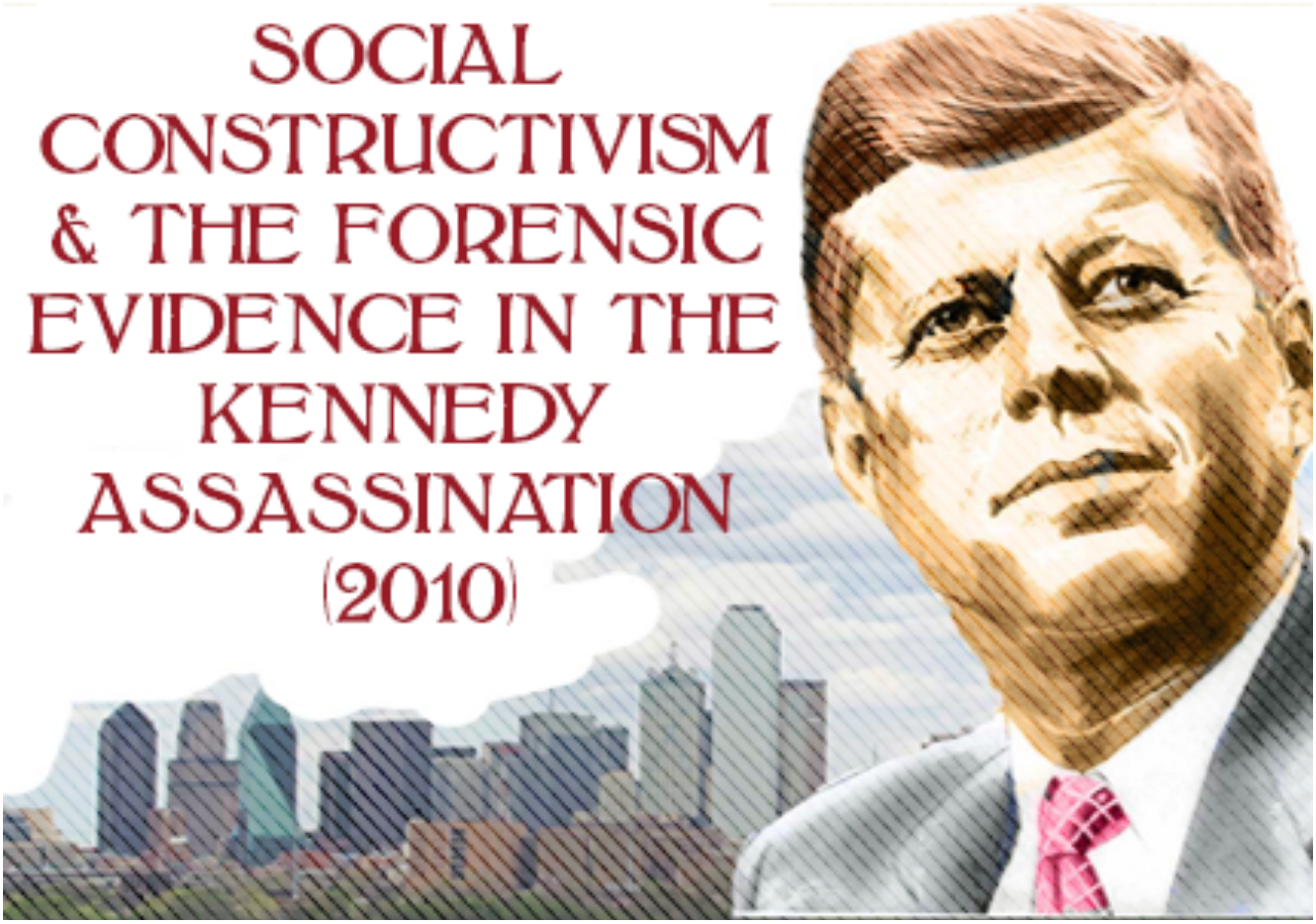
THE MANTIK VIEW

HEAR NO EVIL: SOCIAL CONSTRUCTIVISM & THE FORENSIC EVIDENCE IN THE KENNEDY ASSASSINATION (2010)



HEAR NO EVIL: SOCIAL CONSTRUCTIVISM & THE FORENSIC EVIDENCE IN THE KENNEDY ASSASSINATION (2010)

By Donald Byron Thomas

A Review by David W. Mantik

The Mantik View

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UPDATE *NOTE: This version adds clarity, but also corrects multiple mistranslations that arose during conversion of my original Word document for the CTKA website (now known as Kennedys and King). I also correct a few of my own mistakes (e.g., Fig. 3). The most important point though is this: The pièce de résistance from physicists Linsker and Garwin (their reductio ad absurdum—see Figure 21 ff.) remains not only unchallenged, but quite unaddressed by Dictabelt advocates. My fellow JFK conspiracy believers would like to discipline me for denying their case, but the facts do not support them, and their failure to address this reductio issue does them no credit. Until they address this paradox, the acoustical case for conspiracy lies lifeless in the morgue. That these promoters are unlikely (ever) to address this conundrum is provably based on the absence of any attempt to do so during the intervening 8 years. I here also briefly update my conclusions about the head shots, an issue explored in greater detail in my e-book, JFK's Head Wounds (2015). My (new) monologue on the current controversy about p-values appears just before Appendix 1. Appendices 8–11 are all new; the cognoscenti may wish to begin there. Appendix 11 summarizes the Sonalysts 2013 report, which holds a quiver of arrows fatally targeted at the acoustic case.*

People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices.

Adam Smith, The Wealth of Nations, 1776

New York: P.F. Collier & Son, 1911: 207

It is essentially impossible ever to establish the absence of a conspiracy… whereas in some cases it is possible to establish the existence of a conspiracy. -R. Linkser, RL Garwin, H Chernoff, P Horowitz, NF Ramsey

Note 1: See Appendix 1 for commonly used abbreviations.

Note 2: See two of my prior lectures for visual aids—

- For the Pittsburg lecture (2003), Google: Twenty Conclusions after Nine Visits;
- For the Dallas lecture (2009), see The JFK Skull X-rays: Evidence for Forgery at:

http://www.assassinationscience.com/

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An Introduction to this Review

When Thomas's peer reviewed paper (*Science and Justice* 2001; 41: 21-32) first appeared, the media contacted me, and I was happy to praise Thomas. It is a pleasure now to be more specific about this fascinating acoustics work. I have heard Thomas speak on several occasions. On December 28, 2005 (my wife had major surgery that day, so I went from the ICU to the lecture) we hosted him for a large audience of prostate cancer patients receiving proton radiation. Thomas was brilliant as usual, as he adapted his talk and visual aids precisely to the level of the audience. The question and answer session quickly elicited the enthusiasm of our listeners. I would personally echo the comments of Rex Bradford (Thomas's publisher) in Bradford's opening comments in the book: Thomas obviously displays keen intelligence, curiosity and honesty. One can scarcely ask for more from a scientist, but (in my opinion) one gets more—and that is passion. He has passion for the subject, passion for truth (the oxymoronic title of Specter's book) and, finally, passion for what this seminal event means in American history and for what it reveals about our national character. To appreciate his passion, see his comment (in Chapter 14) about Arlen Specter. I also sense that on occasion he can scarcely refrain from strangling a few of his constructivist adversaries. For all his work, and for his passion, I salute him. I am pleased to be a fellow traveler in this JFK conspiracy research.

A Foreword by Jim Lesar (May 23, 2010): My Comments

I have met Lesar (a fellow Wisconsin alumnus and fellow basketball addict) on many occasions, including a tête-à-tête dinner. Jim has a marvelous legal mind and is a bulwark for the critical community. His is no generic foreword. On the contrary, Lesar precisely details Oswald's contacts with the DRE, a CIA-funded, anti-Castro Cuban organization. Of central importance is the role of George Joannides as the CIA's case officer for the DRE. This same character also served as liaison to the HSCA (House Select Committee on Assassinations) in its probe of Oswald's links to the DRE. Joannides was brought out of retirement by the CIA specifically for this purpose. G. Robert Blakey (Chief Counsel for the HSCA) later confessed that he had not known that Joannides had played the role of case officer for the DRE. Furthermore, Joannides did not see fit to tell Blakey of his own dubious history. Precisely at the moment that Joannides emerged from retirement the CIA stopped cooperating with the HSCA. It was not until 1998, thanks to the ARRB (Assassination Records Review Board), that Joannides was finally identified as the case officer for the DRE. Lesar quotes Blakey's top aide, Dan Hardway: "*I am now certain that Joannides was hiding evidence of conspiracy to kill Kennedy."* Even to the present day, the CIA still stonewalls Jefferson Morley in his efforts to expose further records on Joannides.

Lesar next introduces the acoustics (misspelled as "accoustics") story, including Mary Ferrell, who brought the police Dictabelt to the attention of the HSCA. Based on this tape the HSCA concluded that there had been a "probable conspiracy" to kill JFK; in fact, this was the only evidence that they recognized for conspiracy. Lesar then briefly summarizes the history of the acoustic debate, beginning with the original analysis for

the HSCA by Barger, Baranek (misspelled as “Beranek”), and Newman (this team identified five possible shots), continuing with the ethically dubious role played by Luis Alvarez with the NRC (National Research Council), and finally the egregious 2003 ABC-TV “documentary” by Peter Jennings. More to the point, Lesar claims that Thomas has demolished the counter arguments to his peer reviewed paper of 2001. These latter issues are all addressed in detail below.

My Overview of the Book

CIA cognoscenti will recognize that this book is not a sequel to its near namesake, *See No Evil* (2002) by Robert Baer. The latter is an autobiographical account of a ground soldier’s war (as a CIA officer) against terrorism. It has nothing to do with the JFK assassination. This book is recommended to those who want to know exactly how the CIA has gone off track in the past several decades. *[UPDATE: More recently Baer has promulgated disinformation about Oswald in Mexico, so Baer now also has gone off the rails.]*

Thomas, on the other hand, does recognize the CIA—he cites them ten times. In fact, he fearlessly confronts countless central issues in the JFK assassination. For such a Herculean challenge he needs a huge endowment of chutzpah—which he clearly possesses. The risk, of course, is that he will frolic too close to the chasm, where his knowledge base is thin, and then crash. For the medical evidence especially, I shall return to this risky behavior. Although I tend to be a stickler for clarity and brevity in print, Thomas usually gets good marks for writing style, although occasionally I wished he had had an editor (none is identified). Intermittent prolix phrases need more pruning and, as a result, clarity sometimes suffers. I could not locate an Acknowledgments section, which greatly astonished me, given his sprawling survey of a vastly littered landscape, and especially since he was not likely a solitary traveler on his journey. On a personal level, the reader might want to know what exactly drew him to this case and when lightning first struck him, but no comment is forthcoming. Similarly, when (and why) did he study statistics?

Our national dialogue has long been plagued with stock labels: assassination buffs, conspiracy theorists, regressive right, progressive left, Tea Partiers, radical socialists, bleeding hearts, compassionate conservatives. (Also see *1984* by George Orwell, or “Language: A Key Mechanism of Control” (GOPAC Memo, 1996) by Newt Gingrich, or George Lakoff’s online discussions of “framing.”) The purpose for such labeling (or framing) is simple: to curtail any genuine discussion. The goal of those who label, or frame, is single-minded—simply winning the debate. This is done merely by spraying a certain color or odor onto a specific argument or even onto a real human being. Such a practice severely limits honest communication and often leads to an impassible gulf between the political left and the political right (or other similarly paired dichotomies). Unfortunately, and surprisingly, Thomas repeatedly uses some of these biased phrases to describe those who oppose the Warren Commission (WC). Oddly enough, he does not tell us if he himself is a “buff.” Even worse, he does not define such loaded phrases, nor does he explain his motive for perpetuating this media-inspired nonsense. Furthermore, almost nowhere does he label WC supporters with parallel

disparagement, e.g., lone gunman theorists, or even assassination buffs. (Well, there is one odd exception (p. 361), where he implies that John Lattimer—not named in the index—and Luis Alvarez were both buffs, presumably meaning lone gunman buffs.) On rare occasions, Thomas uses the term "critic" for WC opponents. This term clearly carries much less disapprobation, and, in my opinion, should be preferred in neutral discourse. Additional dispassionate options, too rarely employed by Thomas, might be "researcher" or "student."

The second part of the title (the social constructivism part) will likely prove jarring for many readers. Given its prominent role in the title, it is puzzling that Thomas does such a meager job of defining his terms. In its first appearance (p. 8) it is a "scientific philosophy." This rendered me almost speechless, as I have sometimes (whimsically) viewed it as an "anti-scientific philosophy." Furthermore, Thomas offers only one reference (Cole 1992). He notes that in this circle of believers, science is viewed as a social process; in fact, and more to the point, scientific conclusions are thought to be merely social constructs. I quote Thomas: "*The consequences of the results [of science], as much if not more than the empirical evidence itself, will often steer the scientist to one conclusion over another*." In the very next paragraph, Thomas offers a concrete example (but he fails to explain how that example follows from the primary definition):

…the scientists and technical experts who worked on the Kennedy assassination evidence appear to have allowed political considerations to influence their judgment…

Unfortunately, we get very little more from Thomas about social constructivism. Even though he often mentions it (pp. 211, 292, 309, 314, 323, 448, 598, 618, 660, and 661), his index has only three citations (pp. 8, 91, 737). His final reference (p. 737) clarifies things a bit but it comes far too late: "*…these men [the scientific community] had 'sifted and sorted' the evidence to bring it into accord with the conclusion which they believed*." Perhaps Wikipedia can help:

Social constructivism is a sociological theory of knowledge…, wherein groups construct knowledge for one another, collaboratively creating a small culture of shared artifacts with shared meanings. When one is immersed within a culture of this sort, one is learning all the time about how to be a part of that culture on many levels. Its origins are largely attributed to Lev Vygotsky. [DM: See Mind and Society 1930, 1978].

However, even given a more robust definition, this idea needs to be elucidated to show how it applies to the JFK assassination. Some examples from the constructivist literature would help, but none are offered. Alternately, some historical examples would clothe this concept with some real flesh and blood: How about the Nazis' use (or, more accurately, abuse) of their own scientists and engineers? Given the peripheral role (in any explicit sense) of social constructivism in the book, I suspect that most readers would just prefer to skip this philosophy. Furthermore, it is not likely that Thomas's targets were necessarily part of an insider cabal who had agreed in advance that science was merely a social construct—that would be far-fetched indeed. Instead, I would simply have emphasized that all humans are inevitably and deeply subject to social pressures. I have previously cited the astonishing work of Stanley Milgram,

published just before the JFK assassination, but not cited by Thomas. My summary of Milgram's work can be found in my Foreword to '*In the Eye of History'* (2005) by William Law, or in my online 2003 Pittsburgh lecture.

[UPDATE from Albert Rossi (February 13, 2019). Perhaps Thomas finally grasped the unnecessary puzzles evoked by his original subtitle, as his more recent Skyhorse edition (September 3, 2013) bears the subtitle "Politics, Science and the Forensic Evidence." Social constructivism was thus promptly exiled to the dustbin. C'est la vie.]

Next, we come to a deeply divisive issue: the authenticity of the Zapruder film. Thomas immediately makes his bias clear: The Z-film is "hard evidence" (p. 3). However, he offers this in an almost off-handed manner—in the context of describing the film as the sole item that triggered the HSCA investigation. Most believers in Z-film alteration would likely be willing, for the sake of discussion at least, to accept his premise, i.e., they would permit him to build his case on this belief, so long as that assumption is clearly stated from the outset. However, given the countless Z-film issues currently bandied about, he has chosen instead deliberately to ignore the entire controversy, as if he still lived in 1975. Why not merely recognize reality (there is a debate) and acknowledge these dissidents? (There are many serious researchers, often with professional pedigrees.) He could, after all, opt merely to state that he chooses to accept authenticity and proceed forthwith, without wishing to be drawn into this debate. But to don (a pale pun) blinders to this issue, as he does—even if he believes his opponents to be fools—seems disingenuous, and not what one would expect from a scientist. Given his deliberate evasion of this issue, I shall be brief.

He could at least have nodded in the direction of physicist John Costella's luminous (he is an expert in optics) work on the film (*The Great Zapruder Film Hoax* 2003, edited by James Fetzer), or in the direction of Douglas Horne's exhaustive account (*Inside the ARRB* 2009). That would have been intellectually respectable, and it would have put newcomers on fair notice that not everyone shares his most fundamental assumption, i.e., Z-film authenticity. Unfortunately, Thomas has instead chosen to totally ignore <u>both</u> authors. (Surely there was ample time to review Horne's 2009 book—e.g., Lesar's Foreword is dated as late May 2010.) Thomas does not even cite Horne in his index (even though he occasionally quotes him); even Vincent Bugliosi (*Reclaiming History* 2007), the arch WC loyalist, is cited only twice in the index. As an employee of the ARRB, Horne had interviewed (or assisted in interviewing) many of the medical protagonists in the JFK case. He is the only former government employee to write about the case. He has added much new information, including powerful eyewitness reports of two successive and independent CIA events with the Z-film during the weekend of the assassination. Thomas, however, totally ignores this new evidence about the film. On the contrary, he only regales us with the now hoary story of CIA Item 450 (page 370, footnote 8). I shall come later to several serious and substantive issues raised by Thomas, issues that depend precariously on the authenticity of the film. For the most part, however, I shall try to accept, for purposes of this discussion, Thomas's fundamental assumption that the Z-film is authentic. [UPDATE: Z-film authenticity was severely challenged at the recent CAPA conference (November 15, 2018) during the

video presentation by Sydney Wilkinson and Thom Whitehead. The cartoon-like Black Patch in the Z-film was overtly obvious over the back of JFK's head, especially at Z-317. Further comments in this video by two film professionals, and the historical perspective of Dino Brugioni, now add to the overwhelming case against Z-film authenticity.]

On a fair number of occasions, usually when I disagree with him, Thomas reaches key conclusions even when the data seem slender. When he wants very much to resolve an issue, he tends toward a conclusion instead of just leaving the case open. To state his bias would be fine, so long as he simultaneously admitted that the evidence was ultimately inconclusive. For example: Did the carbine truly belong to Oswald (LHO)? Thomas says: Yes. Was there really no Mauser on the 6th floor? No Mauser, he says. Another problem raised by this tendency to reach final conclusions prematurely is this: some of these premature conclusions recur later as critical supporting pillars for additional arguments. In other words, these earlier conclusions (e.g., the 6.5 mm object inside JFK's right orbit on the AP X-ray was merely a piece of shrapnel) become cornerstones in subsequent arguments. That means that if his first conclusion is mistaken, he will also inevitably mislead us about the second one. Sometimes this problem even cascades.

Did LHO fire a shot that day? (I doubt it.) Most readers would like to know. As I scoured the book for Thomas's answer, I became frustrated. He unequivocally implies that LHO ordered the weapon and that it was found on the 6th floor, but then he tells us that LHO wore a dark shirt, whereas the 6th floor shooter wore a light-colored shirt. So there the matter rests. Even in his reconstruction (near the end of the book), he refuses to tell us whether LHO fired a weapon that day. Of course, he is not required to give us his opinion, but think about this: If LHO did not fire, then how did his carbine get to the 6th floor? The reader will not find the answer in this book. Bugliosi, on the other hand, offers a simpler inference from LHO's ownership of the carbine: unless someone stole it from LHO (or he gave it away), then Oswald fired that day. I doubt that, too.

Thomas's Introduction: My Comments

Thomas gives us an enlightening summary of the Dealey Plaza witnesses, as first described by Josiah Thompson and David Butterworth. [DM: Also see *Cover-Up* 1998, Stewart Galanor, p.171.] Then he recounts the ear-shot experiment, arranged in 1978 by psychoacoustic (sic) experts for the HSCA. These results were straightforward: a gunshot from the TSBD (Texas School Book Depository) sounded like a shot from there, while a shot from the GK (grassy knoll) sounded like a shot from that spot. Despite this, the experts insisted that the results were consistent with the official conclusion, i.e., only shots from the TSBD. Their argument was simple: if there were only two shots, then most witnesses would have reported two. On the other hand, since only a small number reported two (most heard just one—see Galanor 1998, p. 171), the official conclusion must be correct. Thomas gleefully proceeds to dissect this bizarre argument.

The Daubert decision (1993) is a landmark in American law. No longer can just anyone claim to be an expert witness and appear credible in court. Not even bona fides are

enough today. Instead, the expert's method must be recognized as genuine science, accepted by the community at large, and the reliability must be quantifiable as a "rate of error." Thomas implies that Daubert might have saved us from the fiascoes of the WC (they assumed that LHO was the assassin) as well as those of the HSCA (they assumed that the WC truly tried to be objective).

Chapter 1: The Crime Scene (p. 15)

We come now to one of Thomas's main courses. He does a brilliant job of highlighting the facts, only some of which I list here. Unless I protest specifically the reader may assume that I tend to agree with him.

NOTE: Unless specifically stated otherwise, all the numbered items in the chapters below represent the views of Thomas.

1. Eleven witnesses said they saw a gunman in the TSBD.
2. Police inspector Herbert Sawyer radioed a suspect description at 12:44 PM: 30 years old, 5" 10" tall, 165# with a .30-30 carbine. [DM: This was hardly LHO; nor does it match Howard Brennan's description.]
3. Nearly all the LHO evidence was compromised by the DPD (Dallas Police

Department); furthermore, the WC hid this fact. Carl Day's assistant, Robert Studebaker, had no formal training and only three weeks on the job, but Will Fritz was the worst offender (p. 125).

4. The WC sometimes even promoted evidence known to be false.
5. The SS (Secret Service) disturbed the crime scene in the limousine at Parkland.
6. Handwriting experts agreed that LHO's writing was on the order for the carbine. [DM: However, see the post office rules for receipt of items not addressed to the owner of the box (Galanor 1998, p. 89 and Document 37).]
7. The fibers on the carbine's butt plate matched LHO's shirt, but they were not unique.
8. None of the witnesses described the shooter as wearing a dark or plaid shirt; they said it was light colored. (LHO wore a burgundy plaid shirt and black slacks.)
9. The ex-con Givens initially said he met LHO in the lunchroom at 11:50 AM; only later did he reverse himself. The WC ignored the fact that he had recanted his original statement and instead used him as a witness against LHO.
10. The Warren Report (WR) lied: it said no other witness had seen LHO during the lunch break.
11. Carolyn Arnold told the FBI that LHO had stood inside the front door just minutes before 12:30 PM. The WC did not depose Arnold.
12. The 6th floor carbine belonged to LHO (p. 25). Thomas cites Buell Wesley Frazier, who said that LHO had carried the package under his arm, which was impossible even with a dissembled M-C. If that is accepted, then Thomas does

not address the question of how the M-C got into the TSBD. Likewise missing from Thomas's analysis are all the many anomalies about the M-C: how it does not match the ad, how payment was received at the gun store in Chicago, how the post office had no receipt for its collection, etc.

13. Mrs. Paine stored packages of curtain rods wrapped in brown paper in her garage. The WR omitted this bizarre fact.
14. The DPD did not find LHO's prints on the paper wrapper, but the FBI found them there later!
15. No gun oil was found on the paper wrapper, even though the FBI said it was well-oiled.
16. No direct evidence linked the paper wrapper to the gun.
17. No conclusive evidence linked the blanket (or any other object at the Paine's) to the carbine.
18. The fibers on the paper wrapper may have come from LHO's blanket.
19. The WC did not have photos of the paper wrapper at the sniper's nest.
20. No official report identifies the discoverer of the paper wrapper.
21. Will Fritz hinted that the paper had been discovered later—and that it had not been found in the sniper's nest—although four officers said they saw it in the sniper's nest.
22. The boxes in the sniper's nest had been piled up that day by a work crew. The crew said they had not seen LHO that day.
23. Neither the HSCA nor the WC had firm evidence that LHO had visited the 6th floor during the critical time interval, or even that he had ever been there that day.
24. The boxes in the crime scene photos were an ex post facto reconstruction. Three different arrangements can be seen in extant photos.
25. Tourists to the 6th floor can still view the "gun-rest," but there was none.
26. Chicken bones prove that someone (probably Bonnie Ray Williams) was in the sniper's nest, at around 12:15 PM, just as Carolyn Rowland had testified; furthermore, he may not have left before 12:25 PM. The WC had placed LHO in the sniper's nest by 12:00 PM.
27. Givens (the ex-con) was the only witness to place LHO at the crime scene, but that was only after he had recanted his original statement.
28. The only evidence for LHO at the sniper's nest was his fingerprints on boxes.
29. Police Chief Jesse Curry said there was no evidence to place LHO with the gun at the time of the shots.
30. At least three TSBD employees, other than LHO, went missing afterwards.

31. The FBI stated that the paper wrapper matched the paper commonly used in the TSBD, but another report (p. 63, footnote 55) stated the exact opposite. Therefore, the FBI might have lied. [DM: Bugliosi conveniently ignored this paradox.]

My Summary (these summaries include editorial comments from me). The WC and FBI manipulated both the evidence and the eyewitnesses. It is difficult to place LHO in the sniper's nest during the shooting.

Chapter 2: Fingerprint Evidence (p. 67)

1. The handling of the fingerprint evidence was irregular.
2. Mac Wallace did not leave his fingerprints at the sniper's nest (p. 91). [DM: This still does not seem conclusive to me. On the other hand, why would Mac risk exposing LBJ?]
3. The palm print on the carbine was not planted (pp. 68, 76-79, 110-111).
4. An unknown print was found on the fore grip of the carbine. These prints were under seal at NARA (National Archives and Records Administration); therefore, the FBI and WC knew about them.
5. Unknown prints were on the paper wrapper and on the box next to the window. [DM: No serious effort was made to find their owners.]
6. Recent court decisions, based on Daubert (p. 71), have challenged the validity of fingerprint evidence in general. [DM: This challenge seriously weakens the fingerprint case against LHO, a matter that Bugliosi does not address.]

Chapter 3: Gunshot Residues (p. 95)

Thomas claims that only one conclusion can be drawn: the DPD was incompetent and both John Gallagher and Vincent Guinn were deceptive, perhaps on purpose. Thomas does not define GSR, but it probably means "gunshot residues." I would add the following data, cited by Paul Chambers (*Head Shot* 2010, p. 172). The FBI conducted a control study in which seven men fired the M-C and then had paraffin tests taken of their cheeks. Via neutron activation analysis, all seven showed nitrates, whereas LHO had none (Chambers citation: Paraffin test records, 75-226 file, Weisberg Archives).

Chapter 4: The Murder Weapon (p. 117)

NOTE: See Appendix 3 for definitions of ammunition terminology.

1. No 7.65 mm Mauser was found. According to Thomas, Will Fritz made this mistake at initial identification, and after that everyone else just quoted him. Eugene Boone said that Fritz had made the first identification (Horne 2009, Vol. 4, pp. 1102-1103). Curiously, a Mauser had been seen in the TSBD just two days earlier (p. 123). [DM: On the other hand, Gary Savage (*JFK: First Day Evidence*

1993, p. 157) claims that the carbine was discovered by Seymour Weitzman and Deputy Eugene Boone. But I still wonder if a Mauser was found—after all, the Mannlicher-Carcano (M-C) carbine was clearly labeled: "CAL. 6.5, MADE ITALY, 40," C2766 (p. 119). Furthermore, Weitzman, who had operated a gun shop, submitted an affidavit the very next day that cited a Mauser:

Boone submitted a similar affidavit, although he later recanted. Deputy Sheriff Roger Craig insisted that he had seen "7.65 Mauser" stamped on the weapon, and he never changed his mind. His persistence led to his 1967 dismissal from the DPD, and perhaps even to his 1975 death.]

2. No purpose can be served by giving credence to the allegations of Roger Craig or Frank Ellsworth. Ellsworth said the carbine was on the 4th floor, while Craig always insisted that he had seen a 7.65 Mauser. [DM: Thomas does not explain himself well here. Furthermore, his syntax is peculiar: Why should anyone care what ultimate purpose is served, so long as truth is served? Curiously, Thomas seems to believe Craig's statement, which implies that Fritz had picked up the casings in the sniper's nest (p. 152, footnote 22.)]
3. Captain Fritz was incompetent and had no formal training in forensics.
4. The photos of the three casings were taken after Fritz had picked them up.
5. Although Fritz took written notes of his LHO interview, he told the WC that he had not (p. 152, footnote 25).
6. The sniper's nest was mauled before the crime scene unit got there.
7. The HSCA Firearms Panel could not duplicate the dent on the lip of CE-543 (one of the three cartridges found on the 6th floor).
8. Lt. Carl Day never found his personal identifying mark on CE-543 (p. 133).
9. Two sworn, but contradictory, versions exist for the chain of custody of CE-543; these were given by the same person!
10. If CE-543 was fired during the assassination, then the dent and the deeper firing pin impression must have been made after the assassination.
11. The scope was misaligned (and loose), but the degree of misalignment is still unknown. The lands and grooves were partly effaced. The bolt action was stiff. The firing pin and spring were rusty and worn, even though the FBI claimed the carbine was well oiled.
12. Lt Day did not have time to dismount the scope or to search for prints.
13. Arlen Specter has built a long political career by inventing fictions to explain away inconvenient facts.
14. The M-C carbine was the least precise of military rifles. Thomas actually uses both nouns (for the weapon) in the same paragraph (p. 120).

15. Robert Blakey (HSCA), the FBI, the Army and CBS all demonstrated that the carbine was inaccurate and unwieldy; then all of them claimed the opposite.
16. The Walker bullet was distinct (spectrographically) from the JFK bullets.
17. An American company, Western Cartridge, produced four lots of M-C bullets for the US Marine Corps in 1944. That was the origin of the Walker bullet. But why did the US produce four million rounds that were only useable by the Italian army? Was the US (via the CIA) plotting a military coup to evict the communist government of Italy (as they did in Iran, Guatemala, and the Philippines)?
18. Robert Frazier (FBI), while in retirement in 1995, suggested that the CIA furnished this ammunition to Italy, but added that it was later sold on the US market.
19. The accuracy attributed to LHO could only have been achieved with great luck, but it was not his lucky day.

My Summary. At best, the sixth-floor evidence is confusing— for the boxes, the weapon, and the ammunition.

Chapter 5: Photogrammetry (p. 157)

Thomas does not define this term, but he should have. See my Appendix 2 below. For a reference (not cited by Thomas), see *Elements of Photogrammetry* 1974, by Paul R. Wolf. Here are Thomas's chief conclusions for this chapter.

1. LHO owned the M-C carbine. [DM: For weapon conundrums, see *Harvey and Lee 2003*, John Armstrong, pp. 437-458. Armstrong would not agree with Thomas.]
2. LHO's remarkable ID cards suggest his involvement in skullduggery, more than the WC wanted to know.
3. The backyard photos are authentic—the strap change from rope to leather by itself is compelling, according to Thomas. [DM: Regarding the *LIFE* photo, see Galanor 1998, Document 30. Also see Galanor's Document 36, a silhouette (shown here in my Figure 1), which was found in the DPD files, but which is not discussed by Thomas (or even by Gary Savage). Furthermore, the weapon in the photo may not be the one found in the TSBD (Fetzer 2003, p. 98-99). Finally, see my Summary below—with new information from the recent CAPA meeting].

Figure 1 Silhouette of Backyard Man

4. If photos of LHO's body had been subsequently inserted into this image (in the darkroom) then, according to Thomas, during stereo viewing these photo inserts would seem to float detached above the background (p. 162). [DM: I emphasize this point—because I saw precisely this effect at NARA on the back of JFK's head in the autopsy photos. This result means to me exactly what Thomas says it must mean: someone altered JFK's haircut in the autopsy collection. I shall return later to this discussion, partly because Thomas concludes that the autopsy photos of the back of the head are authentic. But they can't be—Thomas and I disagree profoundly here.]
5. Three photos of Walker's home were found among LHO's possessions—and they had been taken with LHO's camera.
6. Mohrenschildt called Walker a fascist and compared him to Hitler; LHO greatly admired Mohrenschildt. Two weeks after Mohrenschildt's comment LHO began to reconnoiter at Walker's place and more than two weeks after that he ordered the carbine. [DM: This is all fascinating but still not proof that LHO fired at Walker.]
7. Thomas discusses the window boxes at some length, but I wonder why this section is segregated from the earlier discussion of the boxes.
8. Badgeman is an optical illusion. [DM: Gary Mack, one of its discoverers, would have been relieved to hear that.]
9. Black dog man, however, may be real. His role may have been to discourage tourists from getting too close to the shooter behind him.
10. Someone in the DPD sold backyard photos to the media. [DM: William Manchester (*The Death of a President* 1967, p. 635) claims that Marina got

$5000 for one of these photos; she also got $20,000 for LHO's diary. Manchester also states that Marina, oddly enough, destroyed some of these photos (p. 457).]

11. Regarding the various photos of the window boxes, their photographic sequences cannot be established, at least not by measuring shadow lengths.

My Summary. Although Thomas accepts all the backyard photos as authentic, I remain dubious. He does not discuss Marina's destruction of some of these, nor does he address the silhouette found in the Dallas Police files. [**UPDATE**: At the recent CAPA meeting (November 15, 2018) in Dallas at the Old Red Museum, I watched a current video of James Wagenvoord, who handled these original photographs for LIFE magazine. He was certain that portions of these photographs had indeed been altered. So, Thomas was wrong to conclude that they had not been rehabilitated.]

Chapter 6: The Zapruder Film (p. 193)

1. The Zapruder family released an enhanced version of the film, with frame numbering, which is an invaluable aid for researchers. [DM: Most of these claims are wrong. For a detailed critique, see "Which Film is the Z-film?" in Fetzer 2003, p. 29.]
2. Don Olson and Ralph Turner (*Journal of Forensic Science* 1971) published a frame-by-frame analysis of the Z-film. Olson earned his Berkeley PhD under Prof. Rainer Sachs ("Two perturbation problems in general relativity"), while Turner was a Michigan State criminologist. They concluded that JFK was first hit during Z-186 to Z-190. Hoch and Olson were acquaintances via Berkeley physics. Hoch, although a member of the Alvarez group, never worked with him or even took a course from him. Arthur Snyder is a physicist at the Stanford Linear Accelerator Laboratory, who earned his physics PhD (1975) from the University of Illinois. [DM: These biographical corrections were submitted by Paul Hoch.]
3. JBC (Connally) swiveled quickly to the left during Z-162 to Z-167. Michael Stroscio (misspelled as "Strocio" by Thomas), a Duke physicist, found a jiggle onset at Z-152, which matches JBC's swivel.
4. JFK jerked forward during Z-194 to Z-207. Jackie snapped her head around quickly to look at JFK during this same interval.
5. At Z-224, JBC stiffened and his lapel flapped. [DM: See Costella's Z-224 at: http://www.assassinationresearch.com/v2n2/zfilm/zframe224.html]
6. At Z-313, JFK's head bursts open. Thomas claims that the timing of the head shot is not in doubt. [DM: This latter claim ignores much contrary evidence—see my discussion in *Assassination Science* 1998, edited by James Fetzer (pp. 285-292). Also, see my discussion in Fetzer 2003, pp. 299-301; in this latter analysis, the head shot seems closer to Z-321, as opposed to Z-313. As often the case for the Z-film, appearance is not quite reality.]

7. Given all the above shots, we now have one shot too many for the WC.
8. The jiggle analysis by Alvarez assumed a six-frame delay for reaction time, but that is far too long for a reflex. Alvarez failed to distinguish between a voluntary reaction and a reflex.
9. Alvarez changed his critical frames: in *AJP* (1976) he listed Z-177 and Z-215, but earlier he had cited Z-197 and Z-227 (pp. 205 and 219, footnote 43). [DM: I heard Alvarez give his original talk in Los Alamos in late 1975.]
10. The blur at Z-313 cannot derive from a sniper's nest shot, but it is consistent with one from the GK. In my opinion, this is a crucial issue, too long ignored by WC loyalists. I agree with Thomas about this—and have for some time. Also, see my frame-by-frame analysis of the Muchmore film (Fetzer 2003, p. 302); no hints of any gunshots appear here at all, despite Muchmore's closer proximity to the TSBD (compared to Zapruder). This paradox raises deep questions: Does jiggle analysis have any value at all? Or was the Muchmore film altered, possibly in a simple-minded way, perhaps with the expectation that it would not be so closely scrutinized for alterations (as the Z-film would be)? And why is the Muchmore film damaged at precisely the head shot frame? (See The Killing of a President 1993, Robert Groden, p. 37.)

My summary. Reconstruction of the Dealey Plaza crime scene, based on the extant Z-film, is at best a soft science. If the film has been altered, then reconstructions based on it are hopeless. On the other hand, the analytical work of Costella, which focuses on film anomalies, is in another league and, for many (including me), provides actual proof of alteration. My observations on the Muchmore film raise another thicket of questions. Thomas does not address any of these concerns.

Chapter 7: The Neck Wound (p. 221)

We now arrive at a subject of profound interest to me—the medical evidence. Here Thomas and I have some major accords but also some weighty disagreements. For example, we both believe in evolution—of the autopsy report. Horne (*Murder in Dealey Plaza* 2000, edited by James Fetzer, pp. 271-273) has documented these likely changes, which occurred over a surprisingly long interval (Thomas, p. 230). Also see Horne's update (9/24/2010) at:

http://insidethearrb.livejournal.com/.

Thomas and I both agree that a shot struck JFK from the front, very near the hairline, directly above the right orbit. But after these agreements, things go off track, quite badly in fact.

Thomas claims (p. 232) that Malcolm Perry's surgery (at Parkland) completely obscured the throat wound. Perry, however, claimed the opposite: he said that he had left the wound "inviolate." Thomas does not cite evidence for his contrary conclusion.

Thomas claims that Dr. Perry made his original incision rather large, and Thomas quotes Perry as saying that he did this on purpose: Perry wanted to see clearly on either side of the trachea so that he could assess for trauma. Based on Perry's recollections for the WC (6H10 and 3H370), Milicent Cranor concurs, i.e., Perry did make a rather large incision, as that was the standard procedure for neck trauma cases. (See Cranor's March 10, 2018, "Ricochet of a Lie" at:

https://kennedysandking.com/john-f-kennedy-articles/ricochet-of-a-lie-and-kennedy-s-throat-wound

…and "Collar incision for lower midline wounds of the neck," *J. Trauma* 1978; 18(1): 2-7 [11]. The latter includes a sketch of a rather wide horizontal incision.)

Oddly enough though, when David Lifton (*Best Evidence* 1980, p. 347) asked Perry, he recalled the incision as only 2-3 cm across—or possibly 4 cm at the very most. Charles Crenshaw claimed that the tracheotomy in the photo did not match the one he saw in Dallas. Joe D. Goldstrich, a medical student, who had been learning about tracheotomies that very morning (at Parkland), and who had seen JFK's neck before and after the tracheotomy, said that when he saw the incision in the autopsy photo, he was stunned (*JFK: Breaking the Silence* 1993, Bill Sloan, pp. 84-97). John Ebersole, the autopsy radiologist, expressed his horror to me at seeing such a large and irregular incision; he told me he would never do one like that. On the other hand, Dr. Robert McClelland told Cranor that the Parkland incision was quite wide. (For further discussion, see Horne 2009, Volume IV, p. 1011.) My best guess is that the Parkland incision was larger than 2-3 cm, more likely 4 cm at least, based on Lifton's interview with Perry, and based on McClelland's recollection. My review of CT cross sections suggests that 4-5 cm might be enough to inspect the carotids for trauma. Besides the size though there is the question of how irregular (or ragged) the incision looked. It is possible that some Parkland personnel did not recognize it in the autopsy photographs because it was not very neat or very precise. If that were true, then the door may lie open to surreptitious intervention between Parkland and Bethesda (in an attempt to extract a bullet from the throat area).

Thomas implies that George Burkley did not know about the throat wound during the autopsy. On the contrary, Burkley had advised George Barnum about this wound during the autopsy (*Best Evidence* 1980, David Lifton, p. 671). After all, Burkley had been in the Parkland ER, where he had pushed steroids. Would he really have missed seeing (or at least hearing about) the throat wound? Even Boswell later told the media that he knew about the throat wound during the autopsy. And then there is Manchester, who claims that Humes and friends had learned of Perry's press briefing that same day; they supposedly even learned that Perry's surgery had obscured the anterior neck wound. Manchester states that Humes telephoned Perry after midnight (Manchester 1967, pp. 432-433). Quite remarkably, this late-night telephone call is consistent with what Dr. John Ebersole told me; he also told the HSCA about the telephone call with Humes:

https://archive.org/stream/JFKEbersole/Dr.ebersoleHscaTestimony_djvu.txt

In fact, during this March 11, 1978 deposition before the HSCA, Ebersole stated five times that Humes had spoken to Dallas late that evening and had learned about the "exit" wound in the throat. Furthermore, during this same deposition, Ebersole twice recalled that he had stopped taking X-rays—specifically because of this new information. Therefore, Thomas is almost certainly wrong about Burkley's ignorance of the throat wound (from a projectile).

Ebersole also told the HSCA that the throat wound was sutured. In view of this, I specifically asked him about sutures, but he denied seeing them. Perhaps, for the HSCA, he had had a memory merge, i.e., perhaps he recalled how the throat looked after the morgue personnel had sewed up the tracheotomy. It is not often noted, but Ebersole also told the HSCA (twice during his interview) that JFK was hit from the side of the head. Of course, he also insisted that the large skull wound was occipital—in agreement with the Parkland MDs. This is particularly striking because he, as the official autopsy radiologist, had seen the skull X-rays. This alone refutes those mavens who claim that the extant skull X-rays do not show an occipital hole. In fact, they do; Ebersole was right.

The Parkland doctors, quite aside from Perry, were very clear that the throat wound represented an entry (p. 245, footnote 34). Dr. David Stewart also said that all the Parkland doctors in attendance had concluded that the throat wound was an entry (*Post-Mortem* 1975, Harold Weisberg, p. 60).

[**UPDATE**: We now have the reports of at least three physicians, each of whom were told directly by Perry that the throat wound was an entrance. One of these is Donald Miller, MD, who spoke at the above-cited CAPA meeting at the Old Red Museum.]

Cranor makes one more perceptive observation. She quotes Humes (2H367):

When examining the wounds in the base of the President's neck anteriorly we noted some contusion and bruising of the muscles of neck.

As a control site, Humes had also stated (2H367):

Those wounds [made at Parkland on the chest, the arm, and the ankle] showed [emphasis added] no evidence of bruising or contusion which made us conclude that they were made during the agonal moments…

Humes clearly understood that bruising could only occur when blood was flowing through the tissues (while the heart was beating), but after the heart stopped, no further bruising would occur. Therefore, Perry could not have caused the bruising in the neck muscles. Rather, such bruising must have occurred while JFK was still alive (on Elm St). Humes understood this while at the autopsy; it was not a conclusion that he reached after the autopsy. So, he must have known (although could not explicitly admit) that he had identified a (projectile) wound in the neck—while at the autopsy. That is, of course, what Boswell also later recalled, i.e., both Humes and Boswell knew at the autopsy that the tracheotomy incision incorporated a separate (projectile) wound.

Cranor also reminds us that Perry had told the HSCA, *"The edges were bruised."* Since a bruise is a contusion, this strongly suggests an entry wound. She also cites Vincent

DiMaio's book, in which he reports that entrance wounds can have slightly ragged edges. Furthermore, during the WC, Specter had asked Perry if the wound had been "pushed out" (as might be expected for an exit wound), but Perry replied that it was not pushed out. In Cranor's essay, she also cites Thomas for several other errors. See "Suppressed Evidence of JFK Throat Entry," by Milicent Cranor, March 5, 2019, at:

https://kennedysandking.com/john-f-kennedy-articles/suppressed-evidence-of-jfk-throat-entry.

Regarding body alteration, Thomas makes his bias clear (p. 225): there was none. That clearly places him well outside the camp of David Lifton and Doug Horne. My views (in disagreement with Thomas) are obvious in my review of Horne's book:

http://assassinationscience.com/HorneReview.pdf

Thomas does not address body alteration in any detail. He does, however, accuse the autopsy photographer of failure to document the head wound properly. However, since this individual was the award-winning photographer, John Stringer, this dismal photographic record, by itself, raises questions about the authenticity of the autopsy collection, but Thomas is reluctant to open that Pandora's Box.

I agree with Thomas that, in their hurry to remove JFK's clothing, the nurses' scalpels created the slits in the shirt. (He cites the doctors (p. 312), but I suspect he really means the nurses.) I have seen the shirt at NARA; that scalpels caused these slits seems obvious. But Thomas concludes that, despite its small size, the throat wound was an exit wound. He bases this on (1) the non-frangibility of a metal-jacketed bullet (e.g., the M-C bullet) and (2) the buttressing of the skin while supported by the collar (see Lattimer's experiments). However, for multiple reasons this argument stretches credulity beyond reason. I suspect that Thomas is merely trying here to build his case for his SBT (single bullet theory).

Regarding the throat wound I would add the following. WC loyalists like to cite medical articles that ER personnel cannot reliably distinguish entry from exit wounds. Even if true, though, that comment obfuscates the situation here. To the contrary, in this particular case several facts trump those medical reports: (1) such a tiny exit wound could not be duplicated in experiments by Olivier and Dziemian at the Edgewood Arsenal (also see Inside the ARRB 2009, Douglas Horne, Volume IV, p. 1083 and Michael Kurtz, The Assassination Debates 2006, p. 35); and (2) Milton Helpern (who had done 60,000 autopsies) said that, under similar conditions, he had never seen an exit wound that was so small (Where Death Delights; the Story of Dr. Milton Helpern and Forensic Medicine 1967, Marshall Houts). The reader should also view the goatskin tests by Army Wound Ballistics experts (Galanor 1998, Document 3). Finally, note that the pertinent eyewitnesses recalled the wound as lying above the tie and above the shirt collar; if so, then Lattimer's buttressing notions are quite irrelevant.

There is one last intriguing comment about the throat wound. Although the autopsy report does not describe taking tissue from the throat area, Jan Gail Rudnicki tells a different story. He was Boswell's assistant that night, and he prepared the tissue slides

for examination. In a telephone interview with Mark Flanagan of the HSCA (May 8, 1978)

"Rudnicki stated that the specimens included tissue from the head and throat area" [emphasis added]:]

https://www.kenrahn.com/Marsh/Jfk-conspiracy/RUDNICKI.TXT.

Unless the pathologists suspected a projectile wound at the throat site, it is beyond eccentric that they would sample tissue from the throat.

Then there is the question of the magic bullet, which Thomas does not accept as causing any JFK wounds (although the HSCA did). As Thomas summarizes, its provenance has been decimated by Josiah Thompson, with recent assistance from Gary Aguilar. In the face of the persistent refusal of the relevant witnesses to identify this bullet, most likely it would never have been admitted at trial—and that alone would thoroughly devastate any magic bullet case. (David Wrone has made a similar argument against the chain of custody for the Zapruder film; see Fetzer 1998, p. 265. Wrone claims that a good lawyer could have kept the film out of the courtroom. As expected, Thomas does not address this troubling issue.) A final telling blow against the magic bullet derives from the NPIC (National Photographic Interpretation Center): before political leverage was exerted, their first scenario included a frontal throat shot at Z-190.

I turn next to Thomas's claim that the SBT is still alive and that this bullet entered JFK's back, traveled anatomically upward (despite flying downward from the 6th floor of the TSBD), exited JFK's throat and then struck Connally (JBC). This should simply be a matter of human anatomy, but the problem lies in precisely identifying the entry and exit sites. Thomas admits that this is a challenge (especially to achieve the final upward trajectory), but he hangs his case on these items: (1) JFK was tilted far forward when struck and (2) JFK's arms were up at that moment. In my opinion, these items cannot save the day. The first problem is the actual level of the back wound—serious disagreement persists. Thomas even admits that the medical evidence is ambiguous. That alone should cause him to hesitate. Burkley's death certificate places the back wound at T3 (Galanor 1998, Document 8). Ebersole, in his conversation with me, placed it at T4. The single specialty in which knowledge of internal anatomy must correlate precisely with external anatomy is radiation oncology—that was Ebersole's specialty (and it is my own, too). The bullet hole in the shirt and coat are nearly at the same level (as one another); the hole in the coat lies eight centimeters inferior to the horizontal shoulder seam in the shirt. It also lies three centimeters inferior to the top of the scapula. (I measured these distances on a male model who wore JFK's coat while at NARA; also see Galanor 1998, Document 6.) For additional evidence supporting such a low back wound, see Fetzer 1998, pp. 110-111. Then there is Diana Bowron, a Parkland ER nurse, who saw the back wound and described it as far too low for the SBT; see her notation on the autopsy photo (Killing the Truth 1993, Harrison Livingstone, p. 368; or Horne 2009, Vol. 4, pp. 1070-1072). Also see the photo of the hole in JFK's coat (Thomas, p. 227).

On the other hand, both the autopsy photo (Galanor 1998, Document 12) and the autopsy diagram (the descriptive sheet—see Galanor 1998, Document 5) place the wound much higher, close to the level of T1 or T2. While before the ARRB, when shown the autopsy photo, Boswell chose T2. Now to give Thomas a fighting chance to make his case for the SBT, I shall adopt the higher level of T1 for this discussion. But where did the bullet exit? While before the WC, Charles Carrico (a surgeon, who saw the wound at Parkland) clearly implied that the wound was above the necktie and above the shirt collar (p. 236). Bowron also reported seeing this wound before JFK was undressed (Horne 2009, Vol. 4, p. 1079)—but she could not have seen it unless it had been above the tie. Thomas, on the other hand, has chosen not to believe either of these witnesses. On the contrary, he states: "the bullet passed below the necktie." The autopsy describes the wound at the level of 3rd or 4th tracheal ring. However, Lifton notes that Dr. Baxter, who was in the ER, described the incision at the 2nd ring (6H42). Charles Crenshaw placed it at the 2nd or 3rd ring (*Trauma Room One* 2001, Charles A. Crenshaw, p. 62). If the bullet struck above the shirt collar (as the witnesses and tracheal levels strongly suggest), then Lattimer's experiment with the buttressed collar (p. 235) is irrelevant. However, giving Thomas his best chance again, we adopt C7 for the throat wound (Thomas likes this). A bullet traveling forward from the level of T1 to C7 (see Galanor 1998, Document 13) must travel upward (with respect to JFK's body). Given a shot from the TSBD, is this truly feasible? JFK's elevated arms do will nothing useful to change to position of C7 with respect to T1. And was JFK really bent that far forward? Thomas merely states (p. 242) that JFK was leaning forward at Z-224, the supposed critical frame. However, and somewhat astonishingly, he offers no quantitative analysis at this point. Despite this, though, he concludes that a bullet struck the back and exited the throat. In summary, it seems that Thomas's zeal to protect the SBT suppresses the desperate paradoxes here. The reader should also recall that we have chosen only the most favorable evidence for the SBT, i.e., C7 for the throat site (it was likely higher) and T1 for the back site (it was likely much lower).

While before the ARRB, Frank O'Neill, one of the two FBI scribes at the autopsy, openly scoffed at the SBT. He had seen for himself the level of the throat wound (the incision for the tracheotomy) and the level of the back wound, so he knew what he was talking about. He also ridiculed Boswell for later (verbally) elevating the back wound noticeably higher than Boswell's own autopsy diagram had shown.

But that is not even the last of the problems for the SBT—we have not yet considered the anatomic conundrums in the horizontal plane. Although Thomas cites (p. 237) my CT scan of an adult male (Galanor 1998, Document 45), he seems not to recognize its full import, or if he does, he evades it. This scan clearly demonstrates that a bullet entry at 5 cm to the right of the midline (in the back) and an exit near the midline of the throat must either (1) demolish bone (which was not seen on the autopsy X-rays) or (2) transit some lung tissue, thus producing a pneumothorax (also not seen at the autopsy by the pathologists nor visible now on the autopsy X-rays).

Thomas also cites the abrasion collar seen in the back wound, which implied that the projectile was traveling upward (with respect to the body). However, such an abrasion

could also have resulted from shrapnel from a bullet that bounced off the street. In my opinion (see discussion below) this wound was more likely caused by a bullet fragment, rather than by an intact bullet. In any case, though, the HSCA pathologists (p. 440) concluded that the trajectory was anatomically upward, which seems absurd for a downward bullet trajectory from the TSBD.

Even though Thomas admits that the autopsy report was later altered (p. 230), he concludes this section by claiming, somewhat paradoxically, that there is no compelling reason to believe that the available evidence has been altered! Although he does not explicitly list his "available" evidence, he must mean the level of the back wound—after all, evidence for its location is all over the back. (Also recall that Gerald Ford decided to play pathologist and elevate the back wound into the lower neck—some powerful motive was surely at play here.) The wildly inconsistent level of the back wound alone leaves the door open to possible alteration of the autopsy photo. I have previously addressed tampering of the autopsy x-rays and of the photographs of the back (see my 2009 Dallas lecture). These questions of deception should be decided purely by the evidence—preconceptions will not avail.

Finally, there is much evidence that the back wound, although one of entry, did not penetrate (p. 231), and therefore that wound cannot be part of a SBT. For starters, Humes initially reported that a bullet fell out of this wound via cardiac compression and did not penetrate. *JAMA* (January 4, 1964) reported that the first bullet struck the upper right shoulder, but then fell out of the back (see Thomas's long quotation on p. 228). Thomas even notes that someone at *JAMA*, in order to report this, must have had access to first-hand information from the autopsy. That also seems likely to me. Furthermore, in an early WC transcript (January 27, 1964), J. Lee Rankin read from a document (presumably the contemporaneous autopsy report) that a fragment had exited the front of the neck—this was obviously no intact bullet. The entire paragraph (Rankin's quote) is as follows:

We have an explanation there in the autopsy that probably a fragment came out the front of the neck, but with the elevation the shot must have come from, and the angle, it seems quite apparent now, since we have the picture of where the bullet entered in the back, that the bullet entered below [emphasis added] the shoulder blade to the right of the backbone, which is below the place where the picture shows the bullet came out in the neckband of the shirt in front, and the bullet, according to the autopsy didn't strike any bone at all, that particular bullet, and go through. (Whitewash IV: Top Secret JFK Assassination Transcript 1974, Harold Weisberg, p. 102.)

Directly contrary to the above statement, it cannot escape the reader's attention that the autopsy photo shows the back wound above the shoulder blade. To clarify whether the commissioners possessed autopsy photos, we have this dialogue between John McCloy and J. Lee Rankin during a WC executive session (January 27, 1964):

Mr. McCloy: ...They talk about the colored photographs of the President's body—do we have those?

Mr. Rankin: Yes, it is part of it, a small part of it.

Mr. McCloy: Are they here?

Mr. Rankin: Yes.

--(Whitewash IV: Top Secret JFK Assassination Transcript 1974, Harold Weisberg, p. 133.)

In short, the case for the SBT is highly tenuous and plainly inconsistent with a wide array of data. To traumatize Thomas's case for the SBT even more though, note that he does not attempt a rigorous defense of the SBT, nor does he attempt any quantitative analysis of the 3D paradoxes. That he is capable of such an analytic approach, however, is readily seen in his Chapter 12.

Here are two corrections for Thomas's footnotes.

Footnote 12: Crenshaw did not settle with the AMA for $300,000; it was $213,000.

Footnote 39: The recorded interview with John Ebersole, the autopsy radiologist, was not by Gary Aguilar, but rather by me. I keep a copy of this tape safely in a top drawer in my desk; NARA has the original. I also transcribed that interview for *Murder in Dealey Plaza* 2000, edited by Fetzer (p. 433). My CT scan (cited by Thomas) also appears in that book.

My Summary. In my view, a frontal projectile that did not exit (perhaps a shard of glass) might have caused the neck wound. The superficial back wound was probably caused by metal shrapnel that bounced up from the street.

Chapter 8: The Head Wounds (p. 247)

Thomas wonders why anyone would suggest that the photographs are fakes when they do not support the WC. To this, I would answer: Whoever said that the WC cared about consistency? Ultimately, though, the evidence must speak for itself; preconceptions about the WC are irrelevant. None of the Parkland doctors recognized the photo of the back of the head—not one of them! Even the medical assistants at the autopsy did not recognize these photos (Law 2003—see my Foreword), nor did the FBI scribes (see their ARRB transcripts). I saw JFK's floating posterior scalp via stereo viewing at NARA, no matter how I viewed these paired photos. That can only mean photo tampering. What are the chances that this particular site—precisely where so many recalled a large hole—would be the same site of such a bizarre floating image? Furthermore, we should not really care in advance whether the photo supports one theory or another, but only whether it is accurate. Surely the back of the head cannot represent reality. For example, look at the back of the shirt, which is covered in blood. If the back of the head was so pristine, then how did the shirt become so bloodied? Surely not from a superficial back wound. The photographer was supposedly John Stringer, highly respected among his peers and a multiple award winner. Furthermore, he (initially) confirmed that the large skull defect (hole) was at the right rear, a conclusion that even Thomas apparently endorses. There is something very wrong with that photo of the posterior scalp. At the very minimum, it is highly misleading—surely not consistent with Stringer's well-documented skills.

Thomas enthuses over Dr. Lawrence Angel's reconstruction of the skull (p. 251), especially his placement of the Harper fragment (see my Figures 2 – 4 here).

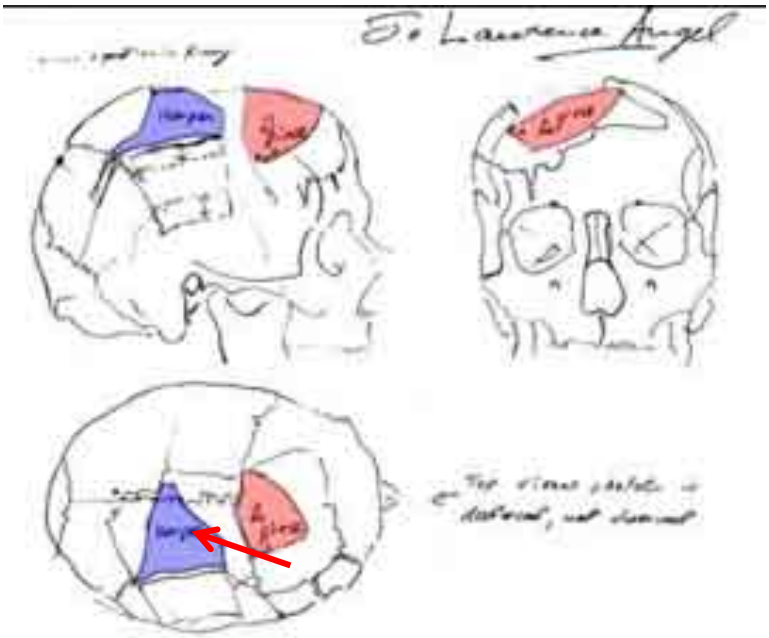

Figure 2. Angel's placement of the Harper fragment (in blue). The triangular fragment (in pink) lies anterior to the coronal suture. The red arrow identifies the metallic smudge. That it is on the outside implies an entry. I borrowed this colored sketch from John Hunt; the uncolored version was published by the HSCA.

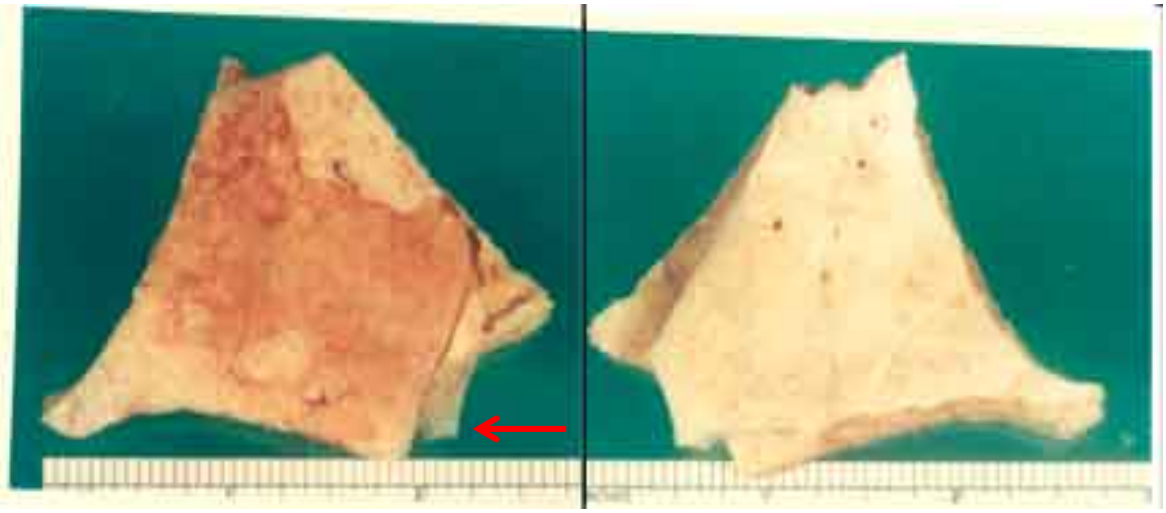

Figure 3 Harper fragment photos from Dallas. The outer surface is on the left: note the faint metallic smudge (red arrow). The inner surface is on the right; it shows no suture, and no smudge.

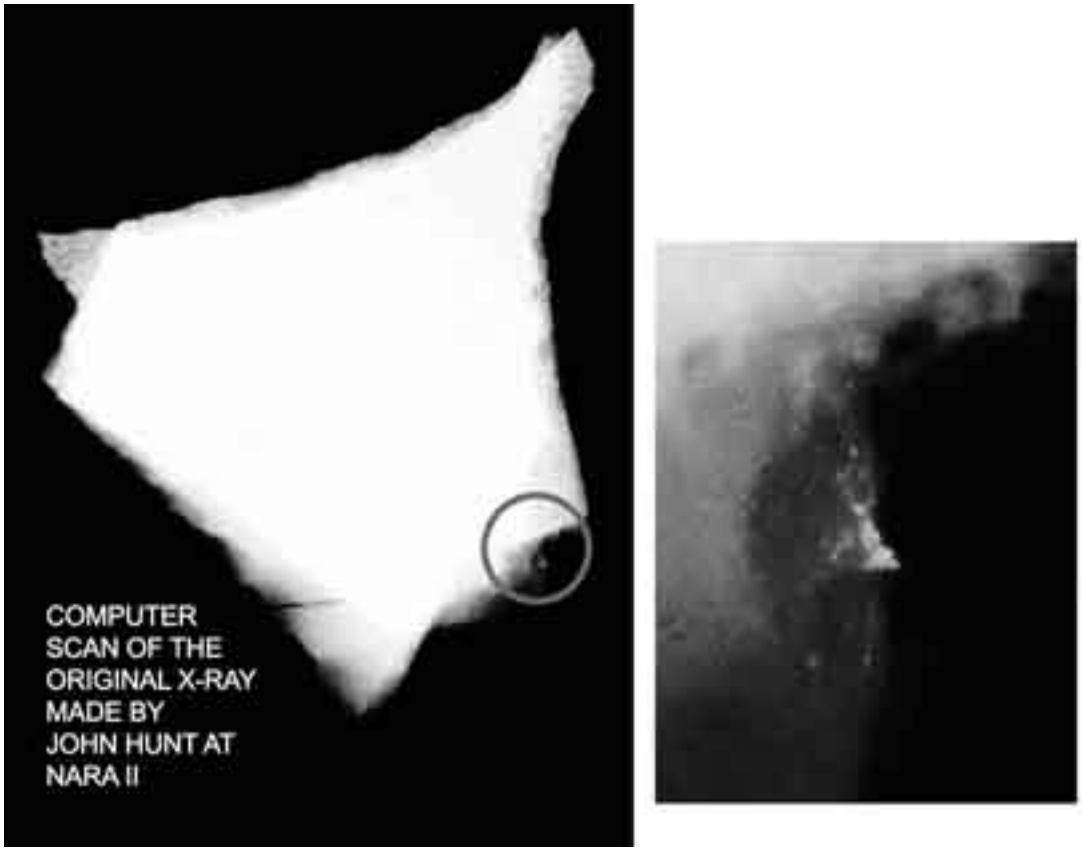

Figure 4. X-ray of the Harper fragment. Note metallic debris, circled on the left, and shown enlarged on the right. This is the same site as the metallic smudge in the photograph (Figure 3). Just rotate either photograph by 180° for easier comparison. John Hunt was the source for this X-ray, which he discovered at NARA.

But there are serious problems with this. First, Angel was not advised about the occipital hole (that Thomas apparently accepts), so Angel's options were severely constrained, especially after he named the large triangular fragment as frontal bone. (I agree with his placement of this bone fragment.) Second, Angel did not see the X-ray of the Harper fragment, which was not discovered until much later. Even worse though, Thomas does not even cite this X-ray, which was discovered at NARA by John Hunt. [Another Thomas correction is in order here: John Hunt (deceased November 2018) was not a Dr., but rather a baccalaureate graduate.] It turns out to be a critical clue. The X-ray (my Figure 4 here) shows metallic like debris just where an (apparent) metallic smudge is seen in the photograph. The Dallas pathologists (not the Parkland MDs), who personally handled this bone, described this smudge as lead-like. (I spoke to one of them myself.) Now here is the point about Angel's reconstruction (see Figure 2 here): in his sketch, the metallic smudge on the Harper fragment would lie near the skull vertex, several centimeters right of the sagittal suture. (Angel clearly states that the sagittal suture is visible on the Harper fragment (p. 250), which is also consistent with his sketch.) But here is the bad news for Angel: the smudge does not match anyone's entrance or exit point. Even worse though is this: Angel's placement requires that the smudge be on the outside of the fragment, which implies a bullet entry near the skull vertex! Surely Angel

did not mean to imply that a bullet entered here. More to the point, when I performed my own reconstruction, using a real skull X-ray in a radiology suite under fluoroscopic guidance (the only such attempt ever made), the Harper fragment ended up in the occipital area (see my Figure 5 here). But Angel's suture is merely an illusion. Based on the high-resolution FBI images (see my e-book), there is no suture on the Harper fragment. The apparent suture on the exterior is merely an optical illusion; no suture is visible on the inner surface, and none is seen on the X-ray either, even when viewed over a large range of image intensities. Angel did not know this—nor did Thomas. So, on two very serious counts, Angel (and Thomas, too) have been misled.

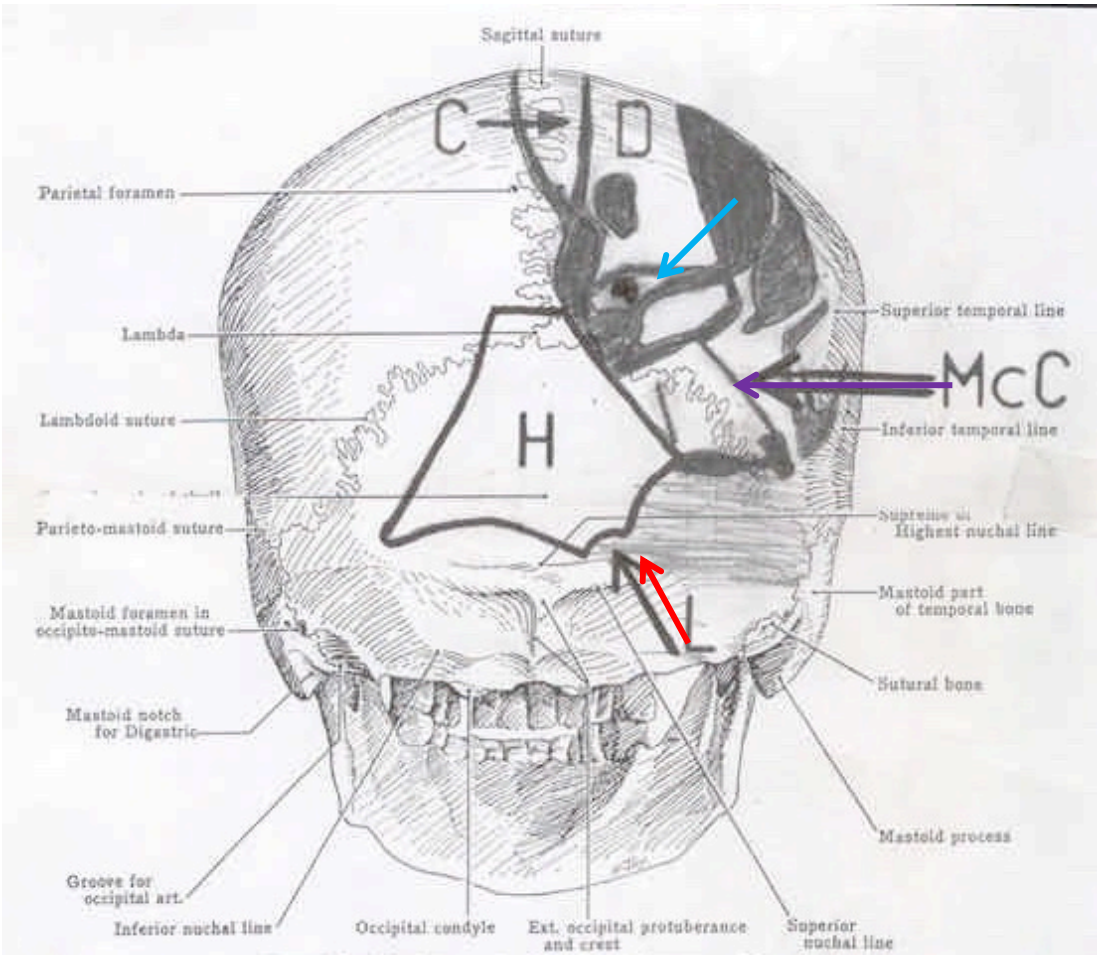

Figure 5. This is my reconstruction of JFK's posterior skull, showing the Harper fragment (H), McClelland's bone flap (violet arrow), bone islands C and D, the metallic smudge (red arrow), and the 6.5 mm object (cyan arrow). Cf. Fetzer 2000, p. 227, or my 2009 Dallas lecture, slide 21.

Even after a great deal of arm twisting by the HSCA (which favored a site 10 cm higher), the pathologists stubbornly clung to their EOP entry site. They also identified a hole in the occipital scalp that perfectly overlapped the corresponding entry site in the occipital bone. This distinctly contradicts Thomas, who states that the scalp hole matched the upper wound (see Figure 8.10 by Thomas). No autopsy pathologist ever said that—and only they could possibly have made the correct correlation. On the contrary, they have always consistently cited the much lower EOP entry.

Thomas claims that some researchers (no buffs here) have interpreted photo #44 (also known as F8) as a posterior view of the skull. Surely, he meant me (Fetzer 2000, p. 293), from which my Figure 6 (here) has been taken. (For more illustrative details, see my 2009 Dallas lecture, slide 22.) [Update: My e-book contains a much more detailed discussion of these issues.]

Figure 6. This is the posterior skull: photo F8 (alternately #16, #17 in b & w or #44, #45 in color). The red arrow identifies the EOP entry wound that Humes selected while before the ARRB. Although he had the wrong site, his identification strongly implies that this is indeed a posterior view.

It is disconcerting that Thomas should be so certain that this is not a posterior view, despite never viewing this photo at NARA. I have not only done so, but I have viewed it repeatedly in stereo. The upper left-hand corner cannot be appreciated in reproductions, but it is highly relevant. In that corner, abdominal fat is seen folded out (as it was during the autopsy) and even a nipple is visible. Until the recent review by the ARRB, I was the only observer to note these features. Now, however, I am not alone: one of the ARRB experts, Robert Kirschner (a forensic pathologist, no less), saw the same anatomy in this corner of the photo. (See my 2009 Dallas lecture, slide 58.) [UPDATE: More recently Michael Chesser, MD, a neurologist, has also confirmed this same anatomic finding, based on his own visit to NARA.] Those specific anatomic landmarks in that corner can mean only one thing: this is a posterior view of the skull. But there is more. When this photo was originally catalogued (during the "military review" by the autopsy personnel on November 1, 1966), they described it as a posterior view. Furthermore, when the ARRB asked Humes to identify the posterior skull

bullet entry, he identified a site (see my Figure 6 above) that unequivocally proves that he—perhaps subconsciously—interpreted this photo as a posterior view. My correlation of several bone fragments based on (1) the photos—as viewed in stereo, (2) the X-rays, and (3) Boswell's diagram is totally self-consistent. Such consistency can mean only one thing: this is the posterior skull (see my Figure 5 here and my discussion in Fetzer 2000, pp. 292-295). This latter discussion locates the Harper fragment in the occiput. It is noteworthy that Thomas does not even attempt a similar correlation of the X-rays, photographs, and the autopsy descriptive sheet—as my analysis does. The reader might also profit from a review of John Hunt's reconstruction, as it includes useful critiques of the HSCA: Google "A Demonstrable Impossibility." [**Update:** My e-book provides a much more detailed discussion of why the Harper fragment must derive from the occiput.]

Thomas argues against the pathologists' EOP entry by citing the trauma in the brain photos. This is, however, a step into a deep morass. (Thomas seems to accept the brain photographs as authentic, but they cannot represent reality.) Horne has extensively documented the case for a surrogate brain (see Fetzer 2000, p. 299). My OD data add even more power to Horne's argument. Also see Fetzer 1998 (pp. 120-137 and 153-158) and "Paradoxes of the JFK Assassination: The Brain Enigma," by Cyril Wecht and me, in *The Assassinations* 2003, edited by Jim DiEugenio and Lisa Pease (p. 250). The lateral X-rays (the OD measurements, especially) clearly imply virtually no brain on either the left or right side in that large dark area at the front. Paradoxically, though, the brain photos show nearly intact brain in the entire frontal region (on both left and right sides). Something is radically wrong here—either the X-rays are inaccurate, or the brain photographs are inaccurate (both cannot be correct) but Thomas does not even recognize this profound paradox. Furthermore, the supposed photographer, John Stringer, denies taking these brain photographs (for one thing, the brand of film is wrong), which means that it is an orphan—no photographer has claimed it. To base any case whatsoever on such frail evidence, as Thomas does, can only be fraught with serious risk.

But Thomas next leaps to an even more controversial conclusion: a metal fragment on the posterior skull caused the severe and numerous fracture patterns seen on the X-rays. (Thomas and Angel call this "The Radiopaque Lump"—see my Figure 7 below.) By taking this step Thomas has become a true iconoclast—no expert has ever made such a proposal. But the real problem lies on the lateral skull X-ray: here there is only a very tiny metal fragment, far too small to cause such skeletal trauma (see my Figure 8). The problem is that Thomas has based his case on only the AP skull X-ray, where the 6.5 mm metallic object (within the right orbit on my Figure 7) dominates the image. The two skull X-rays (AP and lateral) are so radically inconsistent with one another that a profound paradox ensues. To date, despite innumerable experts (including those employed by the ARRB), no one has solved this deep mystery, unless, of course, my proposal of subsequent X-ray alteration in the darkroom is accepted (see my Dallas lecture or Fetzer 1998, pp. 120-137).

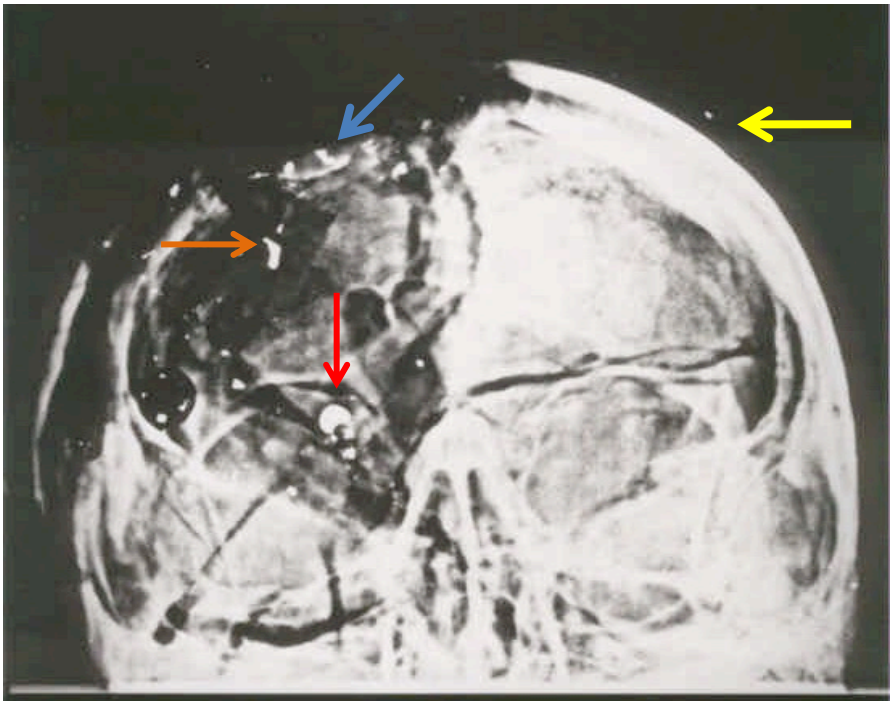

Figure 7 The AP skull X-ray. Note the 6.5 object (a fake) within the right orbit (vertical red arrow)—presumably the "Radiopaque Lump" of Angel and Thomas. The elongated 7 x 2 mm fragment (horizontal orange arrow) was authentic; Humes removed it. The trail of debris (oblique cyan arrow) lies at the very top of the skull. Also note the tiny metal fragment in the left scalp (yellow arrow).

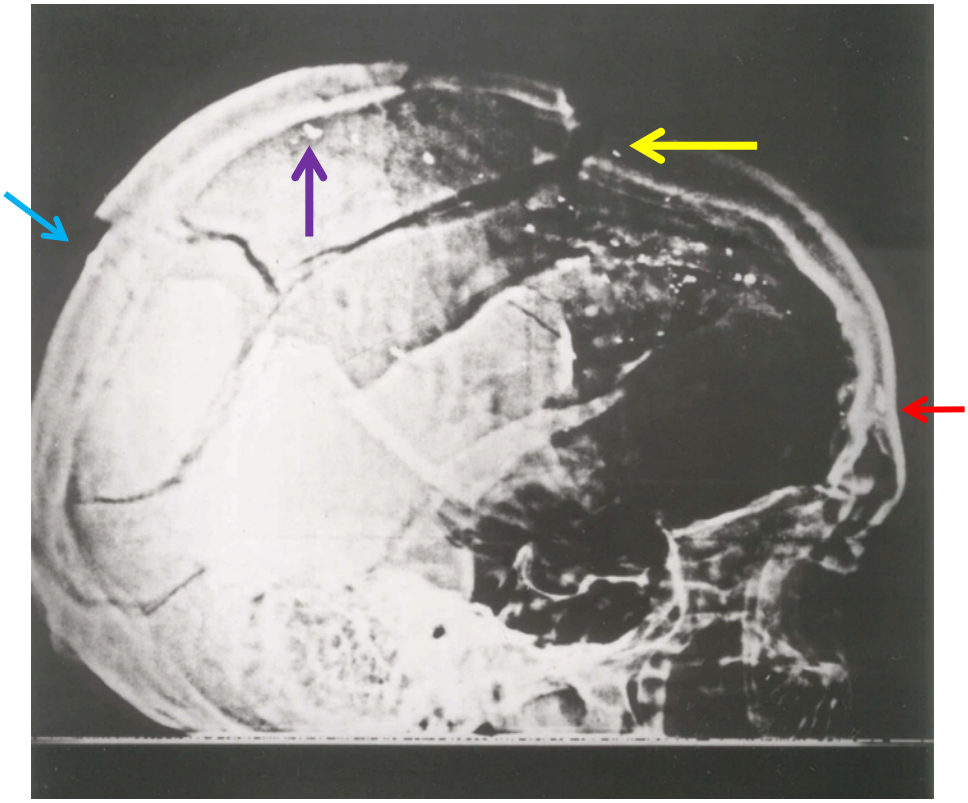

Figure 8. The right lateral skull X-ray. Note the faintly visible, tiny metal fragment (oblique cyan arrow), presumably the "Radiopaque Lump" of Angel and Thomas. This (authentic) fragment correlates spatially with the 6.5 mm object on the AP X-ray, but it is clearly the wrong size and optical density to match that 6.5 mm fake. The 7x2 mm authentic fragment (horizontal red arrow) lies far anterior. The yellow arrow identifies the tiny metal fragment in the left scalp. The largest fragment in the metallic trail (violet arrow) lies most posteriorly, which strongly suggests a frontal shot.

Thomas accuses Humes, Boswell, and Finck of dissembling about this queer 6.5 mm object (on the AP X-ray)—he thinks they saw it. Unfortunately for Thomas, all three autopsy pathologists—independently and under oath before the ARRB—denied seeing it. And when I asked Ebersole (from my own specialty) about it, he immediately and forever stopped talking about the assassination. (This recording is at NARA but has been transcribed in *Murder in Dealey Plaza.)* Even worse though, many bystanders saw those X-rays at the autopsy and not one of them reported this strange 6.5 mm object, even though such large metallic objects were the sole point of taking X-rays that night. The real problem with this forgery is that the 6.5 mm object is so radically inconsistent between the two X-ray views. The optical density data clearly demonstrate this, and the AP X-ray contains ghost images (which only occur via double exposures) inside the 6.5 mm fake object. Even the ARRB experts strongly emphasized the radical inconsistency between the AP and lateral views. (Regarding these experts, see my 2009 Dallas lecture, slides 38-40.) The FBI scribes, too, were consistent with my interpretation: they agree that Humes removed a metal fragment from the front of the skull (the 7x2 mm one). Thomas thinks the two FBI men located the 6.5 mm fragment at the rear, but that is a gross misunderstanding. Their report to the FBI described a small fragment at the

rear of the skull—probably shown by the cyan arrow in Figure 8. Furthermore, when interviewed by the ARRB, these men did not recall anything like the 6.5 mm "metal fragment" on the AP X-ray. Finally, there is the clinching statement by Larry Sturdivan, the HSCA ballistics expert: despite seeing nearly 20,000 cases of gunshot trauma, he had never seen anything like this 6.5 mm object. He does not believe that the 6.5 mm object represents a piece of metal. I agree—it is not a piece of metal. In fact, it is quite irrelevant to the Dealey Plaza crime scene. (It is, however, relevant to a different crime scene—one of illegal alteration of critical evidence in the darkroom.) Sturdivan believes that this curious object was an accidental artifact. I disagree—it is a deliberate artifact, placed at the most incredible, but pertinent, location.

The trail of metallic-like debris across the top of the skull X-rays warrants some comment. Thomas notes that this is consistent with a shot from the front. I agree. He also emphasizes that it is not consistent with a shot from the EOP. I agree again. But that is not the end of the matter. The 6.5 mm object that so dominates the AP X-ray is not on that trail of metallic debris. Thomas agrees that it lies off the trail (p. 268, Figure 8.8), but then adds that the 6.5 mm object matches the puncture wound in the right temple. I found these comments highly confusing: Thomas believes in only one headshot; therefore, shouldn't all the debris lie on just one trail? Furthermore, the tiny fragment at the rear of the skull (the cyan arrow in Figure 8) also lies far off the main fragment trail!

So, in Thomas's scenario, where did that 7x2 mm fragment come from (the one that Humes removed)? It also lies well off the main trail. One possibility is a fragment from the posterior EOP shot, the one cited by the pathologists (which Thomas apparently does not accept). Of course, that would promptly mean two head shots. See Horne (2009, pp. 1147-1155) for a thorough discussion of multiple head shots. For my review of Horne's book, as well as my discussion of multiple headshots, try this link:

http://assassinationscience.com/HorneReview.pdf.

[**UPDATE**: My e-book contains a very long discussion, with images, of three head shots, a scenario first proposed by Douglas Horne, and only later accepted by me.]

Then there is another problem: although the experts have been rather quiet about this, there are, in fact, multiple tiny metal fragments scattered widely (on both left and right sides) over the skull X-rays (these are self-consistent on the lateral and AP X-rays). There are also multiple, tiny fragments immediately inferior to—and even inside (as ghost images)—the 6.5 mm object (see my 2009 Dallas lecture, slide 33). The most obvious extraneous fragment is high on the left side of the skull, lying within the scalp. This is visible on virtually every reproduction I have seen in books and online images. It is even faintly visible in Thomas's low-resolution image; it is readily seen in my Figures 7 and 8 here. As viewed at NARA, this tiny object certainly looks like metal, and it is spatially consistent on all three skull X-rays at NARA. The small piece at the rear (cited by the FBI and visible on the lateral X-ray—see the cyan arrow in my Figure 8) most likely represents shrapnel. That would imply a strike from the rear. Also note that this is well off the main trail of metallic debris. Thomas is therefore at least partly right: there

was a piece of shrapnel at the back of the skull—but it was tiny, far too small to cause skull fractures. (See my 2009 Dallas lecture, slide 25.) So now the key question is this: Was the back wound also caused by shrapnel? All the evidence, including Humes's inability to find a deep wound in the back, is totally consistent with shrapnel striking from the rear. If so, any SBT is dead. Unfortunately, this possibility is not even considered by Thomas, which is a great loss.

Thomas quotes Vincent DiMaio as saying that the trail of debris does not fit with a full metal-jacketed bullet, but rather fits with a high velocity hunting bullet. But no one considers a mercury bullet, which I have discussed elsewhere (see my Figure 9). [**UPDATE** from Wikipedia: *"The United States used frangible lead/Bakelite M22 bullets in aircraft .30 caliber machine guns for target practice at armored RP-63 manned target aircraft…"* Apparently, these bullets behave similarly to mercury.]

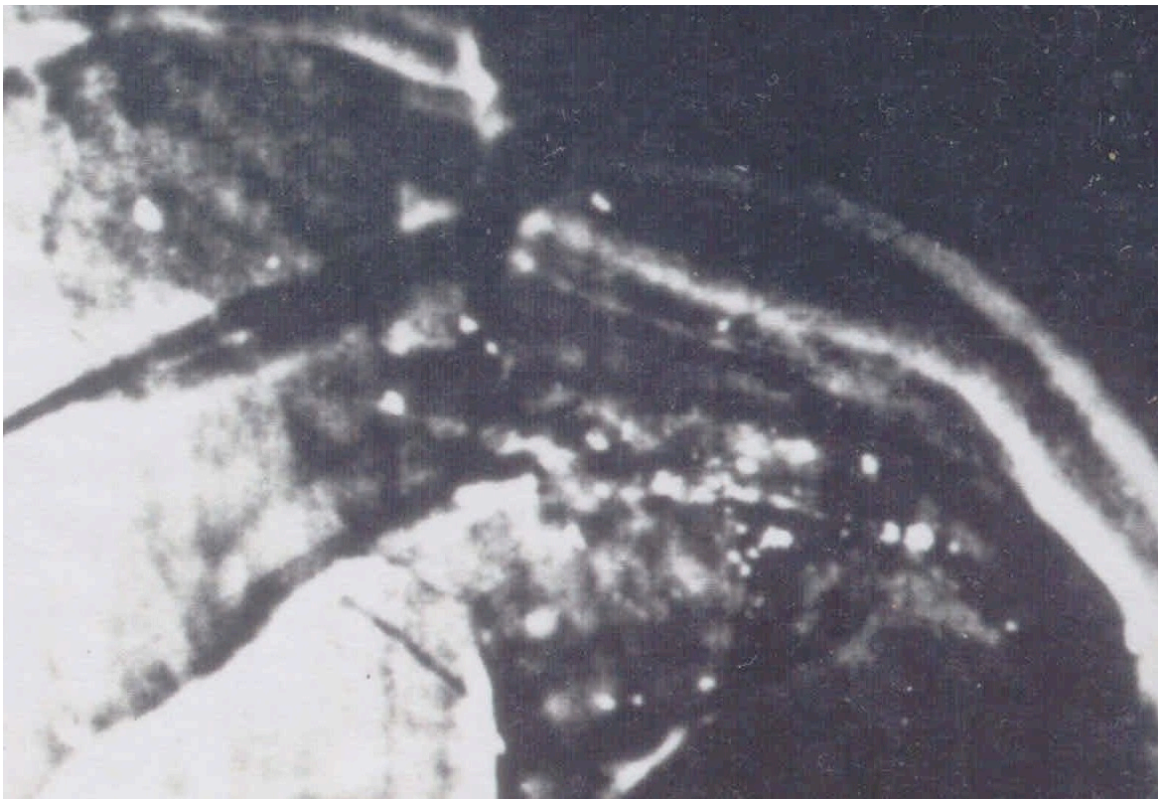

Figure 9. Close-up of the bullet trail on the right lateral skull X-ray. Most fragments have very fuzzy borders, as mercury [or lead/Bakelite] droplets might have.

I cannot claim certainty that it was a mercury bullet, but I can confirm that the borders of the fragments (as viewed at NARA) in the so-called trail are remarkably fuzzy. That stands in stark contrast to the metal fragments that were removed and are known to be solid metal (e.g., lead). By contrast, the edges of most of the fragments in the trail are ill-defined. I have repeatedly observed this distinction at NARA. In an odd coincidence that I have previously noted, such devastating mercury bullets were described—in a 1963 scene— in *The Day of the Jackal* by Frederick Forsyth (an assassination attempt on DeGaulle—see p. 149 in Thomas).

James Sibert (whom I have met) and Frank O'Neill (misspelled "O'Neil" by Thomas) heard Humes's comment about surgery to the top of the skull. When interviewed again about this before the ARRB, both men adamantly refused to change their story. Thomas apparently does not believe them, but it should be noted that James Jenkins, whom I find highly credible (and whom I have met on multiple occasions), also heard this same phrase spoken by Humes during the autopsy (Horne 2009, Volume IV, pp. 1036, 1038). Horne spends several pages addressing this significant issue, a discussion that Thomas avoids.

Thomas apparently believes that the "red spot" seen in the posterior head photo represents an authentic wound. Of course, the pathologists all insisted that there had been no damage of any kind there. In fact, on some NARA photos (especially the black and white set), I saw hair growing directly out of this site. Thomas also insists that the scalp must have been cleaned up before this photo. On the other hand, the ARRB (including Horne) repeatedly quizzed autopsy personnel about this very issue: every one of them denied that any cleaning had been done before these photos were taken, so the source of Thomas's conclusion is a mystery.

[**UPDATE:** In my initial review, I closed this chapter with a discussion of the GK (Grassy Knoll) shot, which Thomas supports. I had then objected that the angle of entry (60º) from the GK was too large to produce the occipital exit hole. However, I now believe that if the trajectory was via a glancing blow this would work. In other words, I now agree with Thomas. In fact, Kemp Clark, the neurosurgeon, described the wound as tangential (although **oblique** is a better word). The orientation of JFK's head is also directly relevant. Since I do not believe in Z-film authenticity, I claim that JFK's orientation cannot be known with any certainty; but if JFK had turned even a bit toward his left, such an oblique trajectory becomes even more feasible. I should emphasize though that this trajectory in no way explains the metallic trail of debris across the top of the skull. That trail requires a separate shot, one entering at the hairline, in the forehead. For further discussion, see my e-book.]

My Summary. It is extremely challenging to attribute JFK's head trauma to a single bullet (as Thomas does). One bullet from the GK likely struck at a glancing angle near the right ear, and then produced the large occipital hole. A mercury [or lead/cordite] bullet may have struck from the front—at the hairline, and then deposited the metallic trail without exiting. [Update: See my 2015 e-book for a detailed discussion of three head shots, including one from the rear—in agreement with Douglas Horne's initial proposal. The rear shot was reported by the pathologists.]

Chapter 9: Terminal Ballistics (p. 297)

Thomas claims that JFK would not have been able to raise his arms after his spinal cord was damaged. He believes this damage was caused by the bullet through the back, i.e., his SBT. What he seems to forget, though, is that only those nerves inferior to such a level would be knocked out, whereas those superior to it would be unaffected. There are two issues here: (1) Was the spinal cord damaged? (2) If so, at what level? For the first question, there is no answer: the pathologists simply did not examine the cord. For the

second, the level is in serious doubt, as the above discussion about the SBT has already suggested—it might have been as low as T4. Thomas wants to place the cord injury at about C7, which would impact the central brachial plexus (the plexus encompasses C5 to T1). However, the deltoid muscle (see my Figure 10) is the principal abductor (that implies movement away from the body) of the upper arm and it is supplied by nerves from C5 and C6, which clearly lie superior to C7. Therefore, they would not have been damaged by trauma at C7. Furthermore, initiation of abduction is achieved by the supraspinatus muscle, which is also supplied by C5 and C6. However, without help from the supraspinatus, the deltoid cannot abduct by itself, unless the shoulder is first dipped a good deal. Based on these considerations, therefore, Thomas is wrong to conclude that JFK could not have elevated his arms after this gunshot.

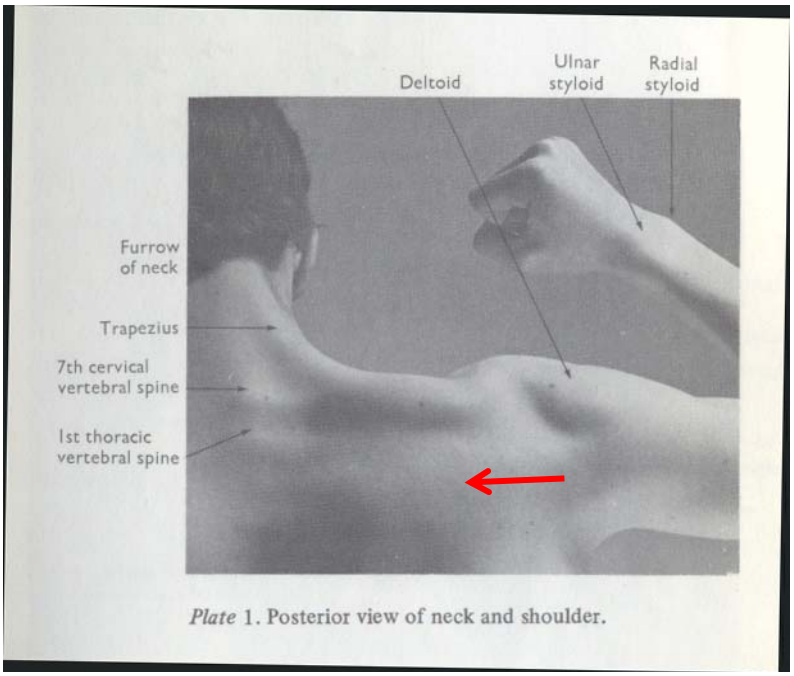

Figure 10. Posterior view of neck and shoulder. In this image, the ulnar and radial labels should be reversed. The horizontal scapular spine can be faintly seen (red arrow), inferior to the level of T1.

Thomas waffles about the exact site of the minimal vertebral trauma (C7 vs. T1), although both are cited on p. 237. (See an excellent resource for this question and for many others in this review: "How Five Investigations into JFK's Medical Autopsy Got It Wrong" (May 2003), by Gary L. Aguilar, MD, and Kathy Cunningham.) See Figure 7.5 by Thomas for the chest X-ray, where he incriminates C7. On two pages (pp. 317, 713) Thomas states that the vertebral process was knocked out of place, while at another (p. 242) he implies the opposite. On yet another page, he states that the bullet was merely deflected. In any case (in my opinion), this trauma was quite trivial. It is not even certain that it occurred during the assassination, so no serious time should be wasted on this issue.

There is a compelling alternative scenario to the SBT. If a frontal projectile caused the throat wound, and especially if this wound lay above the necktie and collar (as the

eyewitnesses stated), then the level would have been close to C7, consistent with tracheal injury between the 2nd and 4th rings. If this wound had been caused by a shard of glass from the windshield, then it might well have gone undetected at the autopsy (and on the X-rays). It could even have caused the contusion seen at the top of the right lung—and the 2-3 tiny punctures in JFK's right cheek that the embalmer had to fill. Furthermore, the shard would likely not have exited through the back, but rather would have stopped within the body. The shock wave from this shard might even have triggered the reaction of the deltoid and supraspinatus muscles (via C5 and C6), i.e., JFK's splayed arms as seen in Z-224. That both arms are elevated is perhaps a bit unexpected. The shock wave, though, might have reached (or nearly reached—which is good enough) both left and right spinal foramina (the openings in the vertebrae, where the spinal nerves emerge from the spinal cord) for C5 and C6, so that both arms were affected. There is, of course, a corollary to this: the back wound would then be viewed merely as a superficial shrapnel wound (totally independent of the throat wound). I have already noted the generous evidence on the skull X-rays for shrapnel. In addition, multiple witnesses recalled events that likely produced such shrapnel. Thomas does not address this scenario.

Thomas seems to believe that the deltoid and supraspinatus muscles did not cause JFK's arm reactions. Instead, he proposes this: *"…exaggerated splaying of the arms and the elbow flap can be interpreted as a myotactic reflex; a reaction induced by the pluck of the tendons of the trapezius muscles"* (p. 320). But then he sows confusion:

A myotactic contraction of the trapezius…, after [emphasis added] his arms were raised, would pull on the shoulder (deltoid) muscle, causing the upper arms to 'flap' as seen in the Zapruder film.

The first quote seems to explain why the arms became elevated, but the second quote implies that they were already elevated (from what, he does not say). So, I emerged quite confused from all of this. Thomas does make one item clear though: JFK's arms must have been elevated at Z-224, before he was shot through the "base of the neck." (This level of trauma, of course, would please that up-and-coming pathologist Gerald Ford, who elevated the wound to this level.) Now here follows yet another puzzle created by Thomas. The trapezius muscle is very long—it attaches to all twelve of the thoracic vertebrae. For this large muscle to respond over much of its length, merely from localized pressure on a tendon in the lower neck (i.e., as in the SBT), seems fanciful. Furthermore, the action of the upper trapezius is to draw the shoulders strongly backward and also to extend the arm, neither of which is seen in Z-224. Lastly, unless the single bullet struck both the left and right trapezius muscles, Thomas's proposal (of a tendon "pluck") cannot explain why both arms were elevated. This is serious trouble indeed. But there is even more: the tendons are in the wrong location for Thomas's effect—they are located too near the midline, much too far from the back wound.

But Thomas does not stop there—he suggests that the trapezius muscle triggered the deltoid muscle (p. 320). This is most peculiar. I have never encountered anything like this before in anatomy or physiology: muscles are not triggered by other muscles. On the contrary, they are triggered by specific nerves.

My Summary. JFK's splayed arms probably resulted from a frontal projectile. A shard of glass (from the windshield)—due to pressure effects on the nerves—is a possibility. The many witnesses to a (pertinent) windshield bullet hole (including one at the Ford plant) are discussed elsewhere, especially by Douglas Weldon.

Chapter 10: The Rearward Head Snap (p. 333)

Thomas claims that the report of the forward head movement originated with CBS news, presumably meaning Dan Rather (p. 371, footnote 15). However, this overlooks the fact that Hoover's top aide, Deke DeLoach, also reported, in his autobiography (*Hoover's FBI: The Inside Story of Hoover's Trusted Lieutenant* 1995, Cartha DeLoach, p. 139), that he saw a forward head snap in the film that same weekend. Like Rather, DeLoach says nothing about a backward snap. To further contradict the extant Z-film, no one who saw the original film that weekend reported a backward head snap. And, finally, Dealey Plaza witnesses did not report it either. In the face of all this evidence I am surprised that Thomas is certain that federal authorities knew about the (backward) head snap that weekend. No one else seemed to know. Furthermore, I have never met anyone, who, after viewing the extant film, claimed that the head went forward—but at least two presumably sane individuals (DeLoach and Rather) described precisely that. Thomas emphasizes (correctly) that Jackie could not have pushed JFK's head backwards that quickly. If the film is authentic, that would be true, but if frames have been excised then all movements would accelerate. It is likely that Jackie did indeed push JFK's head backward, although not at the speed seen in the extant film.

Both Larry Sturdivan and ITEK concluded that a frontal shot from an M-C could not account for the backward head snap. Thomas (apparently approvingly) cites controlled experiments by Olivier and Sturdivan (p. 363) with human skulls that make this point:

'...a Mannlicher-Carcano will not impart kinetic energy to the brain case in a manner and quantity sufficient to propel the head in the direction and velocity seen in the Zapruder film (p. 370) ...'

He does, however, argue strongly that a .30-06 bullet, fired from the GK, would yield a backward head speed of 2.6 ft/sec, well above the observed value seen in the Z-film of 1.6 ft/sec (see Josiah Thompson's analysis for the latter figure). His calculations, however, include important assumptions: (1) he assumes that 80% of the momentum is deposited, and (2) he ignores JFK's torso, which also moves, but the required energy (and especially the mass) is not estimated. Thomas does quote Sellier (Thomas, p. 356; also see my Appendix 5 here), who claimed that 50% of a bullet's energy is thermalized, which leaves only half for the final kinetic energy. I suspect that Thomas may be correct in both of his conclusions: (1) a M-C bullet won't work and (2) a different bullet just might do the trick. However, more experiments would be required to verify this. Physicist G. Paul Chambers (*Head Shot* 2010, pp. 210-212) also agrees that most of the bullet's energy would be lost to heat. More to the point, Chambers essentially agrees with Thomas that a frontal bullet (e.g., a Winchester .220 in the case of Chambers) could do the trick. Incidentally, he also notes that the energy required to lift JFK's body against gravity would require a negligible amount of the bullet's energy.

After repeating his calculations, I agree with him about this. If the M-C bullet cannot produce the head snap, then film alteration is left as the best explanation for believers in the M-C bullet. However, a different bullet from the front, with more momentum (and especially a higher speed), might well work, but that conclusion would then immediately destroy the WC's conclusion. After all, the WC only permitted a M-C bullet.

Thomas briefly revisits the issue of multiple head shots, but quickly dispenses with this option, saying that evidence in recent years has convincingly failed to show multiple shots and that, furthermore, the brain suggests only one trajectory. I shall not browbeat this issue much; however, I have already cited the very recent work of Horne (and my own work, which is not yet too old), neither of which Thomas cites. Likewise, the brain photographs cannot be authentic. Nonetheless, Thomas accepts them as the real deal.

The "Kronlein Schuss" effect is Thomas's primary explanation for the arresting destruction of JFK's skull. Though he may have Lattimer's experiments in mind in this specific discussion, curiously, he does not cite them until later in this chapter. Instead, he cites a textbook, *Wound Ballistics* (Beyer 1962). I agree that Lattimer's skulls did show a great deal of skull trauma (see Figure 10.3 by Thomas). It should be emphasized though that they did not, even remotely, duplicate JFK's skull wounds. Gary Aguilar has repeatedly made this point.

After some detailed calculations Thomas concludes that, contrary to the work of Alvarez, there was no jet effect. (I agree.) In addition, though, he accuses Alvarez of tinkering with his experiment in order to get the results he wanted. He also states that Alvarez should have used a M-C and a coconut. (I agree.) See Thomas's footnotes for more finely-honed comments about Alvarez.

In the section on Lattimer's stepladder, Thomas notes that the work of Alfred Olivier and Larry Sturdivan, using ten skulls shot from the rear with the M-C, did not support a jet effect either—in fact, all ten skulls moved forward! Thomas finishes this chapter with a harsh critique of Lattimer's experiment, and a similarly ruthless one of the Discovery Channel's program, *Inside the Target Car* (2008). I agree with both critiques and I have published my own critique of the latter.

My summary. WC supporters cannot attribute the backward head snap to a M-C bullet. However, a different bullet (especially with a greater mass and/or a higher speed) just might work. More likely though, the head snap is due to Z-film alteration. This head snap, despite strongly suggesting a frontal head shot, probably could not be excised in the few hours available that weekend during the reformation of the Z-film in Rochester, New York.

Chapter 11: The Magic Bullet (p. 375)

Even though the HSCA made the magic bullet (CE-399) the key to its case, Thomas does not accept it. He does, however, believe in a (different) single bullet theory (SBT). This is defined as the magic bullet theory stripped of CE-399. In other words, some other single bullet (one not in evidence) did pass through both JFK and JBC. Thomas believes this occurred at Z-224. He bases his conclusion on JFK's arm elevation, JBC's

lapel flap, and the subsequent motion of JBC's wrist and hat. (Thomas says JFK's arms did not elevate due to a shot through the neck, although he does not state what the true cause was.)

Thomas then recounts the WC's treatment of James Tague and the magic bullet theory, which even Humes had labeled "extremely unlikely." Of course, the SBT also directly contradicted the FBI report. Richard Russell even reportedly said that he would not sign onto the SBT (Thomas, p. 417, footnote 24). The curbstone (stored at NARA), which Tague's projectile had apparently struck, was discovered by Harold Weisberg to have been repaired (!), thus obscuring the original evidence. Thomas concludes that, within the framework of just three shots, neither the FBI nor the WC was able to offer a coherent explanation for Tague's wounds—or for the curbstone mark. On the contrary, they offered only bewilderment. I agree that Thomas is right on target here.

Regarding the magic bullet (CE-399), supposedly found on a Parkland stretcher, Thomas states that there is no evidence that it had ever harmed anyone (except, of course, the WC). Darrell Tomlinson, who found the bullet, adamantly refused to be coerced by Specter into supporting the SBT. Thomas emphasizes that the bullet was found on a stretcher that also held a stethoscope, but JBC's stretcher had no stethoscope. Given Specter's passion for truth, though, he did not want to know about the stethoscope.

Thomas then tackles the receipt from the autopsy for a "missile." After reviewing the Dealey Plaza evidence, including an interview with Jean "Lois" Hill" (curiously, not correctly called Jean Lollis Hill), he concludes that a bullet had been plucked from the turf and carried to Parkland, where it was dropped into JFK's coffin (perhaps by Special Agent Richard Johnsen), only to reappear again during the autopsy. Captain David Osborne, Chief of Surgery at the Bethesda Naval Medical Center, recalled that an intact bullet had fallen out of JFK's clothing and that it had been collected by the SS. The FBI, however, maintained that the "missile" receipt was for two tiny slivers of metal taken from JFK's brain. Dennis David (whom I have met and interviewed) also later recalled that he had typed a receipt (requested by the SS) for four metal fragments. Even Commander Stover, whose name was on the receipt, admitted to Lifton that <u>the bullet</u> had indeed been present at the autopsy. Finally, a memorandum written by SS Chief James Rowley stated that Elmer Todd (FBI)

...was given the <u>bullet</u> [emphasis added] enclosed in an envelope.

Thomas concludes that this bullet most likely was CE-399. After all, it has been the only intact bullet in the JFK case [UPDATE: That was the case until the recent discovery of Dr. Young's document about another bullet found in the limousine.] Furthermore, Thomas suggests that the appearance of this bullet led Humes to speculate that this bullet had fallen out of JFK's back. It should also be noted that Special Agent Richard Johnsen [DM: apparently spelled as "Johnson" on p. 407—Richard Johnsen, but not Richard Johnson, is in the index] said he had not personally carried any bullet from Parkland to Bethesda. If true, that would make the bullet's journey via the coffin more

likely. Thomas tells a fascinating story, but it has been mortally wounded by the independent analyses of Martin Hay and John Hunt (discussed below).

Humes recalled that only one bone fragment had been brought into the autopsy room (despite three obvious bone fragments on the autopsy X-rays). James Jenkins recalled several bone fragments lying in the coffin. Vincent Drain (FBI) told Larry Sneed (*No More Silence* 1998, pp. 245-262) that a security officer at Parkland probably gave him a skull fragment; presumably this was then placed into the coffin. Thomas suggests that the donor of the fragment was Doyle Williams (FBI), who got into a scuffle at Parkland with Andrew Barger (SS).

My Summary. Thomas does not believe that CE-399 (the magic bullet) caused any injuries. Instead, he proposes that some other single bullet did the same job. He suggests that CE-399 arrived at Bethesda via the coffin, but Hay and Hunt strongly disagree. So do I.

Chapter 12: Bullet Trajectory (p. 421)

This chapter is an effective annihilation of Thomas Canning's trajectory work for the HSCA. Thomas goes into exquisite (and welcome) mathematical detail to expose Canning's biases (social constructivism at work again, although Canning probably never heard that phrase). Thomas concludes that Canning's analysis was thoroughly unscientific, even though in real life Canning was a rocket scientist. Canning's analysis assumes, of course, that the magic bullet followed a straight line. However, the Forensic Pathology Panel had specifically advised the HSCA to avoid an analysis like Canning's—because the data were too uncertain. Canning then proceeded to ignore this very advice. [DM: I have criticized Dale Myers for his computer reconstruction on these same grounds, i.e., uncertain data.] Thomas explicitly, and appropriately, chastises Canning for his bizarre concept of "the minimum reasonable margin of error," a concept that does not exist in standard practice. Canning's chief concession though was this: the circle of potential error included parts of another building behind JFK! Of course, this statement was omitted from the final report. That would, after all, have opened the door to another gunman.

Thomas next tackles, in commendable detail, the trajectory of the neck wound. He concludes that the forward tilt of JFK, cited by Canning in order to facilitate the SBT, would require a man with the height of Shaquille O'Neill (misspelled as "O'Neil" by Thomas). His conclusion though is that this type of trajectory analysis can neither prove nor disprove the SBT: "*No precise alignment can be proven*" because the data are simply too imprecise and the bullet track through tissue cannot now be known. I liked this conclusion very much—that's just the way it is, but someone should tell Dale Myers.

My summary. The magic bullet is a fiction. So is any other single bullet theory, including Thomas's SBT. The throat wound (possibly from a glass shard) and the back wound (probably from shrapnel) derived from separate events.

Chapter 13: Comparative Bullet Lead Analysis (CBLA) (p. 451)

Thomas is at his best here. He destroys the utility of NAA (neutron activation analysis), as it has been historically employed. It had been Bugliosi's matchless bad luck to applaud this antiquated approach, only to see several peer-reviewed papers instantly appear to decimate his entire discussion. Of course, the ultimate irony is that Blakey had once called NAA the "linchpin" of the assassination. If that had ever been true, then that linchpin is now permanently cracked—and should be exhibited at the Sixth Floor Museum. And someone really should ask Blakey about this. I shall be brief here: it is now simply a waste of time to discuss NAA in the context of the JFK assassination. Thomas makes one final point though (which I have also emphasized): the number and size of the NARA bullet lead specimens (which I have seen at NARA) do not match the specimens removed from JFK in 1963. To say the least, that is very queer.

Chapter 14: The Tippit Case (p. 491)

I am not an expert on the Tippit escapade, so I found Thomas's discussion informative and fascinating. He concludes that LHO shot Tippit [DM: Not likely—see Joe McBride's '*Into the Nightmare*.'] The circumstantial evidence strongly implicates the Dallas police. According to Thomas, the physical evidence "*fails to pass muster*." The two Remington-Peters casings do not show the marks supposedly placed on them by officers Joseph Poe and Pete Barnes, who swore that they had indeed marked the bullets while at the scene. The two Winchester-Western casings turned up only after the scene had been searched by crime scene detective Barnes—many hours after LHO and his pistol had been in custody! Thomas therefore suggests that these items had first been removed from LHO's pistol and then later planted at the scene. Officer M. N. McDonald (a relative of a patient) swore that six live cartridges had been taken from LHO's revolver, including one with a dented primer, and that he had indeed marked all six. However, none of the six in evidence has a dented primer, or nor do any of them carry McDonald's mark.

Three different expert examinations of the firearms evidence led to three different, but contradictory, conclusions. None of these experts even considered that the evidence might be unreliable. A billfold (one of three purported LHO billfolds) was found at the scene, but it was probably not LHO's.

The radio log shows that Tippit had been dispatched to LHO's area and that he was not on routine patrol (which is contrary to the WC version). Furthermore, the dispatchers had linked suspects in a station wagon to both the JFK shooting and to the Tippit shooting. The FBI learned that this wagon was owned by Carl Mather, a close friend of Tippit, and that the suspects had been lurking at a nearby Mexican restaurant. Moreover, that same restaurant was identified as LHO's destination. Thomas concludes: *"The evidence clearly implicates Tippit and his colleagues as accessories after the fact."* Nevertheless, Tippit's widow received generous financial assistance after

his misadventures (including a huge gift from Zapruder), and he is still regarded as a hero by some Dallas residents.

Chapter 15: Lie Detection (p. 537)

This is another delightful summary by Thomas. He notes that the polygraph had well known flaws, while the problems of the PSE (Psychological Stress Evaluator), employed by George O'Toole (*The Assassination Tapes* 1975) were intractable. Did Jack Ruby know LHO? Ruby said that he did not, and the WR said that Ruby had passed the polygraph. But the WC discounted these results; after all, Ruby carried a diagnosis of psychotic depressive. The HSCA reviewed this lie detector work and cited "*gross abuses of basic polygraph principles*" and "*Total disregard of basic polygraph principles*." Thomas then details exactly how all the basic rules had been flaunted. He concludes that Bell Herndon, the examiner, had purposely sabotaged the exam and that Herndon then gave false testimony to the WC. The HSCA concluded that Ruby's exam had been invalid and unreliable. Thomas says that evidence points toward collusion between the FBI and the staff of the WC—in order to manipulate the exam results. Several footnotes have some tantalizing tidbits, as summarized here.

Footnote 16. Sociopaths are not susceptible to the polygraph. (One can only wonder how LBJ would have done.)

Footnote 19. According to a Mailer novel, John Paisley (CIA) may have been killed because he had gotten too close to the infamous Soviet defector, Yuri Nosenko.

Footnote 90. Ruby's friend, Al Maddox, produced a note, given to him by Ruby, which said that Ruby had shot LHO in order to silence him, i.e., nothing about sparing Jackie inconvenience here.

Chapter 16: Ballistic Acoustics (p. 559)

Now, at last, we come to Thomas's focus—the acoustics case for conspiracy. Here his relentless pursuit of the truth and his dogged shaking of every bone of contention show him at his best. For clarity, I first summarize the basic facts. [Those who want to skip ahead may preview our terminus in **Appendices 8-11**.]

The Basic Acoustic Facts

1. Three audio recordings are germane to the case.
2. The first recording was by radio station KBOX, but analysis to date has been inconclusive. It is not discussed further in this review.

The HSCA obtained 14 Dictabelts (Channel 1) from November 22 (each held about 30 minutes), and a magnetic tape copy of a dictabelt, from former Captain Paul McCaghren, who had taken them home from Chief Curry's office after Curry retired. The pertinent Dictabelt #10 was in such poor condition that Bolt, Baranek & Newman (BBN) concentrated their work on the magnetic tape copy. (The belt could be folded in half and mailed in a standard envelope. But if kept folded for years, this crease would crack and

the belt became unusable, as happened in this case.) For comparison and authentication, BBN made their own magnetic tape copy from the Dictabelt. McCaghren also gave the HSCA the Audograph discs of Ch-2. He had also taken these to his home for safekeeping.

Channel one (Ch-1), the primary DPD frequency, was used for general police activity. Recordings were made at headquarters on a sound-actuated dictabelt (shown in Figure 16.1 by Thomas). This is the channel that contains the suspect gunshots.

3. Channel two (Ch-2) was devoted to the motorcade that day. Recordings were made at police headquarters on a sound-actuated Gray Audograph disc. This channel does not contain suspect gunshots.
4. Silence (after about four seconds) in either channel would stop the recording.

(See Appendix 7 for possible sources of uncertainty in establishing a correct time line.)

5. Time announcements were made only to the nearest minute—there was no second hand, so exact synchrony between the two channels cannot be expected.
6. At 12:34 PM the Ch-2 dispatcher said:

Unknown motorcycle; up on Stemmons with his microphone stuck open on channel one. Could you send someone up there to tell him to shut it off?

7. On Ch-1, for 5 ½ minutes (12:28 PM – 12:34 PM), a motorcycle dominated the sounds.
8. After about two minutes, the motor noise dropped dramatically, suggesting a deceleration. After idling for about 34 seconds, the motor noise increased again.
9. Towards the end of this recording (between 260 and 300 seconds) sirens from two other vehicles are heard. A Doppler effect suggests passing vehicles.
10. On these channels, like a party line, more than one person could speak at the same time.
11. It was known that motorcycle microphone buttons could stick.
12. The first hint of shooting was on Ch-2 when Chief Curry said, “Go to the Hospital!” Six seconds earlier (per Thomas), Curry had announced his location near the triple underpass; JFK should then have been in mid-Elm St. (p. 607, footnote 20). According to the HSCA, no discernable gunshots can be heard by the human ear (i.e., they may be audible, but they cannot certainly be recognized as gunshots). What Gary Mack heard as 6-7 gunshots was merely due to his active imagination. WA reported only static where the suspect shots were identified. These “gunshots” occur within a nine second interval. They may be heard here:

https://www.nap.edu/resource/JFK_audio/

13. AGC (automatic gain control) compresses the amplitude of the original sound, but it may increase the sound of the echoes. In short, it tends to equalize amplitudes. It was triggered by sounds above 100 decibels (dB).
14. Five major buildings around Dealey Plaza typically lead to a total of ten echoes for each sound.
15. The total duration of the initial sound and its echoes (for each "shot") lasts less than 0.5 seconds (i.e., 500 msec).
16. BBN isolated suspect sounds on Ch-1, at 135-148 seconds, just seconds after the motorcycle noise decreased. Five sounds made their final cut, as possible authentic gunshots; ultimately only four were considered. Thomas discusses six and explains why he discarded the sixth one.
17. After 12:30 PM for Ch-2, a graph (Thomas's Figure 16.3) of time announcements vs. playback time (the latter is measured via a stopwatch while listening to the recording) yields a slope of about one, which suggests that moments of tape silence were minimal. Linsker, et al. do not agree with this; as I discuss below, this is a major issue.
18. A similar graph for Ch-1 shows excellent correlation between the time announcements and playback time—that is because the motorcycle engine kept the tape from stopping.
19. A study of the 60 Hz background hum proved that the police tape playback speed was 5% too fast. Therefore, the measured time on Ch-1 needs to be corrected by 5% to obtain the actual elapsed time.
20. After this correction, the difference in elapsed time between the two channels was still 35 seconds (Ch-2 was shorter). However, because of the inexactness of dispatcher announcements of time, Thomas does not consider this to be a problem.
21. Considering the imprecision in the announced times (i.e., no second hand) by the dispatchers, Thomas maintains that the cluster of suspect shots was consistent with conspiracy.
22. As a control, the entire tape (Ch-1) was searched for similar sounds, but no other similar sequence was identified. This should be qualified though: an impulse train of four seconds duration was found (thirty seconds after a bell sound), but it failed to meet the screening criteria, so it was discarded. However, as Thomas notes, this anomaly proves that something besides gunshots could produce impulse trains on the Dictabelt.

The Test Shots (p. 576)

1. Representative Samuel Devine of Ohio, a self-proclaimed expert on gunshot sounds, stated that the sounds on the tape could not be gunshots, but he had just listened to the test tape, not the Dictabelt! On the other hand, Blakey and Bowles said that the test shots were easy to hear on the test tape.

2. Test shots were fired from only two sites: the TSBD and the GK. No test shots were fired from the storm drain on the overpass—or anywhere else.

3. Only four targets were used for these test shots: the limousine position at Z-160, at Z-200, and at Z-313, as well as Tague's position (where he was struck by a fragment while near the base of the triple underpass). The selection of targets was the least sensitive of the choices made in this reproduction (p. 577).

4. To mimic possible sites for the stuck microphone, microphones (36 total) were placed at intervals of 18 feet along Houston and Elm streets. However, each array had only twelve microphones. Therefore, in order to cover all 36 microphone sites, three different arrays were required. So, this entire sequence had to be performed separately for the two GK shots (a M-C and a .38 pistol) and for the test shots from the TSBD shot (a M-C, in all cases).

5. A M-C was fired from the TSBD at the four targets. From the GK, the M-C and a .38 pistol were each fired at just three targets (Z-160 was omitted). No other shooting sites were tried. (See test patterns in Thomas's Figure 17.1).

6. This entire sequence led to 432 test patterns. Of these, 144 were produced by firing the M-C at the four targets from the TSBD, i.e., 36 microphones x 4 targets = 144 test patterns. Then another 144 patterns were obtained with the M-C withdrawn inside the window. At the GK site, for the M-C only three targets were employed (Z-160 was omitted). This yielded another 108 patterns (36 microphones x 3 targets = 108 patterns). According to the HSCA, for the GK the .38 pistol was fired only at the target for Z-313. (But I could not find where Thomas had stated this clearly.) This yielded just 36 patterns for the .38 pistol. Altogether then the total number of test patterns was 144 + 144 + 108 + 36 = 432.

Matching Analysis by BBN (p. 579)

1. The task now was to find matches, if any, between the 432 test shot patterns and the six evidence patterns. Such matches would presumably determine both the shooter locations and the target sites. For this exercise, the reader must imagine a very large matrix, consisting of 432 entries vertically and six entries horizontally. For each element of this matrix there is an evidence pattern and a test pattern, which are to be compared to one another. So, a total of 432 x 6 = 2592 comparisons must be made.

2. Matches for a specific shot were decided based solely on the time interval between spikes, i.e., amplitude was ignored (except, of course, for the already completed, initial selection of suspect gunshots). The individual spikes supposedly represented unique echoes from buildings in Dealey Plaza.

3. A deviation of eight milliseconds (msec) in time intervals was permitted, since the microphones might not precisely match the motorcycle position. Even air movement and temperature might change the matches, due to the effect on the speed of sound.

4. This statistical formula was used for detecting a match:

$$\text{Binary Correlation Coefficient} = r = \frac{i}{\sqrt{N \times n}},$$

Where i = number of coincident events N = number of spikes in the evidence pattern and n = number of spikes in the test pattern.

For a perfect match, r = 1, while r = 0 means no match. But, partly because of so much noise, a perfect match could not be expected. Results of interest were for $r \geq 0.6$; however, it should be emphasized that this was an arbitrary value. Some other value could have been chosen, with a likely different outcome, possibly even wildly different. [Notice that if i = N = n, then r = 1, a perfect match.]

5. Values for $r < 0.5$ were ignored; that left only 15 possible matches (see my Figure 11 below). These 15 had the generic pattern of gunshot echoes in Dealey Plaza. The reader must understand that this does not mean 15 gunshots! After all, duplicate test shots had been fired from the TSBD (i.e., inside and outside the window). Furthermore, matches sometimes occurred at adjacent microphones—from the same shot—as might well be expected if the motorcycle had been between two adjacent microphones. Only four actual shots were proposed by the HSCA.

TABLE 1 Correlations between impulse patterns: six segments of the Dallas Police record and echo patterns from 232 test shots. Time of impulse is relative to the start of the motorcycle motor broadcast in elapsed tape time (Data from BBN Report, Table II).

Time of First Impulse	*Microphone Number*	*Rifle Location*	*Correlation Coefficient*
136.2 sec			all < 0.5
137.7 sec	2(5)	TSBD	0.8
	2(5)	TSBD	0.7
	2(6)	TSBD	0.8
	2(6)	Knoll	0.7
139.2 sec	2(6)	TSBD	0.8
	2(6)	TSBD	0.6
	2(10)	TSBD	0.6
	3(5)	Knoll	0.6
140.3 sec	2(11)	TSBD	0.6
144.9 sec*	3(4)	Knoll	0.8
	3(7)	TSBD	0.7
	3(8)	TSBD	0.7
145.6 sec	3(5)	TSBD	0.8
	3(6)	TSBD	0.8
	3(8)	TSBD	0.7

*This impulse was reported at 145.1 by Barger et al, but then corrected to 144.9 by Weiss & Aschkenasy.

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Figure 11. These are the correlations, as shown in the original 2001 article by Thomas. The impulse at 140.3 seconds was discarded by the HSCA because it was too close to the one before it. No other reason was offered! If 144.9 sec is the GK shot at Z-312, then each of the above six sites would correspond to these Z-frames (starting from the top): 153, 180, 208, 228, 312, and 325.

6. Figure 11 lists these four shots, in chronological order: (a) TSBD, (b) TSBD, (c) GK, and (d) TSBD. (The shots at 136.20 and at 140.32 seconds were discarded by the HSCA; the final decimal places in the following discussion are found in Table 13, p. 582 of Thomas's book.) The first two shots (from the TSBD) were recorded only in microphone array #2, while the last two shots were recorded only in microphone array #3. (Positions of each microphone on the street are shown in parentheses, after the number that identifies the microphone array.)

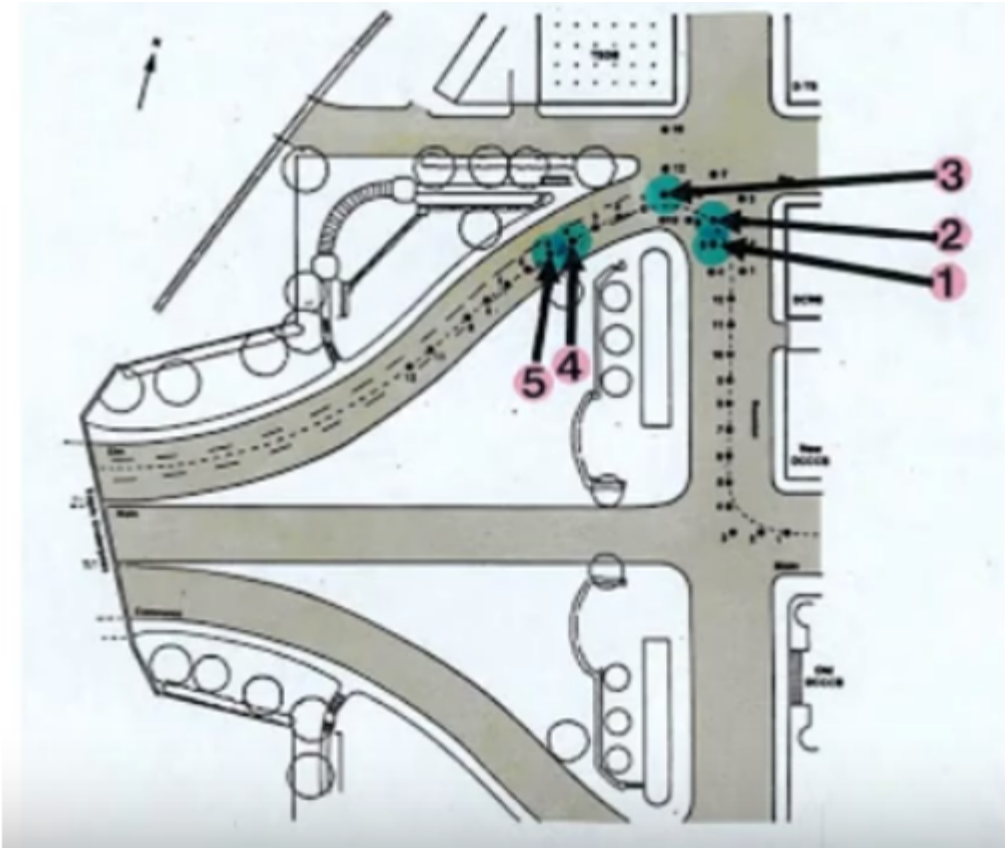

Figure 12. Microphone locations in Dealey Plaza, courtesy of Denise Hazelwood (adapted from Figure 16.5 by Thomas). The five (initial) matches, i.e., the location of the hypothesized motorcycle—not the limousine—are highlighted. The HSCA ultimately rejected #3—merely because the Mannlicher-Carcano could not be fired that rapidly (p. 590), i.e., they ignored the possibility of a second gunman.

7. The first two suspect shots were separated by only 1.6 seconds (and both were from the TSBD), which is an absurdly short interval for a M-C carbine. Because of this extremely short interval, Thomas believes that Blakey should not have permitted these two suspect shots to be accepted as official shots. Nonetheless, Blakey did it, but Thomas was correct about this. Despite the preposterously tiny time interval, both shots were attributed to LHO!

8. The initial three suspect shots (my Figure 11), which were matched to the TSBD at t = 137.70, 139.27, and 140.32 sec, covered a total interval of only 2.6 sec, with individual intervals of 1.6 and 1.1 sec. Therefore, if LHO did it, he could not have fired the 1st and 2nd shots, nor could he have fired the 2nd and 3rd shots. If very skilled, he might have been able to get off the 1st and 3rd shots (an interval of 2.62 sec). For comparison, the Army's best marksman required 2.3 sec between shots.

9. At t = 140.32 seconds (Z-228), the analysis suggested a shot from the TSBD, fired at the limo. It was recorded by a microphone near the corner of Houston and Elm. The HSCA excluded this shot merely because the M-C could not be

fired that promptly; a second gunman was ruled out a priori by the HSCA. The logic here, naturally, is that if the shot was not from the M-C then it could not be a gunshot. Thomas calls this logic by a simpler name— "bullshit." Thomas believes that this shot should have been recognized as an authentic shot, although not fired by LHO.

10. Here is what is striking for Thomas: these four matches occurred in the order expected as the motorcycle progressed on the street, and the intervals made sense. As a contrary example, if the matches had been in random order instead, then the first shot might have matched to a motorcycle far down Elm St., or the last shot might have matched to a much earlier site on Houston St. In fact, all four matches occurred in the proper order: time and space marched together, as they must in the real world. The acoustical experts and the HSCA found this order particularly impressive. (See Figure 16.5 by Thomas.)
11. All these matches occurred between 12:30 and 12:31 PM. They are found within a nine-second tape segment, with these intervals between the HSCA's four suspect shots: 1.65, 1.1, 4.8, and 0.7 seconds.
12. The time between the first and last suspect shots was 8.3 seconds. For a motorcycle to travel 143 feet (a distance that was assumed) in this interval implies a speed of 11.7 mph. Independently (based on the Z-film), the FBI had calculated the limousine speed at 11.3 mph—a lovely match for Thomas between the audio and visual data.
13. A shot from almost anywhere in Dealey Plaza was likely to mimic shots from many (perhaps most) sites—if it was recorded at the same microphone.
14. Twice as many test shots were fired from the TSBD; therefore, TSBD matches were twice as likely as GK test shots.
15. BBN finally concluded that there was a 50% chance that this match was not merely due to random noise.

Matching Analysis by WA (p. 593)

1. Mark Weiss and Arnold Aschkenasy (WA) were asked to move BBN's estimate of 50% off the fence, i.e., the statistical fence of 50-50, not the one on the GK. In particular, they needed a more precise location for the motorcycle and its stuck microphone.
2. WA focused solely on the GK shot. <u>They ignored the TSBD</u>.
3. To achieve their goals of high resolution, they would have required very closely spaced microphones (about 180 total). This promised a resolution of 1 msec. However, such a detailed approach would have been extremely tedious and ultimately impractical. Furthermore, during the time of the echoes (about 1/3 second) the motorcycle would have moved several feet.
4. So, they did not do another round of test firing. Instead, they just calculated how the echoes would look, based on the old BBN data. They got a surveyor's map

that showed the significant reflecting surfaces. Using pins on their map, they precisely measured distances—to the buildings, to the proposed GK shooter, and to the pertinent microphones.

5. In all they correlated 22 structures, which accounted for 26 major echo patterns.
6. Beginning with the impulses from the BBN GK test shot (see Figure 16.6 by Thomas) they then calculated where those impulses should have occurred (on the Dictabelt) had the microphone been moved to various (nearby) locations. (They could also calculate the pattern if the shooter had been displaced by a short distance.) In short, they performed a virtual simulation—but no more test shots were fired. The next step was to compare these newly calculated impulse patterns (for this GK shot) to the Dictabelt. Specifically, they looked carefully at the spacing between adjacent peaks (i.e., the echoes) in the pattern—but they ignored the amplitudes of the echoes. And that's where the remarkable match was made—to a motorcycle on Houston St, but very near Elm St., located five feet southwest of microphone #4. It was a perfect fit, with all 26 echoes in the test pattern (for this one shot), matched to a precision of 1 msec. WA took this one step further: they had identified a specific loud impulse as the muzzle blast (which typically lasts 50 for milliseconds). So, they used amplitude to screen for loud impulses (in order to eliminate irrelevant noises of lower amplitude); they selected 13 impulses on the test tape and 15 impulses on the Dictabelt. Comparison of these echo peaks yielded eleven coincident impulses, with an impressive binary correlation coefficient of 0.79. They calculated the odds of such a random match as only 5%. On that basis, the GK gunman was supposedly now 95% alive (well off the fence) instead of only 50% alive. They had also located the gunman behind the stockade fence, at eight feet (±5 feet) west of the corner of the fence.

Shock Wave Precedence (p. 596)

1. Weiss told the HSCA that the GK shot probably came from a rifle, rather than from a pistol. This was because of the likely presence of a shock wave (from a supersonic bullet), which was more characteristic of a rifle than of a pistol.
2. Thomas estimates the muzzle velocity at 2455 ft/sec, which helps to identify the weapon. He offers the Winchester .30-30 as a tentative fit: it was a popular weapon with a muzzle velocity of 2410 ft/sec and used a 150-grain bullet (the M-C bullet is about 160 grains.) Furthermore, within 15 minutes of the shot, Inspector Herbert Sawyer radioed headquarters, and his comments were recorded in the police log:

The wanted person in this is a slender white male about thirty, five feet ten, one sixty-five, carrying what looked to be a 30-30 or some type of Winchester.

A radio reporter (not identified by Thomas) stated:

Police are looking for a young white man wearing a white shirt and levis carrying a lever type action rifle.

Thomas notes that all .30-30s are level action rifles. Furthermore, the Winchester is a logical choice: it was specifically designed for hunting in bushy terrain where aiming and firing quickly (at moving targets) is required.

Finally, it has power, accuracy, and reliability.

3. Thomas concludes this section by emphasizing the excellent correlation now achieved between the audio sequence and the Z-film. The two anchors in time were Z-313 (the headshot) and Z-224 (Thomas's SBT event). The time interval between these two events on the Z-film was 4.8 sec. On the Dictabelt, the pertinent time interval was also 4.8 sec (after the 5% correction).

My summary. Suspect gunshots were identified on the police Dictabelt (Ch-1). They were attributed to an open (stuck) microphone on McLain's motorcycle in Dealey Plaza. The HSCA fired test shots from the TSBD and from the GK. Two successive acoustic teams finally concluded that a shot (that missed) had been fired from the GK, and that this sound had been transmitted by an open microphone on a motorcycle near the intersection of Houston and Elm St.

[**UPDATE**: For clarity, I have here added figures from the original article by Thomas. These are labeled as **Figures 13-15**.]

TABLE 2 Elapsed time measurements of DPD tapes in seconds (Data from NRC Report, Table C-1).

Broadcast	*Channel 1*	*Channel 2*
"Go to Hospital"	-	0
Last Impulse Pattern	0	-
"Hold everything secure"	0	60
Carillon Bell	7	-
"You want me Stemmons"	171	180

Figure 13. These are the elapsed time intervals, as shown in the original article by Thomas.

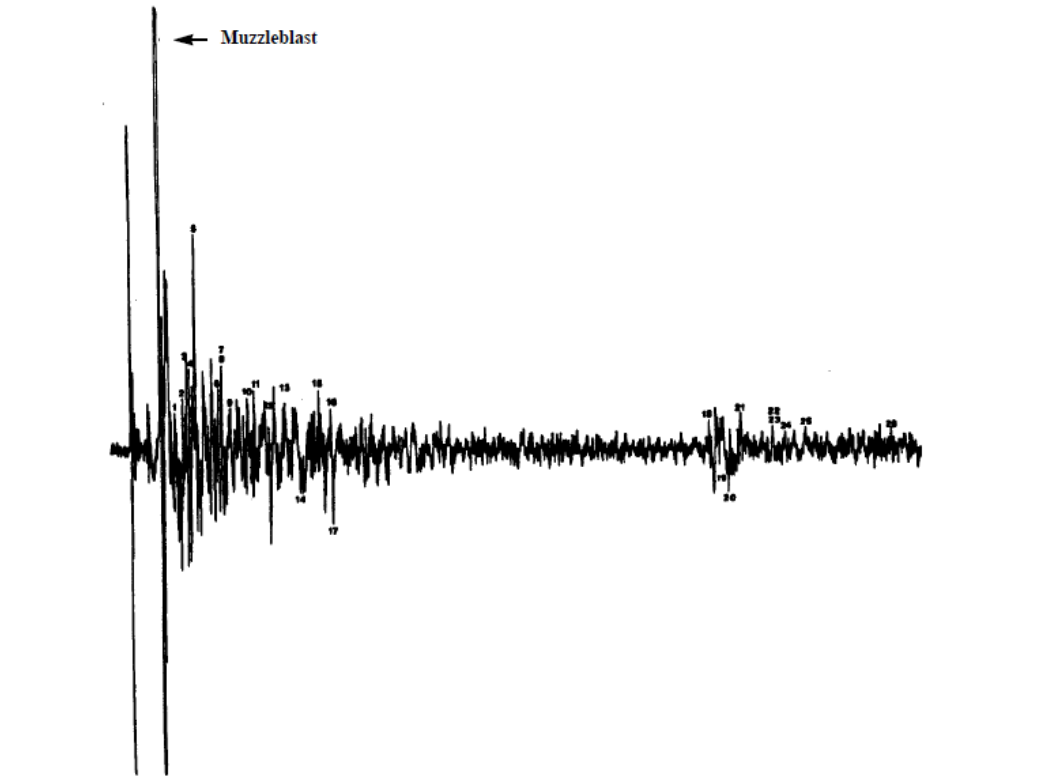

FIGURE 1 Test Pattern: oscillograph of gunshot fired from Grassy Knoll in 1978. Numbered impulses correspond to specific echo producing structures in Dealey Plaza.

Figure 14. This is from the Test Pattern, as shown in the original article by Thomas. Notice that the echoes come in two clusters, initially from buildings on or near Elm St, and then a second cluster, from buildings farther away. Typically, the Test Pattern yielded up to 26 echoes lasting about 1/3 of a second altogether.

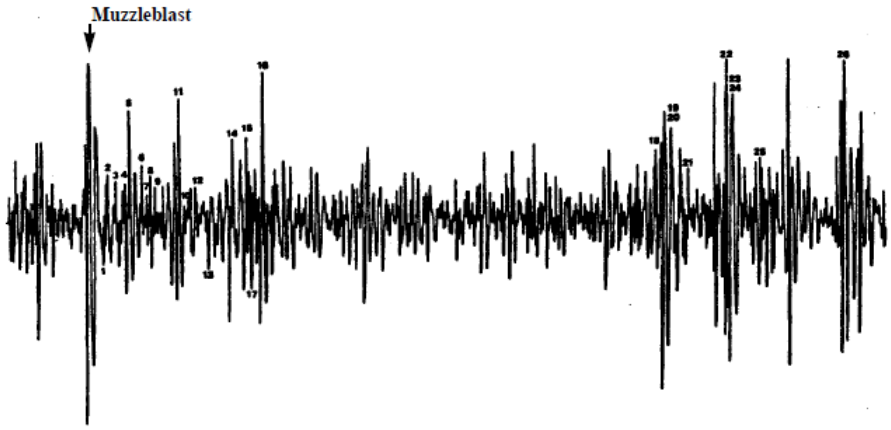

FIGURE 2 Evidence Pattern: oscillograph of impulse sequence on Dallas Police tape recorded on November 22, 1963. Numbered impulses correspond to echoes identified on test pattern in Figure 1.

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Figure 15. This is from the Evidence Pattern, as shown in the original article by Thomas. Notice that two clusters seem to appear here, although the underlying motorcycle noise makes this segregation somewhat uncertain. [See Appendix 11 (from the Sonalysts) for some doubts about this segregation into two clusters.]

Chapter 17: The Acoustics Challenged (p. 613)

1. Steve Barber discovered crosstalk on Ch-1 (the Dictabelt), which came from Ch-2 (see Appendix 4, Reference 2). In Figure 16 below the overlap between "…Hold everything secure…" and several putative gunshots is obvious. This utterance was obviously in response to gunshots; therefore, the suspect GK gunshot—since it occurred at the same time—could not be authentic. So, the NRC concluded that the suspect gunshots had been recorded well after the actual shooting (about a minute afterwards). In that case, the acoustic analysis would be irrelevant—despite the statistical correlations achieved.

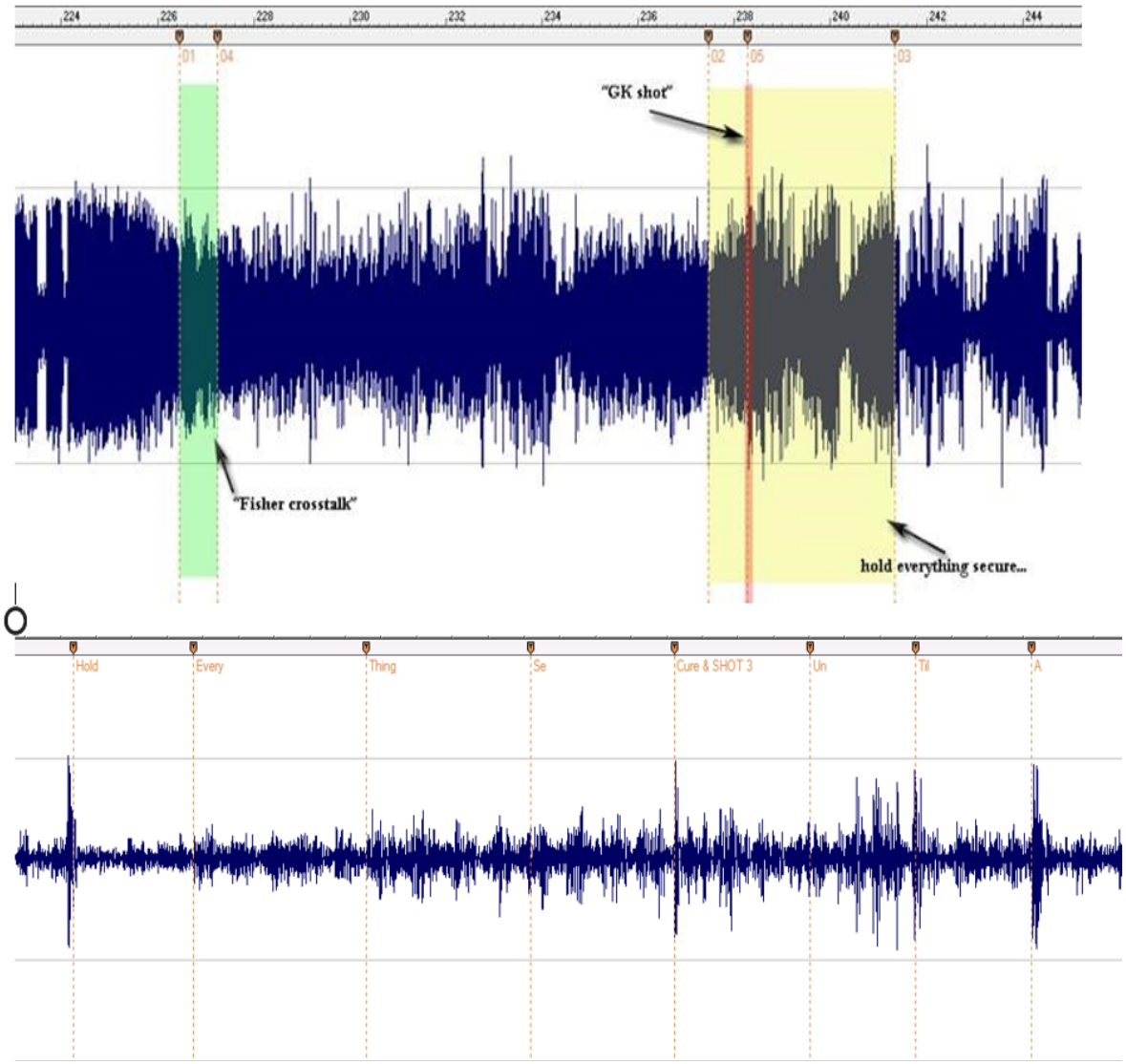


Figure 16. The Grassy Knoll (GK) "shot" versus the HOLD utterance by Decker as seen on Ch-1 (the Dictabelt). These images are from Michael O'Dell. The overlap between the two events ("GK shot" and HOLD) is obvious, as highlighted in yellow. The "Fisher crosstalk" was not an utterance by Fisher, although Thomas bases his case on that premise. A Fisher utterance is likely on Ch-2, but it did not cross over to Ch-1. In the lower image note that "Hold everything secure" is spelled out in a faint orange color. Also note the time scale across the very top of the upper image.

2. The FBI published its own analysis in December 1983. Thomas concludes, however, that no serious refutation of the HSCA was offered in that article.
3. The NRC panel did not include a single expert on acoustics, but it did include physics Nobel Laureate Luis Alvarez, who had already staked his reputation on the lone gunman.
4. According to Thomas, the NRC report is loaded with false statements. Its errors of omission were particularly damaging, e.g., it excluded the crucial evidence that

supported gunshots. The preliminary screening studies were not cited—these demonstrated that the suspect patterns had the expected amplitude, duration, and numbers of impulses. The NRC never stated that the suspect sounds were specific to about a nine second interval. Thomas cites several additional and critical misleading statements by the NRC, which demonstrate an obvious bias by them—according to him. Additional discussion follows below.

5. Here is an especially egregious misrepresentation, according to Thomas:

The identification of shots and impulses by BRSW was completely different from that by WA as demonstrated by the more than 200 milliseconds (or more than 200 ft.) displacement between the two identifications… the BRSW analysis missed the identification that WA considers to be the primary one. [DM: BRSW is not defined by Thomas, but likely refers to the study authors—Barger, Robinson, Schmidt, and Wolf—of the BBN study. BRSW is not in Thomas's index.]

Thomas states that physicist Richard Garwin (of the NRC) had argued that the acoustic teams could not both be correct. But Thomas counters that both had been correct—because the contradiction raised by the NRC did not exist.

6. Thomas explains what had happened (see Figure 17.1 by Thomas). The first echoes to reach the microphone at (3)4 near Elm St. were unique. [DM: This is the microphone from array #3 (of 3), at position 4, the site where the GK shot had been recorded by a microphone. See Figure 12 above.] The second cluster of echoes arrived 190 msec later, from buildings farther away from Elm St. This conclusion (of two separate clusters) does seem to be the pattern on the Dictabelt (see Figure 15). The problem was with the AGC—it reduced the amplitude of the echoes (for an initial interval of 100 msec) after an initial loud sound, and this led BBN to miss this early part of the pattern. They did, however, lock onto the second part of the pattern (190 msec later). But even using only half of the data, the correlation coefficient (by BBN) identified this as a GK shot. That only half of the data was required for the correlation is powerful evidence of their methodology, according to Thomas. Despite this, the NRC concluded that an error had been made.

7. The NRC reported another "error:"

Essential tests to confirm both the analysis procedure and the interpretations were omitted.

The WA sonar model was not used on the TSBD shots. But WA had not been asked to test any TSBD shots, nor had they been funded for this. Thomas asks the most reasonable question: If the NRC truly believed that more tests were essential, why didn't they just do them? After all, the NRC was not constrained by either time or funding.

8. There is a good reason why WA were able to analyze the GK shot: it required only two dimensions. On the other hand, the TSBD shot, because it was elevated above Dealey Plaza, would have required three dimensions.

9. Thomas states that only three of the four TSBD matches likely came from the TSBD. The fourth likely came from a nearby building. More data would have been required to nail this down, but these were never obtained.

Statisticulation (p. 625)

The NRC published its critique in Science, where its authors accused BBN of computational errors. However, they relegated those details to their appendix. Thomas protests, however, that these were not errors; rather, they were merely the NRC's opinion that a different set of assumptions should have been used for the tests. The NRC had not even noted the order in the data (as discussed above). In the end, Thomas insists that the NRC documented zero computational errors.

NRC's central criticism focused on the assumptions that led to the WA's 5% probability estimate. Of the 13 impulses on the test shot, 11 were coincident with the impulses on the Dictabelt (during the expected time interval). Here then is the statistical question: Given 15 static bursts (or other random noises), what is the probability of finding 11 of them in the same time slots (of ± 1 msec) as the Dictabelt candidates? This assumes a microphone located about 72 feet west (i.e., downhill toward the railway overpass) of the Elm-Houston intersection. Within an interval of 90 msec, BBN had calculated the odds as 20 to 1 against such a random event. Thomas notes that these calculations are identical to those for lottery predictions, i.e., the hypergeometric probability function p determines the outcome:

$$\frac{\binom{M}{i}\binom{N-M}{n-i}}{\binom{N}{m}}$$

where M = the number of time slots (or windows) where a spike could occur

N = the number of spikes in the test pattern

n = the number of spikes in the evidence pattern

i = the number of coincident spikes.

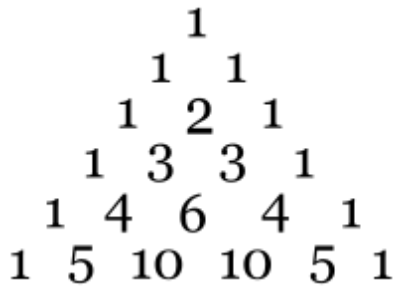
A coincident event is counted when an impulse (or spike) from the evidence pattern (the Dictabelt) falls into the same time window (±1 msec) as an impulse from the test pattern (i.e., a BBN shot from the GK). The factors in the above equation are the usual binomial coefficients although, rather mysteriously, Thomas does not tell us that useful fact. I would add that, in general, where n and k are integers or zero,

$$\binom{n}{k} = \frac{n!}{k!\,(n-k)!} \quad \text{for} \quad 0 \le k \le n.$$

For those who have forgotten their high school math:

$$n! = \begin{cases} 1 & \text{if } n = 0, \\ (n-1)! \times n & \text{if } n > 0. \end{cases}$$

Here is a simple example: 5! = 5 x 4 x 3 x 2 x 1 = 120. Tables of these binomial coefficients are online. They also form the entries for Pascal's triangle, for which a small sample is shown here:



There is even an online site for automatic calculation of the probability p (Google Hypergeometric Calculator: Online Statistical Table). In general, this hypergeometric function is used for comparing two finite populations (the Dictabelt and the test shots in this case) which are sampled without replacement, i.e., each match decreases the number of remaining impulses available for matching. Since Thomas is unnecessarily terse in his explanations here (although he clearly knows what he is doing), several points should be emphasized about p: (1) it is the probability that random chance could explain the event in question and (2) it is the probability of a match for just one position of the microphone. A bit later (sooner would have been better), Thomas recalls that the WA model employed 180 microphone positions (2 feet apart), so that the cumulative probability for a random match (i.e., considering all 180 microphone positions) is 180p. (It is like buying 180 lottery tickets instead of just one.) Suppose the final result for 180p (for a random match) is 5%; then the probability that this is not a random match becomes 95% (i.e., what is left over after randomness is subtracted: 100% - 5% = 95%).

Thomas does report, though, that BBN had made a major mistake (in interpreting data—see p. 628), which had been pointed out to them by WA. BBN thought that impulses had arrived in a 90 msec window, but it should have been 180 msec. (In the test patterns, there were two clusters of impulses, one from Elm St buildings, and a second set from buildings farther away—with a gap between them.) Therefore, BBN had overestimated the probability of a random match. (If done correctly, the probability of a GK gunman would have increased.) The NRC caught this error, which implies that they did understand the 190 msec gap between the two 90 msec clusters of sounds, and that these were not two separate patterns—as they had paradoxically asserted in their critique.

Thomas then details the extremely (perhaps even radically) conservative approach taken by the NRC, by means of which they successively whittled away at the number of matches, so that they got the p-value down to 0.22 (p. 631). One example was the set of assumptions in the WA model: the NRC identified seven. This supposedly allowed the NRC to deduct seven coincident pairs that WA had found. The NRC also deducted two degrees of freedom because the motorcycle was free to vary in two dimensions; so was the shooter, the NRC said. But this latter was incorrect. It should have been one dimension—the fence constrained movement to one dimension (i.e., the gunman did not stroll in front of the fence, nor did he amble backwards away from the fence).

Thomas correctly argues, however, that the seven assumptions were not free variables. In fact, without including motorcycle speed, air temperature, and tape recorder speed, their work would have been criticized. Furthermore, these items were not derived from the acoustical data but rather from independent evidence and had nothing to do with the acoustical analysis. Thomas next notes another error by the NRC: they had mistakenly included the same correction twice. This was the erroneous adjustment for M (the number of time slots or windows), an error that BBN had indeed made. However, WA had already corrected for this, so the NRC should not have corrected for it a second time.

Thomas then tells us how degrees of freedom should be counted: two for the motorcycle, one for the shooter, and one for the muzzle blast. This leads to a final probability of 100,000 to one against random chance as the explanation, meaning that the chances of a GK gunman vastly increase.

Ultimately, according to Thomas, the NRC had engaged in Statisticulation—dressing up minor details as serious errors and mischaracterizing the strengths of the HSCA analysis as though they were flaws.

Ultimately, though, Steve Barber, with his crosstalk proposal, had provided the NRC with their one valid argument. And, as we shall soon see, this is where the sequel article to the NRC (Linsker, et al.—see my Appendix 4, Reference 9) bases its case against conspiracy, and where Thomas ultimately runs into trouble.

The Double Decker (p. 633)

Listening to a copy of the Dictabelt recording (Ch-1), Steve Barber had heard a background voice saying, "…Hold everything secure…" But Barber had also found this same phrase by Bill Decker (who had ridden with Chief Curry at the head of the motorcade) in a transcript of Ch-2. So how did Decker's broadcast (from Ch-2) get onto Ch-1? (The appearance of this spoken phrase on both channels is the source of the pun in "Double Decker.") The answer is crosstalk, an occasional event in radio communication, which occurs when two different frequencies are open (for broadcasting) at the same time. For example, suppose that two radio units (on different channels) are close spatially and that one channel is already open and broadcasting. When the second operator initiates a call on the second channel, then the live microphone on that second channel may overhear the first channel broadcast. If so, then that will be rebroadcast from the second channel—it's a simulcast. Typically, only fragments of the conversation are transmitted. In any case, such simulcasts might serve to synchronize events between Ch-1 and Ch-2.

But how did that happen in this case? Thomas agrees with the HSCA that Officer H.B. McLain was located appropriately in Dealey Plaza and that his microphone had stuck open, so that it continuously transmitted. But the even bigger question now was: How could Decker's utterance overlap with the "shots"? After all, Decker could only advise a trip to the hospital after the shots, so this overlap defied common sense. Thomas now had only one choice: He proposed that the recording stylus had skipped backwards so

that the “shots” became superimposed over Decker’s utterance. Whether or not this is reasonable is the subject of much discussion below.

About 12 seconds after the dispatcher had announced the time as 12:30 PM, Curry (p. 567) had said on Ch-2, “Go to the hospital.” That had been the first overt clue (on the tapes) of a shooting. But here is the catch: on Ch-2, Decker’s broadcast (“…Hold everything secure...”) occurred nearly a minute after Curry’s broadcast (GO), whereas the critical Decker phrase (“Hold”) on Ch-1 (the one that Barber could barely hear) had overlapped the last of BBN’s gunshot impulses. The first conclusion from this is simple: there was no synchrony between the channels. But the deeper problem, if Barber had heard right on Ch-1, was that the entire BBN analysis had been rendered useless—because their suspect shots had occurred far too close to Decker’s “…Hold everything secure…” In other words, the suspect gunshots were not real shots, but must have some other cause, quite unrelated to the crime. Barber, of course, proceeded to advise the NRC of his observation but, ironically, the NRC at first did not even know that the police radio system worked like a party line.

Because the sounds that Barber had heard were so faint, the NRC performed an objective analysis: a digital computer sampled the double Decker sounds at 20,000 readings per second, comparing Ch-1 with Ch-2 (the latter was a copy from the DPD). The recordings from the two channels were permitted to slide across one another in time, while the computer looked for overlap of identical or nearly identical segments. This was not conclusive, so the NRC made its own recording of Ch-2 directly from the Audograph disc. This led to a correlation coefficient of 0.5 (Figure 17.2 by Thomas), which was only modestly convincing (that crosstalk had occurred for the “double Decker”).

The same tests were done for the S. Q. Bellah communication, “YOU want me to…” He had led the motorcade and had broadcast over Ch-2, but there had been some crossover to Ch-1. This correlation coefficient was 0.8 (Figure 17.3 by Thomas), much higher than 0.5, so this suggested that the NRC method had merit. It also implied that Bellah’s phrase was an example of crosstalk. In fact, Thomas had emphasized this in his original article.

But the key question was this: Could any crosstalk be relied upon for synchronizing the two channels? For example, if the Bellah crosstalk was used for synchrony (Thomas would like that), then the suspect shots on Ch-1 fall right between Curry’s two broadcasts on Ch-2, i.e., the triple underpass announcement (when the limousine was in mid-Elm St) and the “Go to the hospital” utterance (p. 567). In that case, Thomas could claim that the suspect sounds were the real shots. However, we already know that using Decker for synchrony means that the suspect sounds occur much too late. So, what is going on here?

The NRC had stated that crosstalk could reliably identify synchrony. In this, according to Thomas, they were almost certainly wrong. (However, see below regarding the question of missing time on Ch-2; Thomas insists there was none, but others disagree.) Thomas suggests possible sources of asynchrony: a recording pause on one channel, stylus

displacement (a skip from one groove to another), and/or over-recording (i.e., an overdub, more than one recording on the same groove). Arriving at this conclusion may have pleased Thomas—after all, if the "double Decker" had been accepted for synchrony, then the acoustic case was dead. But now, knowing that crosstalk could not guarantee synchrony, the acoustic case was still alive. However, as we shall see below, this will not work.

Thomas had earlier (p. 560) stated that the HSCA had acquired (and used for its acoustical data) the original Dictabelt. This (supposedly) original Dictabelt No. 10, located at NARA, is shown in Figure 16.1 by Thomas. (The HSCA had received 14 Dictabelts altogether—all from November 22, 1963.) So, was the original dictabelt used? (This JFK case already has too many duplicates: autopsy reports, autopsied brains, brain examinations, skull X-rays, posterior skull photos, Minox gear, two contradictory FBI reports for the brown paper wrapper, two Oswald's, two contradictory affidavits for CE-543, two Mannlicher-Carcano's, etc.) Now, suddenly, near the end of Thomas's discussion, this specter (not Arlen this time) raises its ugly head again—perhaps the original dictabelt was missing, after all.

The evidence for a missing original is rather remarkable (although Linsker et al. disagree—see more discussion below). The BBN study had found two powerline hums on Dictabelt No. 10: at 60 and at 120 Hz (Figure 17.6 by Thomas). The 60 Hz hum is not too surprising, but how did the 120 Hz hum get there? Oddly, for Dictabelts Nos. 11 and 12 (both at NARA) there is scarcely any hum at all. An FBI study of No. 10 found only the 120 Hz hum (Figure 17.7 by Thomas), whereas the DPD copy of No. 10 showed multiple 120 Hz hums as well as the 60 Hz hum. One explanation is that the machines used for playback caused some hums; since different ones had been employed, different hums may have resulted. Thomas notes that the Dictaphone amplifiers only covered 300 to 3000 Hz (the natural range of the human voice), so the 60 Hz hum lay below that range. Furthermore, the Dictaphones had no outlets for detachable speakers or for any other devices. Therefore, in order to transfer sounds from the Dictaphone to another device, a microphone for the new recording was merely placed (in the air) near the (built-in) Dictaphone speaker, surely a rather antiquated method. But that meant that any sounds in the room (e.g., overhead fans) would be recorded in the process. So, here is Thomas's verdict: "…the presence of prominent hums on Belt 10 which are not present on the other belts suggest the belt is a copy and not the original." Dictabelt #10 at NARA is in such poor shape (it is cracked at the line where it was folded in half) that it can no longer be used for playback. In 2004, NARA contracted with the Lawrence Livermore Lab in Berkeley to determine if optical scanning might be feasible (as had been achieved with old Edison cylinders). I asked Les Waffen (at NARA), via e-mail in late September 2010, about this issue, but got no reply. Chris Scally conducted an exhaustive study of the custody of these recordings. Based on that work, Thomas states that the:

...convoluted chain of possession of the various recordings and his [Scallys] discoveries do not reflect well on the authorities charged with safeguarding this historical evidence.

Thomas summarizes the Dictabelt story with his surprising Table 16, which shows 14 different Police Dictabelts from November 22, 1963, including two labeled as #1! NARA now has only four. No one seems to know what happened to the rest. The start and stop times vary widely and so does the total time interval. From this, Thomas infers that a duplicate set of belts once existed; it also suggests that some of NARA's belts are duplicates. His final comment is highly germane: "If the Dictabelt analyzed for the acoustical studies is a copy, then the scenarios for impositions, whether deliberate or accidental, also multiply." Thomas's comments had been preceded by Barger's comment:

The original Dictabelt could be studied more extensively for possible evidence…of being a copy… Further studies could include a careful search for a second hum…which would characterize a copy.

However, see Linsker's powerful argument below that the original had indeed been employed in the analysis. I suspect he is correct.

The NRC Redux (p. 654)

After his 2001 article, Thomas was reprimanded by surviving members of the NRC—they accused him of making significant errors. Thomas insists, however, that they only cited one error: his mistake in using their adjusted timeline for Ch-2, i.e., the problem of skips and repeats on the Gray Audograph. The NRC (and Ralph Linsker, too) claims that this contains no skips. But Thomas claims that it is missing at least four broadcasts that can be heard on the DPD tapes. In short, Thomas does not even accept this one rebuke from the NRC. Since Linsker and colleagues still disagree strongly with Thomas about this matter, more discussion follows below.

Linsker also analyzed possible crosstalk that had been overlooked by the NRC. For example, here is Deputy Chief Fisher on Ch-2: "Naw that's all right, I'll check it." According to Thomas the last three words had crossed over to Ch-1 (so he abbreviates this utterance as CHECK). Furthermore, on Ch-2, it occurs just a few seconds before the triple underpass broadcast (by Curry) and, on Ch-1, just a few seconds before the suspect shots. This case had also been cited by Jim Bowles as the exemplar of crosstalk. Bowles had been the supervisor of communications at the DPD and had prepared a transcript in March 1964 for the WC. [DM: Oddly, his name is not in Thomas's index, although it is cited many times in Chapter 17, and it is heavily represented in Thomas's endnotes for that chapter.] For Bowles's own account see JFK: First Day Evidence 1993, Gary Savage, pp. 313-370. Another source is No More Silence 1998, Larry Sneed, pp. 169-193. Even the NRC had been aware of this case but did not discuss it. Thomas emphasizes that these crosstalk sounds were analyzed by the computer, just as the Bellah and Decker cases had been, and it (the Fisher case) was indeed positive, even though Linsker (according to Thomas) never acknowledged this result. This issue is revisited below—Linsker disagrees with Thomas. (I agree with Linsker.) Thomas's significant conclusion is this:

The truth is that the Fisher crosstalk supports the acoustical analysis in that it establishes exact synchrony between the acoustically determined gunshots and the time of the assassination when used as the tie-point between channels.

He adds that the proximity of the Fisher crosstalk (just a few seconds before the suspect shots) makes it the most reliable case for aligning Ch-1 with Ch-2. Linsker and colleagues strongly dispute this, as we shall see below.

[DM: Here is another Thomas erratum: Near the top of p. 646, Figure 17.4 is cited—it should instead cite Figure 17.5.]

My summary. Steve Barber discovered a paradox—Decker's words were superimposed over some of the suspect shots. The NRC agreed with this. They concluded that these suspect sounds had arrived about a minute after the actual shooting, so the sounds were not gunshots at all. Thomas wonders if the original Dictabelt had been used in the acoustic studies. We shall soon see that it was.

Chapter 18: The Acoustics in Prime Time (p. 667)

Was patrolman H.B. McLain at the appropriate place in the motorcade, and was his microphone stuck open? Thomas next discusses these two questions, including several TV programs that disagreed with him. McLain had initially testified (to the HSCA in December 1978) that the answers to both questions were affirmative. But shortly after he returned home, he listened to the tapes, and changed his mind, saying that he had stopped on Houston St. and had seen Clint Hill running to the presidential limousine through an arcade window from Houston St. (For McLain's own account, see No More Silence 1998, Larry Sneed, pp. 162-168.) If that had been true, then McLain's location was totally inconsistent with the HSCA's (and with Thomas's) placement of him, and the acoustic case was dead. Thomas then recounts in detail the evidence of various Dealey Plaza movies, some of which have been used to deny the acoustic case for conspiracy. Thomas concludes that, although images of McLain cannot document his presence at the designated site near the corner of Houston and Elm, there is a reason for this: the visual evidence is simply inadequate to the task. Furthermore, he argues that McLain cannot be shown to be in the wrong places either. So, Thomas claims that McLain must either be located appropriately (for the acoustic case) or else he is far back in the motorcade. In my opinion, however, statements of other patrolmen do not place him that far back. It seems likely to me that McLain's recollection should be trusted, and that he was not at the appropriate site.

Sensimetrics (p. 685)

Among his other achievements, Robert Berkovitz (misspelled as Berkowitz) had identified Osama bin Laden's voice, so he was hired by Court TV to assess the acoustic evidence. For this purpose, he wrote a computer program (Impulses) to do yet one more comparison of the Dictabelt impulses versus the virtual patterns calculated by WA. Berkovitz found only five coincidences, which was not a significant number, so he stated that the acoustic case for conspiracy was dead. But, according to Thomas, he had blundered. WA had found their match by slowing the police recording by 4.3%.

Amazingly enough, Berkovitz had used the wrong tape speed—instead of slowing it down, he had speeded it up! Thomas obtained the software program (by Berkovitz) and was able to locate the impulses that WA had previously shown, merely by using the correct tape speed. Michael O'Dell confirmed Thomas's results. Thomas therefore advised the TV producer of this blunder. According to Thomas, the show has not been aired since.

Chapter 19: Reconstruction (p. 691)

In this JFK case, any reconstruction (of the shooting sequence) is perilous—the evidence is simply too inconclusive to marshal a final, reliable sequence. Thomas, unfortunately, falls prey to this unrelenting seduction. For example, to assist his case he imputes motive (e.g., Oswald did not plan to see his family soon again), when no motive can be known. LHO supposedly carried the carbine in a brown paper wrapper, but Wesley Frazier and his sister, Linnie Mae, both saw him carry the paper-wrapped package under his arm (which was impossible for the M-C, even when disassembled). After Bonnie Ray Williams left the sixth floor at 12:25 PM (according to Thomas), a man in a light-colored shirt with the M-C slipped into the sniper's nest. He moved boxes out of the way in order to gain access. [DM: This sequence can only be speculation.] Thomas recounts the crosstalk events and the apparent synchrony between the audio and visual markers, emphasizing that Z-313 is an anchor point. He admits that, over about a ten-second interval, a five Z-frame discrepancy might occur. Thomas's Table 19 lists six suspect shots and their corresponding Z-frames (see my Table 1 below from Dale Myers). Many witnesses recalled the first sound as different from the subsequent ones—it sounded like either a firecracker or a vehicle backfire. On the oscilloscope, the Dictabelt first "shot" does not look like gunfire either, so Thomas suggests that it was a backfire and that, furthermore, this unexpected sound may have triggered the actual gunshots by mistake (i.e., perhaps earlier than planned). Thomas does not explain why a backfire would not produce echoes, but such an explanation would have been interesting. Leaving aside his supposed backfire, he claims that the first shot struck the pavement, yielding shrapnel that struck JFK.

Regarding the six suspect shots listed in Thomas's Table 19, even the HSCA could not exclude shots from other sites—after all, <u>only</u> the TSBD and the GK were tested. That point often gets lost in the discussion.

Here is the shooting sequence, as Thomas sees it.

<u>Shot #1</u>. The first shot struck at Z-175, about 1½ seconds after a "backfire" was heard. He concludes that JFK's reaction to this first "shot" is visible at Z-194, about a second later. He also suggests that JBC reacted to hearing this shot, although it did not strike him. [DM: This sequence seems plausible.] He states that this shot missed; instead it struck the pavement just behind the limousine (based on the reports of Virginia Baker and Royce Skelton). Then a small bullet fragment ricocheted and struck the back of JFK's head. [DM: This may well be the tiny fragment on the lateral X-ray (see Figure 8—note the oblique cyan arrow). It is the same tiny fragment seen <u>inside</u> the 6.5 mm fake on the AP X-ray. The 6.5 mm fake was likely superimposed over this shrapnel in

the darkroom. A similar fragment, perhaps from this same shot, likely caused the superficial back wound.]

Shot #2. The second shot occurred 1.65 seconds later (Z-204), according to the Dictabelt. The HSCA cited the TSBD as the origin, even though LHO would have needed at least 2.25 seconds.) I agree with Thomas that the HSCA did not behave rationally in assigning this shot (which probably missed) to LHO. Most likely, either this shot or the first one led to Tague's injury. Thomas implies that the evidence for this second shot is chiefly acoustic; the visual evidence is less certain. He agrees that this shot missed the limousine. Although the HSCA claimed that this shot came from the TSBD, Thomas concludes that neither the TSBD nor the GK are likely sites of origin. He wonders if the roof of the Dallas County Courts and Records building might be the correct site. (A 30.06 casing was discovered there in 1975.)

Shot #3. The third shot occurred 1.1 sec after the second and matched to the TSBD. It synchronizes with Z-224; this is Thomas's SBT. The HSCA did not accept this shot—because it followed too closely after #2, i.e., LHO could not have fired in so short an interval. [DM: Either JFK or JBC may have been hit (or possibly both were struck by different shots), but the SBT simply cannot be true]. Of course, the HSCA did not want to invoke a third gunman at this point; that was their hidden reason for rejecting this shot.

Shot #4. The fourth shot (at Z-312) arrived 4.8 sec after the third, and was successful; this came from the GK. [DM: In my scenario—discussed in detail in my e-book, JFK's Head Wounds—the GK shot entered just superior and anterior to the right ear, then exited to inflict the large posterior defect seen by virtually all the witnesses. There is one more powerful argument for the GK shot at this time: the blur in the Z-film at this moment is totally inconsistent with the TSBD, but it could be consistent with a GK shot. I strongly agree with Thomas about this. (Alvarez totally blew this one.) Thomas accepts only one frontal shot, but another bullet must have entered at JFK's hairline, above the right orbit (more likely from the overpass storm drain). See my e-book for details.]

Shot #5. The fifth shot (Z-326) came 0.7 sec after the fourth. This one struck the turf near the manhole cover. Patrolman J. W. Foster saw the turf fly up and he even reported this, according to Thomas. Sheriff Roger Craig recalled:

One .45 mm slug was found on the south side of Elm Street, outside on the grass. It was lying amongst…part of the hair, and blood, and bone matter.

Thomas postulates that CE-399 was recovered from this site and began its long journey from here. I agree that some shot likely hit the turf here (see Horne 2009, Vol. 4, pp. 1106-1107), although it may not have been the fifth one. I doubt that it was CE-399, although I smiled at Thomas's speculation about this. Martin Hay (at the Kennedys and King website) has focused on this issue in his review of Thomas's book. He concludes that the chain of custody for CE-399 is highly problematic and that this bullet was not found at Parkland or in Dealey Plaza. The interested reader should also see "The Mystery of the 7:30 Bullet," by John Hunt:

https://www.maryferrell.org/showDoc.html?docId=10213.

Hunt argues that two bullets appear in the evidence trail, not just one. If these writers are correct, it is unlikely that Thomas's scenario can survive unscathed, but it is clearly a novel perspective. [DM: I have personally viewed CE-399 at NARA; see my prior essays about this. I concur with the observations of Hunt and Hay.]

My summary. Thomas identifies five specific shots. Unfortunately, such a reconstruction is inherently perilous. He proposes a new role for CE-399 (the magic bullet), but Hay and Hunt do not agree with him, nor do I.

Thomas's Epilogue (p. 727)

Thomas's epilogue is essential reading. Here Thomas summarizes his overall view of the case. He comments on social constructivism and politics, noting that all the Democrats on the HSCA committee voted for conspiracy (except the most conservative one), whereas all the Republicans (save for the most liberal) voted against. I wonder what this means: Are Democrats more open-minded—or does it rather mean that they particularly objected to the shooting of a Democratic president?

I liked Thomas's final comments very much: "Based on the present study one might not be as certain that scientists can be counted on to resist political correctness." I have already pointed out (Fetzer 2000, p. 290) that this same problem occurred in Nazi Germany—its scientists, engineers, and physicians were often co-opted by that regime. In other words, there is nothing new under the sun (Ecclesiastes 1:9).

That concludes my explicit review of Hear No Evil. What follows next are critiques of Thomas by others, which are not fully addressed in his book, but which are nonetheless fundamental to understanding the acoustic evidence.

Critiques of Thomas by McLain, O'Dell, Myers, Bugliosi, and Linsker

Five critiques are addressed here: (1) the recollections of H.B. McLain (these have been introduced above), (2) a summary by Dale K. Myers, (3) an article by Michael O'Dell, (4) endnotes from Bugliosi's door-stopper book, and (5) a 2005 article by R. Linsker and R.L. Garwin. The citations for these, in the same order, appear in Appendix 4.

Myers (including some support from McLain)

Myers offers a first-rate history of the acoustical events (Reference 5, Appendix 4), including many incidents omitted from this review.

On Sunday, August 20, 1978, the HSCA team (including four HSCA staff members, seven acoustic experts, a photographic consultant, and 37 Dallas police officers) assembled in Dealey Plaza, along with about 200 spectators. To record the test shots, BBN placed their microphones at eighteen-foot intervals in Dealey Plaza. They used a 14-track recorder; as a result, they were limited to only twelve microphones at once (two tracks were reserved for communication). These microphones were deployed in three separate groups—for a total of 36 microphone positions. If the shots were real, BBN

expected to predict the position of the suspect motorcycle to within 18 feet (8HSCA80). Over a five-hour period, they fired fifty-seven test shots into sand bags.

Richard Sprague subsequently examined many films and photographs, including those of James Altgens, Robert Hughes, John Martin, Orville Nix, Malcolm Couch, Dave Wiegman, Mark Bell, and Marie Muchmore. Sprague discovered that, by themselves, three films proved that no motorcycle was at the HSCA's required site. The Hughes film showed that no motorcycle was closer than 220-250 feet from the limousine during the first shot. The Couch and Wiegman films proved that no motorcycle was within 120-140 feet during the final shot.

In July 1990, Sim Heninger wrote (in The Third Decade) that the Hughes film proves that McLain was barely past the Houston and Main intersection when the (real) shots began.

Dr. James Barger appeared before the HSCA to show his support for Weiss and Aschkenasy. Asked about the lack of corroborating images from Dealey Plaza, Barger acknowledged that the "major shortcoming" of their work "…was that there was no evidence that there was a motor vehicle where we had found it to be." Barger later conceded that the lack of photographic verification was "a very obvious place from which to attack the analysis" (5HSCA685-686).

At least three of the twelve HSCA Congressmen–representatives Harold S. Sawyer (R-MI), Robert E. Edgar (D-PA), and Samuel L. Devine (R-OH)–vigorously dissented from the HSCA's four shot conclusion (HSCA Report, pp. 491-511). Sawyer told the press, "I am not myself persuaded by the committee's acoustical findings.... That the motorcycle was there does not appear–from my reading of the evidence in the committee's report–to have been certainly established" (Tom Wicker, "Assassinations report has two important sidelights," Detroit Free Press, July 23, 1979). "We found no evidence to suggest a conspiracy. We found no gunmen or evidence of a gunman… And we found little, if any, evidence of partnership with Lee Harvey Oswald ("Judgment rushed on JFK 'plot'?" The Flint Journal, March 24, 1979).

In 1979, James C. Bowles wrote a 214-page report. He attacked the HSCA's contention that a voice-grade recording system could transmit and record <u>non-audible</u> N-waves created by gunshots. He emphasized the absence of motorcade sounds and offered strong evidence that the suspect motorcycle was at or near the Trade Mart—and <u>not</u> in the motorcade.

On March 11, 1980, Sprague met with two Justice Department attorneys and four members of the FBI's Technical Services Division. Sprague presented the photographs and films of James Altgens, Dave Wiegman, and Malcolm Couch. These demonstrated that McLain was "…more than 250 feet behind JFK when the shots were fired." By the end of the meeting, the FBI and Justice Department officials were "convinced" that McLain was nowhere near the required HSCA location (Richard E. Sprague letter to Norman Ramsey, April 7, 1982, pp. 1, 3, Sprague Collection, National Archives).

In an e-mail (2001), Barger wrote that he had never personally seen any photographs relating to the acoustics evidence, adding, "I limited my work for HSCA to analysis of the police radio channel recordings."

When McLain appeared before the HSCA, they treated him just like its other witnesses, i.e., they had a pre-determined conclusion and their questions were so aimed. When McLain returned to Dallas, and especially after he had listened to the tapes, he complained of his treatment by the HSCA. He denied recognizing the conversation on Ch-1 (the stuck one), but he did recall the events on Ch-2 (supposedly not his channel). He noted the absence of siren sounds on Ch-1 (where he expected them) and thought the motorcycle on the tape was running too fast to be his. He pointed out the absence of crowd noise, which he had expected to hear. (McLain asserted, "Man, there's no way that could have been my mike stuck open!") Bowles had noted that crowd noise was readily audible during Curry's transmissions in the downtown area (see the transcript for Ch-1 at 11:55 AM).

Savage quotes Bowles as saying that the Dictabelt had been played back many, many times. Bowles postulated that lowering the needle onto the belt had created small dimples that were later picked up by the sensitive HSCA equipment and interpreted as gunshots. But the Sonalysts later concluded, "…that BRSW and W&A did not mistakenly include impulses due to surface defects in their identification."

Savage claims that motorcycle speeds on Main St had been about 5 mph (or no more than 6 to 8 according to Bowles), whereas the sounds on the Dictabelt suggest a motorcycle speed of 30 mph. McLain, and others, estimate their speed on Houston as only 2 to 3 mph, if not actually stopped. McLain recalls stopping when he heard a gunshot (he only heard one); almost simultaneously he saw the pigeons flushed from the TSBD. While still on Houston St., he looked through a window in the ornamental wall (to his left) and saw Clint Hill jump onto the presidential limousine. At that time, he was still about 100 ft from the Houston-Elm intersection (where the HSCA had placed him). This stop, by itself, contradicts the uniform motion posited for him by the HSCA; that uniform motion (required for Thomas's scenario) is shown in Thomas's sketch (his Figure 16.5).

McLain recalls turning his siren on immediately after the shooting, a siren that is not heard on the Dictabelt. In fact, no siren sounds are heard on the Dictabelt until two minutes after the suspect shots. Furthermore, McLain insists that he left promptly and did not linger in Dealey Plaza—whereas the HSCA (and Thomas, too), based on the relative quiet of the motorcycle, stated that he had delayed accelerating for about 30 seconds. McLain recalls that his departure from Dealey Plaza was triggered (promptly) by hearing the order from Curry ("Go to the hospital" on Ch-2). But since McLain was supposedly tuned to Ch-1, he should not have heard this order on Ch-2. Nor could he have overheard this order from a nearby motorcycle—according to Dale Myers, none were close enough. Furthermore, this announcement, if that loud, should have crossed over to Ch-1, but it is not there.

Many officers who had heard the tape said that the motorcycle was clearly a three-wheeler, whereas McLain had ridden a two-wheeler. The HSCA did not try to discriminate between these sounds.

McLain arrived quite promptly at Parkland Hospital, early enough to help Jackie exit the limousine while JFK was being moved. On the other hand, if McLain had delayed in Dealey Plaza for 30 seconds (as the acoustic evidence strongly implies), such an early arrival at Parkland would have been well-nigh impossible—he would have had to exceed 100 mph to catch the motorcade. According to Savage, the maximum possible motorcycle speed was 90 mph.

Savage claims that the stuck mike was not in the motorcade and that the (anonymous) guilty officer had confessed about his stuck microphone to other officers. He had said something regrettable (apparently, he thought he had been recorded), which is why he specifically recalled this event. Savage claims that this officer was on the Stemmons Freeway (also recall the above quote from Thomas about a microphone stuck on Ch-1, while on Stemmons) while en route to the Trade Mart, a trip that required about two minutes. After arriving at the Trade Mart, the sound of his motor decreased. While there, his microphone might have picked up the passing motorcade en route to Parkland (which would account for the Doppler effect in the sirens). A replica of the Liberty Bell was located at the Trade Mart and, if someone had incidentally struck it, that sound would have been transmitted by his microphone. On the other hand, Thomas found a possible source for the carillon bell sound. He cites (p. 615) a video tape (with sound track) made in Dealey Plaza by KXAS TV-news at 1 PM on November 22, 1964 (i.e., not 1963). Richard Garwin, however, discovered that the bell sound occurs on both channels, which implied to him that it might just derive from an electronic interference pattern (p. 644). The bell sound has not been used to synchronize the two channels (by anyone). Also, see Steve Barber's negative comments about this bell: Appendix 4, Reference 2. McLain, on the other hand, thinks a loose manhole cover on the street made that sound when disturbed.

Another curious feature on the Dictabelt (Ch-1) is whistling (of an unidentifiable tune), which occurs at least twice (at 12:31:56 and at 12:32:42). It seems unlikely that someone in Dealey Plaza would have whistled, especially after the horror of the shooting. Some DPD officers recalled exactly who liked to whistle. Bugliosi suggests one possibility (see below).

Dale Myers emphasizes that Thomas disagrees with the HSCA on important issues: (1) the HSCA cited four shots, whereas Thomas cites five, (2) some of Thomas's times differ from the HSCA, (3) his first shot is at Z-175 (the HSCA said Z-160), (4) Thomas hints at three gunmen (the HSCA cited two), and (5) Thomas believes that the GK shot struck, whereas the HSCA said it missed. [DM: Thomas is likely correct that the GK shot struck JFK, although the acoustic data are irrelevant for this issue.]

Myers insists that McLain could not have reached the required site near the Houston-Elm intersection (where the suspect shots supposedly began) in the required time for the HSCA scenario without traveling much faster than 11 mph on Houston St. Thomas

estimates McLain's (required) speed as 20-25 mph, but that is chosen merely to meet the constraints of the acoustic data—there is no independent corroboration for this. McLain strongly disagreed with this high-speed scenario. Furthermore, such a high speed would have moved McLain well ahead of his usual relative position in the motorcade—and he had no reason (before the shooting) to accelerate in such an unfathomable fashion. Furthermore, on the tape the motorcycle sounds diminish three seconds before the suspect shots—which makes no sense either—and it wildly disagrees with Thomas's required speed of 20-25 mph. Moreover, McLain recalled stopping after hearing a shot (he heard only one). On the Dictabelt, the motorcycle sounds stay at this reduced level for about 30 seconds, which totally contradicts the recollections of McLain, who claims that he promptly left the plaza. That scenario is also inconsistent with Thomas's scenario, which requires McLain to accelerate (a lot—and for no reason) before the shooting began.

Myers argues that, in the two minutes before the shooting, some hint of acceleration or deceleration should have appeared while McLain was still on Main St, or especially while rounding the corner to Houston. McLain probably did accelerate at the corner in order to stay in his designated slot—he was, after all, on the outside of the turn, so he had farther to travel. Furthermore, the motorcade speed on Main was quite slow (McLain and other officers estimated this at 2-3 mph), so that McLain's subsequent acceleration on Houston (as Thomas requires of him) should have been (acoustically) obvious on the tape. But there is no sign of this. To Myers this means that the stuck microphone was not in Dealey Plaza. Myers offers a useful summary of possible shots, as shown here.

HSCA	Thomas	Source of Shot
	Z-147	"noise"
Z-160		TSBD
	Z-175	TSBD
Z-189		TSBD
	Z-204	rogue shot
	Z-224	TSBD
Z-302		GK
Z-312		TSBD
	Z-312	GK
	Z-326	TSBD

Table 1. "Shots," as summaried by Myers. Notice the disagreement between the two left columns. Thomas's SBT begins at Z-224.

According to Thomas, the "noise" (at Z-147) remains unidentified, although he suspects a backfire. Per Thomas, JBC may have responded to it, or else his movements were coincidental. This sound did not have the usual characteristics of a gunshot (e.g., no

typical echoes). The rogue shot, according to Thomas, came from the rear, but its exact origin, and even its destination, are not known—which is where the third gunman might sneak into Thomas's scenario. Thomas cites Z-224 as his key shot: it establishes the critical gunshot interval (along with Z-313) that ties together the audio and visual evidence for him, and it also defines the moment of the SBT. Unfortunately for Thomas, even the HSCA does not recognize this moment (Z-224) as a shot. In other words, the HSCA does not support two of Thomas's chief claims for Z-224: the moment of the SBT and first part of the critical gunshot interval.

Myers radically disagrees with Thomas about which Z-frame matches the end of the Hughes film—that is the one that shows McLain rounding the corner at Main and Houston. Myers claims that it was taken at about Z-135, just 2.19 seconds before the first suspect shot. Thomas, on the other hand, claims it was 3.8 to 5.9 seconds before the first suspect shot. In Thomas's scenario, McLain needs a long time to reach the designated site near Houston and Elm St, and also needs a speed of over 20 mph during this interval. (Thomas does address these issues in some detail (pp. 673-684).) As further support for his case, Myers adds that the film record shows that Camera Cars 1, 2, and 3 all stopped in front of the TSBD at about six seconds after the final head shot— and they remained stationary for about 15 seconds.

My Summary. McLain was not located where the HSCA (or Thomas) placed him. The suspect shots on the Dictabelt do not match to McLain. Oddly enough, the HSCA seriously disagreed with Thomas on multiple issues.

Michael O'Dell

Michael O'Dell's presentation (see my Appendix 4, Reference 6) is a model of clarity. He obtained copies of the raw data, and has made an enormous effort to analyze this, using his own technical instruments. Furthermore, he has had many exchanges with the NRC authors, and also with Thomas. Beginners might well start with his article. He also provides a link to the National Academy of Sciences website, where these digital CDs can be heard:

The National Academies Press, Audio files of JFK Assassination Recordings at:

https://www.nap.edu/resource/JFK_audio/

O'Dell reminds us that both police recording devices (for Ch-1 and Ch-2) engraved a track on a plastic medium. The Dictabelt (Ch-1) used a rotating cylinder while the Audograph (Ch-2) used a flat disc, like a phonograph record. O'Dell emphasizes that Thomas's case is based on a continuous recording on Ch-2 (during the critical interval); without this, Thomas runs into serious problems with asynchrony.

To support his fundamental assumption, Thomas had relied on the initial NRC data, which was based on the Bowles copy. But O'Dell found a problem with this: he notes that the needle assembly on the Audograph (Ch-2) does not move. Instead the disc (mounted on a spindle) moves horizontally under the fixed needle. Because this needle is somewhat flexible, repeats (overdubs) can occur for one or even two rotations. But the main point is this: because the horizontal movement (of the disc) never stops, the

fixed needle must eventually catch up to where it should be—i.e., at the end, the time lost (or gained) is either nil or very small. The NRC, however, had not considered the forward skips. Therefore, when the NRC corrected for the overdubs, they got the wrong answer. In fact, it would have been better if they had not corrected at all—since the correction was nearly zero (or actually zero).

O'Dell claims that the FBI copy of Ch-2 (made in 1981) is much better—it has no skips or repeats (overdubs). Therefore, the Bowles copy (used in the original NRC report, but not used in their follow-up report) should not be relied upon. The Bowles copy, unfortunately, is the one that Thomas had used. Instead, the FBI copy should be used for timing issues. That is the one that Linsker et al. used in their follow-up report. There is only one problem with this though: this recording could not be played back on the Gray Audograph. The reason is that the device kept producing skips and repeats on playback. (Any recording, for accuracy, ideally should be played back on the same machine that recorded it.) Therefore, a standard phonograph table was used instead, but this created new problems. Phonograph tables operate at a constant rpm, but the Audograph did not. Instead the Audograph rpm decreased as the recording progressed. Therefore, when the Audograph disc is played back on a standard phonograph table, the audible frequencies and apparent speeds are different. So a method had to be devised for correcting this. The NRC used the 60 Hz hum in the background, which had been introduced during the original recording. O'Dell displays a remarkably linear graph of frequency vs. time, which shows that this correction can be done very accurately. Ch-1, on the other hand, does not have this problem, nor does it have skips or repeats; it is only important to get the right playback speed for Ch-1 (which can be done). So the bottom line is this: The original NRC timeline was wrong (but this is what Thomas used in his original article). It needed to be corrected to obtain the correct synchronization between channels.

O'Dell displays (in his Table 1, copied just below as my Figure 17) the corrected time intervals. Thomas's corresponding intervals are shown in my Figure 18.

HOLD to CHECK (labeled ALL by Thomas)	130.68 sec
CHECK to YOU	12.23 sec
YOU to ATTENTION	90.84 sec

Figure 17. O'Dell's intervals for Ch-2

Bewilderingly, O'Dell and Thomas sometimes use different labels for the same event, which can be very confusing. For example, "I'll CHECK ALL these motorcycle radios" is called CHECK by O'Dell but is labeled ALL by Thomas. Adding to the confusion, Thomas labels Fisher's supposed phrase (just seconds before the suspect shots— "I'll check it") as CHECK. In this immediate discussion, since it is based on O'Dell's work, I shall stick with O'Dell's labels. We next compare the above intervals to those listed by Thomas (in his Table 14, p. 640). However, another problem emerges here: Thomas appears to have reversed the two very close utterances by Bellah: YOU and ALL (the latter is labeled CHECK by O'Dell). This mistaken reversal is confirmed not only by

O'Dell, but also by the Bowles transcripts (in Savage's book, p. 406). After correcting for this, we can present Thomas's intervals for Ch-2 (using O'Dell's notation for consistency); in other words, CHECK here represents, "I'll CHECK ALL these motorcycle radios."

HOLD to CHECK	143 sec
CHECK to YOU	12 sec
YOU to ATTENTION	90 sec

Figure 18. Thomas's intervals for Ch-2

The reader will immediately see that O'Dell (130.68 sec) and Thomas (143 sec) disagree about the 1st interval, whereas they nearly agree on the 2nd and 3rd. Also notice that Thomas claims (in the legend for his Table 14) to have used "corrected" playback intervals; he also cites "Data from O'Dell (2003)." I could not find a date on O'Dell's essay, but Thomas (p. 638) provides strong evidence that he and I have cited the same essay by O'Dell—from 2003. So they should agree on this 1st interval; their disagreement is inexplicable. To my knowledge, neither O'Dell nor Thomas has addressed this disagreement (of 143 – 130.68 = 12.32 seconds).

Events on DPD Channel II

AUDIBLE EVENT	*TIME ELAPSED (seconds)*	*RATIO (using Eq.1)*	*CORRECTED (seconds)*
Dispatcher "12:23"	*579.68*	*0.71972*	*417.21*
Dispatcher "12:26"	*652.42*	*0.73450*	*479.20*
Dispatcher "12:28"	*686.13*	*0.74135*	*508.66*
Dispatcher "12:28"	*720.56*	*0.74835*	*539.23*
Dispatcher "12:30"	*771.39*	*0.75868*	*585.24*
"GOING to the hospital"	*793.10*	*0.76309*	*605.21*
"HOLD everything secure"	*865.06*	*0.77772*	*672.77*
Bell sound	*873.25*	*0.77938*	*680.59*
Dispatcher "12:31"	*873.60*	*0.77945*	*680.93*
Dispatcher "12:32"	*899.94*	*0.78480*	*706.28*
Dispatcher "12:34"	*991.68*	*0.80345*	*796.76*

"CHECK all these"	*998.32*	*0.80480*	*803.45*
"YOU want me"	*1,010.43*	*0.80726*	*815.68*
Dispatcher "12:35"	*1,047.88*	*0.81487*	*853.89*
Dispatcher "12:35"	*1,078.18*	*0.82103*	*885.22*
"ATTENTION"	*1,098.58*	*0.82517*	*906.52*
Dispatcher "12:36"	*1,106.42*	*0.82677*	*914.75*

Table 2. Selected events on DPD Channel 2, per O'Dell.

The NRC panel had shown that Decker's phrase, "…HOLD everything secure…" appeared as crosstalk on Ch-1 at the same time as the suspect 3rd and 4th shots. (The 1st suspect shot occurred 6.5 seconds before that.) O'Dell next discusses how that conclusion was reached. His Table 2 (copied here) provides the supporting data for his conclusion.

HOLD synchronization *(Times are relative to "HOLD")*				
Ch 2 Event	**Time**		**Ch 1 Event**	**Time**
Dispatcher "12:30"	-87.5			
"GOING to the..."	-67.5		First "shot"	-6.50
"HOLD everything…."	0		"HOLD everything…."	0
BELL	7.8		BELL	7.8
"CHECK all these…"	130.6		"CHECK all these…"	158.6
"YOU want me…"	142.9		"YOU want me…"	173.5
"ATTENTION…"	233.7		"ATTENTION…"	287.6

Table 3. "HOLD" Synchronization with Corrected Relative Times (per O'Dell)

We next turn to my Table 3 here. Notice that, in the left "Time" column (i.e., for Ch-2) "GOING to the hospital" occurred 67.5 seconds before HOLD. [DM: I use GOING here because O'Dell uses it, but later in this review I use GO for this same utterance—because Thomas and his adversaries use GO.] On Ch-1, the 1st suspect shot occurred 6.5 seconds before HOLD. If HOLD is a valid crosstalk, we should then conclude that "GOING" occurred at least 61.5 seconds (67.5 – 6.5 = 61.5) before the 1st suspect shot. That makes no sense, since the "GOING" utterance presumably prescribes desirable action after the shooting (probably after all the shots had been fired). The case is closed therefore—the suspect shots cannot represent the actual shooting. O'Dell provides a useful timeline (my Figure 11) for the four possible cases of crosstalk, including the final deliberate simulcast, in his Figure 2. (In his book Thomas had added a fifth candidate, Fisher's CHECK, but that suggestion was too late to appear in O'Dell's article.)

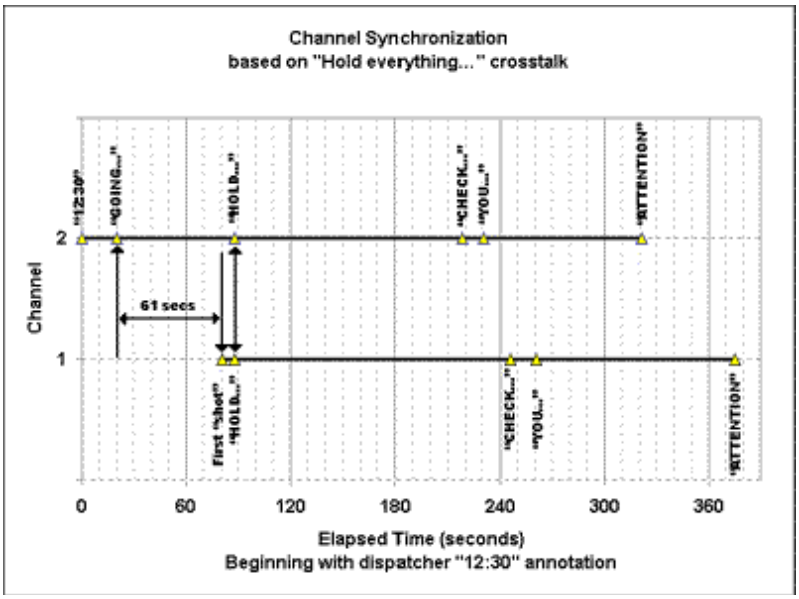


Fig. 11. Synchronization timeline using the Decker crosstalk (per O'Dell). The real shots would have occurred before "GOING." They are not shown here, and they were not heard on either channel.

How much time, if any, was missing on Ch-2? Thomas argues for almost none.

If he is wrong about this though, then his argument for synchrony is also wrong, and his entire case collapses. His argument for no missing time is based on a graph (his Figure 16.3) of dispatcher's announced time vs. playback time. Thomas argued that this was a straight line and that the slope was nearly 1.0. Even if this were true, though, the margin of error (±0.05) for this slope is large enough that some missing time might exist. According to O'Dell, the slope is not 1.0, but closer to 0.918 (or 0.94 according to Linsker). Therefore, Thomas was most likely wrong; time was almost certainly missing on Ch-2—and that seriously undermines his critical assumption. In fact, a slope of 0.918 would correspond to about 30 missing seconds during that total time interval (of about six minutes)—and that is significant.

As further support for this conclusion, O'Dell emphasizes that Ch-1 is about 28 seconds longer than Ch-2—between two specific crosstalk samples: "…Hold everything secure…" and "I'll check all these motorcycle radios." That implies to him that Ch-2 stopped for 28 seconds. Between "I'll check all these motorcycle radios" and "You want me" we see another difference (of 2.4 seconds); between "You want me" and "Attention" is another difference (24.8 seconds). (From Table 14 by Thomas, p. 640, we see similar differences: 31, 3, and 24 seconds, respectively.) In each case, less time is apparent on Ch-2 (than on Ch-1), which is consistent with the conclusion that Ch-2 had stopped. But, if we accept Thomas's thesis that Ch-2 did not stop, and the crosstalk misalignments had been caused by needle skipping (or something with a similar effect), then just one skip could not explain this. Instead, at least one skip must occur between each example of crosstalk (because these differences appear for each interval shown above). Such persistent and regular skipping seems unlikely; on the other hand,

(Thomas won't like this), it is more likely that Ch-2 did stop, as it was supposed to do (after four seconds of silence).

[This is a parenthetical interlude, supplied by Paul Hoch. From a Barger letter (March 22, 1982—http://jfk-records.com/) to the Ramsey panel (mostly paraphrased):

We don't believe that Bowles was correct about the 4-second delay before stoppage. There is no predominance of 4-second silences on either channel, which is what Bowles would predict. Barger and colleagues therefore conclude that the run-out time on Ch-1 is about 7.5 seconds; on Ch-2 it is about 6.5 seconds, or more. "Since there are no silences longer than 5.5 seconds in the Ch-2 Audograph record between 'silence A' and 'YOU want me…Stemmons,' we conclude that there are no recorder pauses in this interval."

The foregoing website also includes a clarifying letter (February 18, 1983) from BBN to Blakey, which includes several proposals for further study. Paul Hoch informs me that he has seen no follow-up on this, even though the cost in 1983 dollars (per Hoch's estimates) would have been only about $4800.

Of course, in view of the many arguments here against the acoustic data, such experiments would almost certainly not change our view of the utility of the basic data.]

But then Thomas proposes another option: he now wants to synchronize on "You want me," instead of "…Hold everything secure…" He thinks that these two utterances were distinct, and that the former would more reasonably place the suspect shots before Curry's "Going to the hospital" (GOING). Thomas claims that this scenario can be justified in one of two ways: (1) "…Hold everything secure…" was misplaced on Ch-1 or (2) Ch-2 stopped between these two utterances. (This latter option seems paradoxical for him, since previously he wanted no time lost on Ch-2). But here is O'Dell's response to Thomas's new proposal: on Ch-1, there are 181 seconds between the first suspect shot and "You want me," while on Ch-2 there are 210 seconds between "Going to the hospital" and "You want me." Therefore, even Thomas's new synchronization places GOING at 29 seconds (210 - 181 = 29) before the first suspect shot, which is nonsense, i.e., why would the police escort go to the hospital before the shooting? Linsker (see below) makes a similar case.

Finally, O'Dell makes a clever suggestion. Using an example of speech, he shows that simple enunciation of the word, "secure," with its prominent K sound, produces an impulse pattern quite like a gunshot sound. He also claims that the consonants D, T, S likewise can do this.

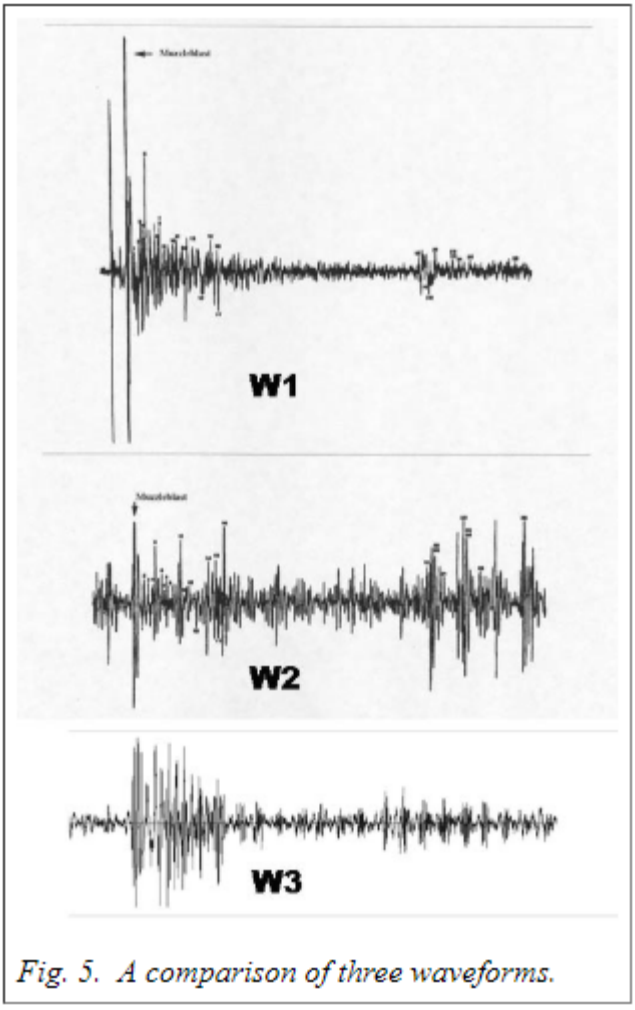


Figure 19. W1 is a test pattern. W2 is from the Dictabelt, and was considered a match to W1. W3 represents O'Dell speaking the letter "K." The image is from O'Dell's article.

This is relevant because it is likely that the suspect shots are superimposed over speech. But there is a counterargument here: the initial sound (of each suspect gunshot) is highly compressed, due to the AGC (automatic gain control). When these "shot" sounds are expanded in time, it is still very difficult to display all the information in them. But that is not the case for human speech, where the information can usually be displayed. If this argument carries the day (O'Dell does not address this counterargument), then we can conclude this: the suspect gunshot sounds are not solely human speech, but rather must contain something else as well. Of course, even if the sounds contain "something else" that does not necessarily mean gunshots. But something rather loud, likely louder than human speech, must have been included in these "shots."

O'Dell has done far more than criticize the work of others. In fact, he created his own acoustic model of Dealey Plaza (relying on the data from WA) and has simulated shots from other sites, such as Zapruder's pedestal and the storm drain on the overpass, using ± 1 msec windows and 14 correlated peaks.

This is from a semiprivate communication: Michael O'Dell to JFK Group, May 10, 2013. Subject: RE: Dictabelt/Zapruder synch with head shots at 144.9 AND 145.15.

Over the entire survey map of Dealey Plaza, he searched for—and successfully found—motorcycle positions that yielded at least 14 (echo peak) matches. Moreover, it was easy to achieve matches from these other sites. For example, with the rifle on the GK, he reproduced the HSCA result, but also found that the GK was not unique. For the rifle on Zapruder's pedestal, he easily obtained motorcycle positions that matched. With the rifle on the storm drain (on the overpass), he even found a match. He adds that, with lower thresholds, or wider (coincidence) windows, matches are even more likely. At this point, it is useful to ponder Thomas's comment:

A gunshot from almost anywhere in Dealey Plaza was bound to be similar in pattern to gunshots from many if not most places in the Plaza.

My summary. O'Dell provides a lucid exposition of events. He clarified the issue of skips and repeats on Ch-2 (which essentially cancelled one another). He concludes that the suspect shots come too late and he suggests that the consonants K, D, T, and S contributed to the suspect sounds. O'Dell's acoustic model of Dealey Plaza lands a knockout punch to the GK gunman. If matches are easily found for gunman sites other than the GK, then the HSCA case for a GK gunman cannot be unique; more likely, it is meaningless. Bugliosi (or possibly a ghostwriter—as David Lifton has suggested)

Michael O'Dell advises me via e-mail (February 4, 2019) that Bugliosi told him directly that Dale Myers had written the first four chapters of Bugliosi's book, and that Dale was mostly responsible for the acoustics section. After O'Dell had cited an error, Bugliosi promptly offered an excuse, i.e., it was not his fault, and he then pointed his finger at Myers as the culprit for the error. I would also suggest that Steve Barber is a likely source for additional discussion of the acoustics (possibly the material in the CD attached to Bugliosi's book).

Bugliosi/Myers/Barber provide a useful summary of the case, including several items not easily found elsewhere. They note that Barger emphasized that the human ear could not identify the suspect shots on the Dictabelt. They then discuss the 15 matches and six false alarms. I would emphasize that false matches are indeed possible—six of them in this case—even though they meant nothing. BBN (Bolt, Baranek & Newman) had expected 13 such false alarms, but only six were found. In view of this, BBN stated, "It is not unreasonable to expect that there are seven more [false alarms] …" They therefore concluded that, of the remaining nine correlations, each was "…equally likely to represent a detection or a false alarm" (8HSCA 95,106).

Barger testified that, at the time of the first suspect shot, McLain was about 120 feet behind the presidential limousine; i.e., he was still on Houston St., about ten feet from Elm (2 HSCA 68, 101). But no photograph has been found to document this. Based on Barger's findings, the HSCA said that the first shot was fired at Z-168. WA (Weiss and Aschkenasy), however, instead chose Z-151 to Z-161. They disagreed about the second shot, too: Z-197 (BBN) vs. Z-188 to Z-191 (WA). They even disagreed about the GK shot: Z-304 (BBN) vs. Z-295 to Z-296 (WA). (See HSCA Record 180-10110-10234,

Draft of the HSCA Report, December 13, 1978, p. 54; HSCA Report, p. 80.) These disagreements are usually greater than five frames. On the other hand, Thomas had touted his good agreement (five frames) as an excellent match between the audio and visual data. However, that the two acoustical teams consistently disagreed by more than five frames makes it likely that Thomas just got lucky.

Anthony Pellicano, president of Voice Interpretation & Analysis Ltd. in Chicago, concluded that the stuck microphone was not part of the motorcade and therefore, "the noise impulses detected …were not shots." His conclusions were largely based on the siren sounds, which only appear about two minutes after the suspect shots. On Ch-2, sirens are audible in the background of Curry's words, "Go to the hospital."

The DPD radio had both a receiver and transmitter. The transmitter had a single button, which must be pushed to talk. The receiver had two possible positions: Ch-1 and Ch-2. Bowles claims that the receiver switch never got stuck; only the push-to-talk button did.

Bowles adds that the HSCA never analyzed the motorcycle sounds, i.e., they never tried to determine whether the stuck microphone was on a two-wheeler vs. a three-wheeler. Bowles insisted that the sound difference (between two and three wheels) was dramatic, like comparing a new Lincoln to a Model-T.

Bowles played portions of the test tape (i.e., the HSCA test shots) to Bugliosi. He (presumably meaning Bugliosi) heard voices saying,

"Stand by just a second," then, "Give us ten seconds when you're ready" (followed by another voice saying, "Ten-four"), and then, "Ten seconds." After this there are no voices for ten seconds and then you clearly hear the sound of gunshot, even the echo, on the tape. This sequence was repeated on the tape several times, all followed by a clearly audible shot.

Bugliosi asked Blakey if he could hear the gunshots on the test tapes; his answer was, "Of course we could hear the rifle sounds on our tape."

Bugliosi emphasizes that the HSCA used its own microphones and recording equipment, even though they could have used the original DPD equipment; Bowles said the DPD had the same audio systems in 1978 as they had used in 1963.

Bugliosi names two DPD suspects for the stuck microphone: Willie Price and Leslie Beilharz. He ultimately declines to select one, but he does note that Beilharz admitted that he always whistled, even during 1963. Beilharz also recalled being on the Stemmons Freeway, en route to the Trade Mart (consistent with Thomas's quote above). He even admitted that his microphone may have been stuck for some time, which did not disturb him because he had already received his orders. Most interestingly, Beilharz rode a three-wheeler that day—and (human) whistling was heard (twice) on the Dictabelt.

WA assumed that Barger's placement of the stuck microphone in Dealey Plaza was correct, even though Barger admitted that photographic confirmation was missing.

Bugliosi quotes in some detail (from the HSCA) about where McLain was located with respect to the suspect shots and concludes that McLain would indeed have had to accelerate in order to make up the required distance (to match the microphone site where the suspect shots occurred). Before McLain started north (toward Elm, while on Houston), the Hughes film shows that the presidential limousine had already turned the corner at Houston and Elm. Other Dealey Plaza movies (e.g., Turner, Bell, and Zapruder) show the limousine accelerating after completing that turn. McLain appears to confirm this:

Yes, they [the presidential and vice-presidential limousines] were just turning the corner onto Elm St as I came around the corner off Main St (5 HSCA 641).

To close their case, the HSCA stated:

…at the time of the assassination he [McLain] would have been in the approximate position of the open microphone near the corner of Houston and Elm indicated by the acoustical analysis (HSCA Report, p. 76) …

In fact, McLain had said no such thing.

Based on photos, Robert Groden agreed with the HSCA's location of McLain. Richard E. Sprague (another photo consultant to the HSCA), however, totally contradicted Groden. Sprague concluded that McLain had not approached the Houston-Elm intersection until well after the last shot. He also strongly doubted that McLain could have made up the necessary ground in the time allotted. In effect, according to Sprague, no motorcycle agreed with the HSCA.

Bugliosi does not discuss the bell tower that Thomas cites (Thomas, p. 643). On the contrary, he quotes Pellicano, who identified a church bell, but this one was not within earshot of Dealey Plaza. The HSCA concluded that the bell sound on Ch-1 did not derive from Dealey Plaza (8 HSCA 112). Thomas does not comment on the HSCA's contrary conclusion.

Steve Barber concludes that Thomas's key crosstalk (by Fisher, supposedly just seconds before the suspect shots) is not crosstalk at all:

http://www.jfk-online.com/doubled.html and http://mcadams.posc.mu.edu/acoustic.htm.

Barber agrees that Fisher had said, "I'll check it" (CHECK) on Ch-2. On the other hand, on Ch-1, Barber hears something quite different: "Alright, Chaney." If that is true, Barber argues, then there was no crosstalk at that time and Thomas cannot use the Fisher event as crosstalk—which would destroy Thomas's case. As further support for Barber's conclusion of no Fisher crosstalk, Gary Mack reported that WA had heard this phrase (on Ch-1): "Alright, Jackson."

There is even one more argument against Fisher's words as crosstalk. On Ch-2, the phrase supposedly is, "I'll check it." But that is not the complete phrase. On Ch-2, it was, "That's alright, I'll check it." According to Barber, neither Thomas nor Bowles have accounted for the missing words, which really should be on Ch-1 also. Barber concludes that he heard a distinctly different phrase on Ch-1 than he heard on Ch-2. If

so, Fisher cannot be an example of crosstalk, and that would be devastating for Thomas.

My Summary. Bugliosi/Myers/Barber provide powerful evidence from eyewitnesses that McLain's position did not match the HSCA's (or Thomas's) scenario. They even name a policeman (Beilharz) who often whistled. Thomas's purported crosstalk CHECK is not the same sound on the two channels. If true, then the case is closed, and the acoustic data are useless. For both Bugliosi and the acoustics case: Requiescat in pace.

Linsker, et al. (Appendix 4, References 9 and 11)

The final published critique discussed here (from 2005) is by physicists Linsker, Garwin, et al. (abbreviated here as LG). See Figure 20.

Richard Garwin (2011)

Figure 20. Richard Garwin authored the first hydrogen bomb design. At the age of 21, he studied under Enrico Fermi. One of Fermi's students (Marvin Goldberger) quoted Fermi as saying that "Garwin was the only true genius he had ever met."

This report is a sequel to the original NRC report. (Four of these authors were also on the original NRC panel.) In this update, they confirm the original NRC conclusion: the suspect shots occur about one minute too late to be authentic shots. They also cite the work of Barber and O'Dell; with whom they agree. Their specific methods include cepstral analysis and spectrographic techniques. Although cepstral analysis was originally designed to detect echoes in acoustic signals, it is also useful for analyzing human speech. It is especially helpful for identifying repeats (overdubs). In addition, LG used "pattern cross-correlation" (PCC) to identify crosstalk from two different spectra; these are graphs of power density (raised to the power of 0.3) at each time and frequency. These results can also help determine the relative speed of the two channels.

One of LG's chief conclusions is that on the FBI recording of Ch-2 (Track 7—see Appendix 6 for a list of these Tracks) …there can be no doubt that the hum was recorded along with the original sound, and not during any subsequent copying process.

LG claim that this FBI recording is superior to the one (by Bowles) that was used by the original NRC. (Thomas also used the Bowles recording in his analysis.) If this argument (about the hum) is accepted—and it is a very powerful argument—then the FBI recording faithfully represents the sounds from the original Dictabelt (Ch-1), and suspicions about alteration (or loss) of the original can be put aside. It also means that the original (or a direct copy) has been used in all the acoustic analyses to date. Because the speed correction factor is well known (based on the hum frequency), LG's analysis relies almost exclusively on the FBI copy (Track 7, Ch-2).

LG first attempt to synchronize the two channels using HOLD and YOU. They emphasize that this FBI copy (Ch-2) contains no repeats or skips. However, they acknowledge that Thomas disagrees: on the contrary, he reports two utterances on Bowles's version (Track 2, Ch-2) that are missing from the FBI copy of Ch-2. For Thomas, this implies forward skips on Ch-2. LG disagree—they claim that there are no forward skips on the FBI copy. Furthermore, the cepstral analysis rules out any backward skips on Ch-2 (i.e., repeats or overdubs). LG also tell us something new: the transit time (i.e., one revolution) through one groove (on the Audograph) is 3.6 seconds. Therefore, when a single overdub or skip occurs, the expected overall time changes by 3.6 seconds (either positively or negatively). They also emphasize that double skips or double dubs are rare, so in order to explain 30 seconds (or possibly more) of missing time would require an Olympic-sized skip. Thomas never addresses skips of this magnitude.

If AGC (automatic gain control) had not been used, the recordings could simply be placed side by side and their peaks compared. However, AGC alters the height of the peaks, which means that comparisons become less reliable. But that is where PCC can help. It measures the power in the spectra; the two channels can then be compared, and a judgment can be made about time offsets. The relative speed correction can also be varied in order to locate the strongest peak in the PCC—the best match will presumably identify the proper speed correction. These PCC graphs display the intensity of the sound (raised to the power of 0.3) vs. frequency. To compute the PCC, the power in the two spectrograms (one from each channel) is cross-correlated at each frequency. That correlation will depend on the time shift between channels, but a range of time shifts can be explored. A strong and clear peak in PCC implies that the Ch-1 segment is contained within the Ch-2 segment (at the relative time shift tested); a strong peak is good evidence for crosstalk. These calculations can be repeated for different speed corrections (in Ch-1). The strongest match in PCC identifies the correct speed correction. The position of the peak in time also identifies the time offset between channels. Therefore, PCC determines both the relative timing and the speed of the two channels. The authors apply this technique to three proposed cases of crosstalk: CHECK, HOLD, and YOU (using Thomas's notation).

Cepstral analysis employs a time-varying signal input, with some repeat or added components to the signal (possibly with attenuation), including some time delays; this is reminiscent of the echo patterns in Dealey Plaza. Cepstral analysis is also useful for identifying stylus jumps during playback of the Audograph disc.

The authors use a Gabor spectrogram (presumably named after Dennis, not Zsa Zsa—although both were Hungarian). This approach uses specific filters, so that the resolution of the signal power, in both frequency and time, can be optimized.

They then attempt to synchronize two crosstalk events: "…HOLD everything secure..." (by Decker) and "YOU want me to…" (by Bellah). If these are authentic crosstalk events, then synchrony should be evident. Thomas had suggested (they cite him for this) the presence of forward skips on the FBI recording for Ch-2 (Track 7). LG disagree—they find no evidence of a forward skip. Furthermore, by means of cepstral analysis they also conclude that there are no Skipbacks. On Bowles's version of Ch-2 (Track 2), however, they find two Skipbacks followed by a forward skip. Aside from that, in this time interval, the Bowles version (Track 2) and the FBI copy (Track 7) match just fine on Ch-2 for every utterance, i.e., no skips backward or forward.

LG then focus on Thomas's most critical proposal for crosstalk, abbreviated here as CHECK: "I'll check it…" (spoken by Fisher, according to Thomas). Thomas had located these (supposed) words just seconds before the suspect shots on Ch-1. Because of this proximity in time, he regarded this event as highly reliable for achieving correlation between Ch-1 and Ch-2. (Thomas had not, however, used Fisher as a tie-in during his 2001 article; he only introduced this notion later.) LG first emphasize that if CHECK and CHECK1 represented valid crosstalk's, then that would be incompatible with both HOLD and with YOU (both of which LG accept as valid crosstalk events). Furthermore, this discrepancy cannot be due to lost time on Ch-1, since there was none—the constant motorcycle noise prevented that. In addition, their PCC analysis showed no prominent peak for CHECK, which also argues against this event as a crosstalk. [DM: Thomas, somewhat petulantly, claims (p. 656) that LG deliberately hid this Fisher result, but they did not; it is Figure 9 in Reference 9 in Appendix 4 below.] On the other hand, if there had been two different utterances of CHECK (at two different times) then this peak should indeed be absent, as it was. In the case of YOU and HOLD, LG's analysis strongly confirmed true crosstalk, but the opposite was found for CHECK—i.e., CHECK and CHECK1 are likely two different utterances spoken at two separate times and therefore cannot represent crosstalk. If this conclusion is accepted, Thomas's newly proposed tie-in (via CHECK) is mortally wounded.

LG (p. 222 in their article) next analyze the time interval between GO (Curry's phrase on Ch-2, which is not an example of crosstalk) and the suspect shots (Ch-1), by using the crosstalk HOLD. On the FBI copy of Ch-2 (Track 7) the time interval between GO and HOLD is 67.7 seconds; on Bowles's version of Ch-2 (Track 2) it is 64.3 seconds. Since LG claim no skips or repeats during this interval, they conclude that the actual elapsed time is 67.7 seconds (the FBI copy was deemed more reliable). Next, they state that the time from the first suspect shot to HOLD was 6.6 seconds (from their Table 1). Since Ch-1 had no down time (because the motorcycle kept the recorder going), they

conclude that 6.6 seconds is the actual elapsed time. They therefore conclude that the time from GO (which paradoxically comes before the first "shot") to the first suspect shot was 61.1 seconds (67.7-6.6 = 61.1). We have already seen this argument from O'Dell. Keep this time interval (of 61.1 seconds) in mind as the discussion proceeds.

Another supporting argument for the validity of HOLD (as a valid crosstalk) is the bell sound. This occurs 7.7 seconds after HOLD on the FBI's Ch-2 (Track 7) and 7.8 seconds after HOLD on Bowles's version of Ch-1 (Track 1). That agreement is excellent.

If crosstalk YOU is employed instead, then on Ch-2 the time interval from GO to YOU (on Track 7) is 67.6 + 143.2 = 210.8 seconds. That same interval, on Bowles's version of Ch-2 (Tracks 2 and Track 3) is 63.9 + 148.4 = 212.3 seconds. On the other hand, the Ch-1-time interval from the first suspect shot to YOU (Track 1) should be longer than this—because the starting point (the first suspect shot) should logically occur before the GO utterance (which was a response to the shooting).

However, the apparent time interval (on Bowles's Ch-1, i.e., Track 1) from the first suspect shot to YOU is ironically shorter, i.e., only 6.5 +173.0 = 179.5 seconds. That same interval on the FBI's copy of Ch-1 (Track 5) is 368.7 -187.7 = 181.0 seconds. Combining these results yields the actual elapsed time from GO to the first suspect shot: 210.8 – 179.5 = 31.3 ± 1.5 seconds (plus the Ch-2 dead time during the interval, GO to YOU.) This is of course illogical, i.e., GO (an obvious response to the shooting) cannot occur before the shooting—but that is precisely what the arithmetic requires. The dead time in question consists of five periods of radio silence, supposedly at least four seconds each; this should add at least another 20 seconds, which would increase the total interval (from GO to the suspect shots) by at least 31.3 + 20 = 51.3 seconds (but likely somewhat more). Therefore, the agreement with the time interval calculated above (the number the reader was asked to keep in mind—61.1 seconds) is quite good, i.e., 51.3 (or more) seconds vs. 61.1 seconds. If this conclusion is accepted, all the acoustic probability calculations by the HSCA (and by Thomas, too) become totally irrelevant—the suspect sounds simply occur at the wrong time to represent real shots.

LG emphasize that Thomas primarily employed the NRC report and the Bowles recordings (Track 2) from the Gray Audograph (Ch-2). This latter copy contains many repeats; the NRC considered it less reliable than the FBI copy (Track 7, which LG chiefly used). They next cite Thomas for four errors, as follows:

LG criticism #1. Thomas had accused the NRC of not using the Bellah crosstalk (YOU) for synchronization. The NRC authors contradict this, saying that it had been recognized, and in fact had been discussed in the 20 pages of Appendix C (of the original NRC report). Since Ch-1 had no missing time, its playback time could be converted directly to actual time (after the time correction factor). Furthermore, the NRC had corrected for the time warp.

LG criticism #2. Thomas had stated that, if the Bellah crosstalk had been used for synchronization, then the suspect shots occur precisely between Curry's two broadcasts, which should exactly (and consistently) define the moment of the

assassination. Three mistakes were made here according to LG. First (in his original article), Thomas had relied on the NRC's timeline, which was wrong (as discussed above, the NRC had not realized that the repeats and skips nearly cancelled one another). Second, Thomas used the wrong speed correction. Third, Linsker, et al. had determined a more accurate value for the speed correction.

LG criticism #3. Thomas claimed that no significant time was lost on Ch-2. To reach this result, he had used the slope of the curve as 1.0 ("Stopwatch time vs. Time annotations," in Thomas's Figure 16.4), but this was wrong, according to LG. According to them, that slope was about 0.94, which translates into as much as 30 seconds of lost time on Ch-2. If accepted, that leads to a large, and important, disagreement with Thomas, who necessarily believes that no time was lost. If Thomas is wrong here, his case collapses. A reductio ad absurdum—to close the case

LG criticism #4. Thomas argued that HOLD cannot be used for synchronization. (Of course, he had no choice because that utterance was concurrent with the suspect shots.) On the contrary LG remind us, via their prior arguments, that HOLD is a valid crosstalk; so also is YOU. But their next step is their pièce de résistance—an assault of the Thomas ramparts via a reductio ad absurdum argument, using Thomas's own scenario. On Ch-1, HOLD occurs earlier than it should (according to Thomas), due to Skipback (SB). Thomas also insists that CHECK (by Fisher— "I'll check it") is crosstalk. In this discussion (see my Figure 21 just below) LG label the Ch-2 dead time (if any between HOLD and YOU) as DTHY (i.e., Dead Time Hold You); red arrows identify crosstalk events accepted by LG (i.e., **HOLD** and **YOU**). CHECK ("I'll check it," by Fisher) occurs on Ch-2, as everyone agrees. The utterance CHECK1 (from "Fisher") is LG's notation for Thomas's purported crosstalk on Ch-1—presumably (albeit incorrectly) derived from CHECK on Ch-2.

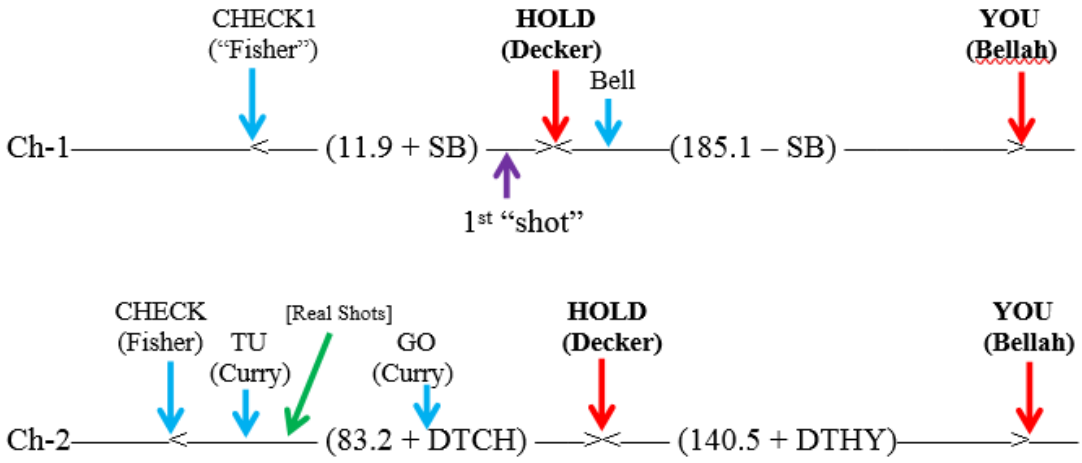


Figure21. This is a schematic (not to scale) timeline for the two channels. The time intervals in parentheses represent real world time. On Ch-1 the playback time between HOLD and YOU is 185.1 seconds, but on Ch-2 the playback time is only 140.5 seconds, a difference of 44.6 seconds (for comparison, see Thomas, p. 637). The NRC therefore

concluded that Ch-2 had stopped for a long interval; they also noted that the interval from GO to YOU (Ch-2) contained five periods of silence, each lasting at least 4 seconds. There is no dead time on Ch-1 due to the motorcycle engine. On Ch-1 the 3rd and 4th "shots" occur during HOLD. These "shots" can be located precisely via the bell (on Ch-1). No siren (or crowd noise) is heard during the suspect sounds on Ch-1, although such sounds should be heard. However, sirens are heard (as expected) in the Background of Police Chief Jesse Currys GO utterance (Ch-2), "Go to the hospital." GO occurred about 32 seconds after Curry's prior announcement that he was near the triple underpass (TU); the limousine would then have been in mid-Elm St, just where the real shots occurred. The bell (if authentic) was likely located at the Trade Mart (along with the motorcycle in question). The real shots must have occurred promptly after TU, and therefore some reasonable interval before GO. Neither GO nor TU are heard on Ch-1, so they cannot be used for synchronization. As authentic crosstalk's, LG used Decker (HOLD) and Bellah (YOU) for synchronization (red arrows). CHECK and CHECK1 are two different utterances. Thomas's use of CHECK1 as crosstalk leads to a deadly paradox (discussed below). The real shots were not heard on Ch-1 because the "bike with the mike" was not in Dealey Plaza; they are not shown in the timeline. The real shots were not heard on Ch-2 either, because Curry was not transmitting during those brief moments, nor was anyone else on Ch-2. They must have occurred very shortly after TU. The absence of real shots on Ch-2 is another clue that Ch-2 did not transmit continuously, even before HOLD.

* SB = Skipback time
* DTCH = Dead Time between CHECK and HOLD
* DTHY = Dead Time between HOLD and YOU
* GO = "Go to the hospital," by Curry
* CHECK = "Naw that's all right, I'll check it," by Fisher (per Thomas).
* CHECK1 = LG's name for Thomas's purported crosstalk on Ch-1
* HOLD = "…HOLD everything secure…," by Decker which, per the NRC panel, occurs at the same time as "shots" #3 and 4
* YOU = "YOU want me to…," by Bellah
* TU = "Triple Underpass," by Curry
* LG = Physicists Linsker and Garwin
* Missing time on Ch-1: The motorcycle never stopped...'so no time is missing.'
* Missing time on Ch-2: At least 30 ,seconds are missing, ' but Thomas Claims that no time is missing.'

INTERVAL (sec)	CH-1	CH-2
CHECK1 -> 1st "shot"	4.8*	N.A.
CHECK1 -> HOLD	11.9 [12.5]	N.A.
CHECK1 -> YOU	197.0	N.A.
1st "shot" -> HOLD	6.5*	N.A.
HOLD -> YOU	185.1 [172.9]	140.5 [143.1]
HOLD -> Bell	8.1	Bell was not identified
CHECK -> TU	N.A.	3.6
CHECK -> GO	N.A.	35.7
CHECK -> HOLD	N.A.	83.2 [99.1]
CHECK -> YOU	N.A.	223.5
GO -> HOLD	N.A.	47.5
TU -> GO	N.A.	32.1
TU -> HOLD	N.A.	79.6

Table 4. I calculated these time intervals from the Sonalysts's very precise tables, then multiplied by their factor of 1.05 (based on the background electrical hum). Note that, between HOLD and YOU, the two channels differ by 44.6 seconds (185.1-140.5), so Ch-2 had conspicuous dead time during this interval. The numbers in brackets were used by LG.

***Sonalysts do not identify the "shots," but O'Dell places the first "shot" at 6.5 seconds before HOLD. This implies that (CHECK ->1st "shot") = 11.3 – 6.5 = 4.8 seconds (assuming that SB =0). Thomas cites this interval as 2 seconds (p. 656). I measured time intervals from the beginning of one utterance to the beginning of the next, as did O'Dell.**

Using Figure 21, this next argument follows an approach suggested by LG and emphasized by Paul Hoch. We compare the interval for (CHECK1 -> YOU) on Ch-1 to the interval for (CHECK -> YOU) on Ch-2, first assuming that SB = 0. If CHECK1 is an authentic crosstalk, these two intervals should be equal in the real world. On Ch-2, the total time for (CHECK -> YOU) is 83.2 + 140.5 = 223.7 seconds. (This assumes no dead time; if dead time existed then the real world time would be even greater than 223.7 seconds—making the pending paradox even worse.) For the supposed comparable (per Thomas) interval on Ch-1: (CHECK1 -> YOU) = 11.9 + 185.1 = 197.0 seconds. We know that no time was lost on Ch-1 because of the motorcycle engine. Therefore 197.0 seconds represents the real world time on Ch-1 (assuming SB=0). If so, how can the supposedly comparable interval of (CHECK1 -> YOU) be longer (at 223.7 seconds) than the real world time? This is a discrepancy of 26.7 seconds. In order for this to make any sense the skipback (SB) on Ch-1 would need to be at least that long. Since each Dictabelt revolution requires 3.2 seconds (see Appendix 8 below), that would require a skipback of not just one or two grooves, but rather 8 grooves (26.7

÷ 3.2 = 8)! That is most unlikely. According to LG there was no significant Skipback on Ch-1, quite possibly none at all. Therefore, the acoustic case is dead.

The reductio ad absurdum of LG is illustrated next, but I have here used the Sonalysts data (from Table 4 above), instead of the data used by LG. For LG's time intervals, see Table 1 in Reference 9, Appendix 4.

* On Ch-1, if Skipback had occurred, then the real world time between CHECK1 and HOLD would be longer than the playback time of 11.9 seconds: (CHECK1 -> HOLD) = 11.9 + SB. On Ch-1 (Track 1), the playback time between HOLD and YOU is 185.1 seconds. Therefore, if Skipback had occurred, the real world time between HOLD and YOU must be less, i.e., (185.1 – SB) seconds, as shown in Figure 21 above.
* On Ch-2 (Track 7), the playback time interval from HOLD to YOU is 140.5 seconds. So, the real world time on Ch-2 from HOLD to YOU is (140.5 + DTHY) seconds. (If dead time existed, then the actual time must have been longer, so time must be added.) These two expressions, which describe the same real world time interval (HOLD ->YOU) must be equal:

$$185.1 - SB = 140.5 + DTHY, \text{ which leads to}$$

$$SB = 185.1 - 140.5 - DTHY = (44.6 - DTHY) \text{ seconds}$$

Since this latter expression must always be positive or zero (time intervals can never be negative), then DTHY can never be greater than 44.6 seconds, i.e.,

$$DTHY \text{ (dead time between HOLD and YOU)} \leq 44.6 \text{ seconds, and}$$

$SB \leq 44.6$ seconds (SB must be less than or equal to 44.6 seconds)

Next LG look at the time interval between CHECK and HOLD. They continue to use Thomas's scenario, which assumes that CHECK and CHECK1 represent true crosstalk. (Refer to Figure 21 above.) The playback time on Ch-1 (Table 4) is 11.9 seconds, so the real world time interval is (11.9 + SB). On Ch-2 (Track 7) the playback time between CHECK and HOLD is 83.2 seconds. The dead time between CHECK and HOLD is DTCH; therefore, the real world time interval from CHECK to HOLD is (83.2 + DTCH). As before, we can then equate these two expressions, since they should (according to Thomas) describe the same real time interval (CHECK ->HOLD):

$$11.9 + SB = 83.2 + DTCH, \text{ or}$$

$$SB = 71.3 + DTCH$$

Since DTCH represents real time it must always be positive or zero. Therefore,

$SB \geq 71.3$ seconds (SB must be equal to or greater than 71.3 seconds).

But this disagrees with the prior result, where SB had to be less than 44.6 seconds.

Because these two conclusions disagree, the assumptions cannot all be true. Therefore, using Thomas's own scenario, LG arrived at their masterpiece—a reductio ad absurdum. One of Thomas's assumptions is that CHECK leads to crosstalk CHECK1; that assumption is now highly suspect. LG conclude: (a) CHECK1 cannot represent a valid crosstalk, (b) there is no evidence for a Skipback (SB) on Ch-1, and (c) YOU is a valid crosstalk.

LG add that there is even more evidence that HOLD is a valid crosstalk, i.e., the presence of strong heterodynes. These are typically heard during crosstalk (as the difference between two frequencies)—and heterodynes are recognizable as beeps on the recording. Their presence at this specific moment implies that the crosstalk had been recorded live at the instant of the crosstalk—and had not been superimposed later. They also emphasize that the HOLD crosstalk is further validated by the bell sounds—after all, the intervals (from HOLD to bell) are consistent on the two channels. Therefore, if HOLD is a valid crosstalk (recall that it overlaps the 3rd and 4th suspect shots), then the first suspect shot occurred 61 seconds after Curry's "Go to the hospital"—and the acoustic case for conspiracy is mortally wounded. (To make this logic explicit, it makes no sense for Curry to advise a hospital visit before the shots begin.)

In their final letter to the editor (Appendix 4, Reference 11, Figure 2), LG add two captivating images related to CHECK and CHECK1. They display the spectrogram from the FBI copy of Ch-2 (Track 7) for "I'll check it." This is like a voiceprint, somewhat analogous to a fingerprint. Their Figure 3 does the same for Thomas's purported crosstalk CHECK1, from Ch-1 (Track 1). These images are displayed below in Appendix 9. Whereas we learned above that several auditors heard something other than "I'll check it" at this point on Ch-1, with these two new visual images we can now see (literally with our eyes) how different these voiceprints are. In other words, our ears and our eyes concur that CHECK cannot represent crosstalk, i.e., Thomas was wrong, and the acoustical case is moribund.

In reference 11 below (Appendix 4), LG advance an independent argument that CHECK did not trigger crosstalk. They note that the product of frequency (f) and time (T) is an invariant, i.e., it is independent of playback speed. Frequency refers to the speaker's pitch (in practice, it is the chosen harmonic of the fundamental pitch), and T refers to the time interval between two selected "feature" points in the pertinent spectrogram. For example, if the playback speed is changed that will stretch (or shrink) one axis, but at the same time it will do the opposite (by the same factor) to the other axis—so that the product fT remains constant. Since the unit of frequency is 1/sec, and the unit of time is sec, the product is (1/sec) x sec = 1, i.e., a dimensionless number. Therefore, it is no surprise that fT is an invariant. So, if a crosstalk is authentic, measurements made separately on each member of a proposed pair (e.g., CHECK and CHECK1) must produce identical values for fT. Unfortunately for Thomas, one of these two examples of CHECK/CHECK1 (on Track 7) is only 54% of the other (on Track 1). A second comparison (for a different interval), yields only 63%. By contrast, when this technique is

applied to YOU, and separately to ATTENTION, the product is 100% (for each utterance), with an error of only 1%. This is powerful—and independent—corroboration that CHECK and CHECK1 cannot represent crosstalk.

To conclude, the authors admit that they have ignored the statistical argument (as developed by the HSCA, and later by Thomas)—simply because the suspect sounds are irrelevant. These sounds simply came too late to be authentic gunshots. To put the matter most simply: once HOLD is accepted as a valid crosstalk (i.e., no Skipback on Ch-1, which was Thomas's best hope as an explanation for the Double Decker), then no further timing analysis is required. The suspect sounds then overlap with "…Hold everything secure…" (HOLD). Since, by the simple logic of human events (causes precede effects), this phrase must have occurred after the shooting (not during the shooting). Therefore, the suspect sounds cannot represent real shots—and the acoustic case for conspiracy lies lifeless in the morgue.

My summary. LG's scientific analysis is robustly persuasive—on many counts. Furthermore, it matches the eyewitness record. If CHECK and CHECK1 are assumed to represent crosstalk, a deadly paradox ensues in the timeline. Moreover, LG assemble multiple other lines of evidence against these utterances as crosstalk, including their spectrograms and the product fT. If CHECK and CHECK1 are discarded, then Thomas's case is history. Finally, the scientific evidence for HOLD and YOU as crosstalk events looks sturdy. In particular, if HOLD ("…Hold everything secure…" by Decker) is accepted, then Decker's utterance is concurrent with the suspect sounds (which then cannot be gunshots), the Double Decker wins, and this conversation is over. QED.

My Final Comments (2011): An Unexpectedly Negative Verdict

Despite the earlier worries that the Dictabelt (at NARA) might not be an original, the extant Dictabelt No. 10 (at NARA) is most likely the original (based on LG's hum analysis). Optical scanning (e.g., digitizing at the Lawrence Livermore Laboratory) might help to clarify issues (especially with respect to the hums), but many years have passed since that option was first raised, and we have heard nothing further from NARA.

In view of the uncertain chain of custody for the Dictabelts, a clever lawyer might still exclude them from the courtroom—unless, of course, the work of LG (regarding the hums) was persuasive to the judge. [DM: At least two more items might be banned from the courtroom: The Z-film and the magic bullet. Even the skull X-rays might find themselves as outcasts—I would agree to testify against them.]

As Thomas emphasizes, the order in the acoustic data is indeed impressive (using the socially acceptable p-values of 2001), and it is also consistent with the intervals on the street. Unfortunately, the suspect sounds probably derive from other sources. Also, recall that for a given microphone, the echo patterns were surprisingly similar for shot origins that were far apart in space—and only two sites were selected for possible gunman: the GK and the TSBD. Based on the rather small role played by sites of origin, even if the acoustic data were accepted, other sites for gunmen cannot be ruled out! (Aspiring lone gunman supporters, like Larry Sabato and Philip Shenon, have

abandoned elementary logic here. See Cyril Wecht's unanswered challenge to Sabato and Shenon:

https://www.justice-integrity.org/1311-wecht-capa-challenge-warren-report-defenders-sabato-shenon

The WA analysis was strictly virtual—no experimental data were ever taken. To check many more sites would have been overwhelming, but to check a few should have been feasible. And, aside from O'Dell's model of Dealey Plaza (cited above), no one has even tried to duplicate this by an alternate computation. That the WA match was so extraordinary for the suspect GK shot (26 of 26 Dictabelt impulses matched to the multiple impulses for the test shot) is impressive, although the NRC did try to whittle this down. In fact, they eventually got the probability into the range of insignificance ($p = 0.22$). And what if other Dictabelts (even from November 22) had been similarly analyzed: Might they also have shown such remarkable matches? (NARA still has four other Dictabelts from that date; the others have vanished into a black hole.)

Sergeant Bowles told Harry Livingstone that, several days after the assassination, he had taken tapes to Oklahoma, where they were copied. What happened there is not known. In addition, Bowles added that they had essentially worn out the belt and disc by repeated playing and that this may have caused some of the spikes (Killing the Truth 1993, Harrison Livingstone, pp. 353-354).

Bowles also told Livingstone that it was rather common for police recordings (on other occasions) to record gunshots (which were confirmed as gunshots by officers at the shooting site)—and that these were easily recognized as shots on replaying. On the other hand, on the Dictabelt, Bowles did not recognize any gunshots. As WA stated, these were not inaudible, but rather just sounded like static to the human ear. Gary Mack's sensitive ears disagreed with this but, unfortunately for him, he later had to admit that the "gunshot" sounds he heard occurred two minutes before the HSCA "shots"! [Gary Mack, "Acoustics as Easy as 1-2-3…4," The Continuing Inquiry, Vol. 2, No. 1, August 22, 1977, pp. 2-4.] It is regrettable that some of those other Dictabelts (see Table 16 by Thomas), even for November 22, were not subjected to a similar analysis—just to see how many (if any) false matches existed. Only Dictabelt No. 10 was scanned for such matches, but none were found on that one. (Well, there was one exception discussed above—the so-called 6th shot—but this did not satisfy the screening criteria.)

McLain's disagreement with the HSCA (and with Thomas, too) about his location is serious. Why did he change his story? His answer is that he felt pressure during his initial interview in Washington, mostly because he had been asked misleading questions, so afterwards he just wanted to clarify his recollections. He also emphasized that he had not listened to the tapes until after his HSCA appearance, which seems an odd sequence for discovering the truth. For many reasons now, I am strongly inclined to believe McLain—i.e., he did not have an open microphone. In 2006, McLain confessed to believing in a conspiracy:

https://kennedysandking.com/videos-and-interviews/interview-with-h-b-mclain.

No crowd noises or sirens appear on Ch-1, even though the stuck microphone should have recorded them. By contrast, they were easily heard on Ch-2. Even whistling can be heard (twice) on Ch-1. The so-called bell was also easily heard. So why were the gunshots so hard to hear on Ch-1? No one has an answer for that.

To my surprise, Thomas does not mention silencers, which were available in 1963 ("Silencers, Sniper Rifles & the CIA," Probe, Nov-Dec 1995, Carol Hewett). It is critical to recall that silencers can only affect the muzzle blast. They cannot affect the sonic wave, since that is produced during the flight of the bullet, far away from the gun. Did any dictabelt patterns (of sonic waves) suggest silencers? No test shots were fired with silencers so here is another black hole.

As a believer in a JFK conspiracy, I began this review hopeful that Thomas was correct; in fact, I would have been delighted had he been correct. But I have become more and more dubious about the acoustics case for conspiracy. The arguments advanced by LG (and O'Dell, too) are particularly decisive, although it was Barber who triggered this skeptical chain of events. It is unfortunate that more acoustic data were not taken, e.g., at least some data to confirm WA's virtual simulations. Even a limited sampling might have helped. Most researchers would also like data for other shooting sites. It cannot be emphasized enough that the HSCA used only two: the TSBD and the GK. The storm drain (on the overpass) might have been interesting. So, in the end, the acoustics data tell us nothing about other possible shooting sites. Of course, if the suspect shots were not real shots, they were always useless for this purpose anyhow.

Mostly in this review, I have ignored the issue of Z-film authenticity. Although it is a fundamental disagreement between Thomas and me, it was not necessary for me to invoke those beliefs in order to doubt the acoustics case. However, if the Z-film has been altered, then Thomas's remarkable audio-visual agreement for the critical time interval, between his first (Z-224) and second (Z-313) tie-ins, must be bogus. Thomas never acknowledged these problems of Z-film authenticity. Although he admits that the audio-visual correlation cannot be closer than five Z-frames, we already know that a prior correlation (by the HSCA) was quite different from his—in fact, they disagreed with him by much more than five frames. Furthermore, we have seen how even BBN often disagreed with WA by more than five frames. As one more astounding example, Alvarez changed his mind by up to 20 frame intervals, based on evidence that was supposedly objective. Therefore, the close correlation (of five frames) that Thomas claims for his audio-visual data may just be Madame Luck smiling at him.

If I were to base my entire case for conspiracy on the acoustics data, I would feel very insecure. On the other hand, the primary evidence for conspiracy in this JFK case (i.e., the medical evidence, the LHO items, and the historical documents) is so compelling that the acoustics data might serve as a light auditory encore. Unfortunately, that melody seems to have ended in discord.

Despite this, I still admire Thomas's persistence and courage for so meticulously picking his way through this massive and chaotic knoll of evidence. As a solitary prophet in this noisy, echo-filled wilderness, he has kept this peculiar story in play for a long while. For

that we all owe him and wish him well. However, many doubting Thomas's likely still have their guns cocked for Thomas. All that is missing are the sounds of gunfire. No doubt, though, Thomas has learned to bob and weave, and will not easily exhibit a (metaphorical) head snap.

Will Thomas be disappointed that I became unconvinced? I don't think so. After all, he is the outlier—the conspiracy believer who accepts the SBT. That's about as paradoxical as it gets. So why not a conspiracy believer (like me), who is not convinced by the acoustic data? To paraphrase a chief suspect in this JFK case, this house has many rooms. James Jesus Angleton, of the CIA, had offered precisely that comment when asked who had killed JFK. (As an aside, a patient gave me an Angleton family photo.)

So, if the suspect sounds were not shots, then what were they? Bowles thought that impressions in the disc from frequent replaying were responsible. O'Dell thought that normal speech patterns from hard consonants caused (or contributed to) the suspect sounds. Or perhaps they came from the motorcycle ignition system, or from the engine itself. WA suggested another possibility: electrical or mechanical disturbances in the DPD radio transmission, reception, or recording equipment. Even Barger had cautioned that while the loudest "impulse sounds" might be gunfire, they could just as easily be motorcycle engine misfires, ignition system noises, intermittent microphone relay sounds, scratches on the surface of the Dictabelt itself, electrical or mechanical distortions due to components in the communication system, or any number of other non-firearm sources. (See 8HSCA15 or Reference 5, Appendix 4.) McLain even said that these random radio noises could sound like gunshots. WA stated: "Some test more discerning than the human ear was required to determine the probable cause of the sound impulses." Perhaps the actual source of the suspect shots was not haphazard, but rather had a persistent and genuine physical basis (as O'Dell speculated). If so, then BBN should not have compared the suspect sounds to randomness, but instead to a real physical source. Unfortunately, the HSCA never seriously considered such an option, so an adequate explanation has evaded us.

*AN **UPDATE** (2019): A Contemporary Reflection on p-Values*

Thomas based his case on the surprising order (and sensible spatial intervals) in the data, which led to a communally respectable p-value (of less than 0.05). For decades before Thomas's paper, this p-value had been widely used in the medical literature, and in the social sciences, but not in the physics literature. The question therefore is simple: Is this p-value still good enough? For starters, his p-value does not account for the contrary evidence in Appendix 8. In fact, it avoided all of that evidence. Is that fair? In my opinion, this contrary evidence is powerful, if not overwhelming, and it must not be ignored. If these items had been included, would Thomas's p-value have been nullified? Very likely it would have shrunk into insignificance.

Moreover, in recent years (especially—and pertinently—since nearly every article cited in Appendix 4) we have heard an increasing professional clamor: a p-value of merely less than 0.05 is far too relaxed. This low threshold has already led to multiple misadventures in medicine and in the social sciences. [Examples are the still persistent

use of cardiac stents (hundreds of thousands per year) for stable patients, and hormone therapy for post-menopausal women (90,000,000 prescriptions per year at the last millennium). We now know this caused harm—and I still see these women for their breast cancers.] Physics would never be so indulgent. For example, the p-value required for the announcement (on July 4, 2012) of the Higgs boson was five-sigma, which corresponds to a p-value of $3x10^{-7}$, or a probability of 1 in 3.5 millions of occurring by chance. With such requirements in medicine, the total number of published medical articles per year could likely be read within one hour—or even less.

Here are several key quotations from the professional literature, with my underlining occasionally added. First, from a 2001 paper (the same year as Thomas's original article):

P-values need to be much smaller than 0.05 before they can be considered to provide strong evidence against the null hypothesis… We suggest that medical researchers should stop thinking of p <0.05 as having any particular importance… Bayesian arguments have been used to show that the usual p <0.05 threshold need not constitute strong evidence against the null hypothesis… it is reasonable to regard p <0.001 as strong evidence against the null hypothesis.

--"Sifting the evidence—what's wrong with significance tests?" by Jonathon A.C. Sterne, George Davey Smith, The British Medical Journal, Volume 322, 27 January 2001, p. 226.

In 2005 (four years after Thomas's original article), John Ioannidis (now a Stanford professor) published his most famous paper in the journal PLos Biology, Volume 2, Issue 8, p. e124: "Why Most Published Research Findings Are False." By 2011 (the year after Thomas's book), it had been viewed more than 400,000 times and by 2019 it had been cited 6775 times. According to Ioannidis:

There's a major problem using p-values the way we have been using them. It's causing a flood of misleading claims in the literature.

-- https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1182327/

-- http://robotics.cs.tamu.edu/RSS2015NegativeResults/pmed.0020124.pdf

Next, from a 2008 paper ("p-Value, a True Test of Statistical Significance? A Cautionary Note"), we have this comment (paraphrased here):

The use of 0.05 is not a standard with an objective basis. In fact, p= 0.05 is merely a convention that evolved from the practice of R.A. Fisher. There is no sharp distinction between "significant" and "not significant" results…

--https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4111019/

From an online article (paraphrased here) by Brian Resnick (July 31, 2017):

"Generally, p-values should not be used to make conclusions, but rather to identify possibilities—like a sniff test," according to Rebecca Goldin, the Director for Stats.org and a math professor at George Mason University.

And for a long while a sniff of $p < 0.05$ smelled pretty good. But over the past several years, researchers and statisticians have realized that $p < 0.05$ is not as strong as they once thought.

Many papers that used 0.05 could not be replicated with more rigorous designs.

A famous 2015 paper in Science found that only 39% of 100 papers in a prominent psychological journal could be replicated. In economics, it was 60%.

There is a credibility crisis also in biomedicine, but it is awaiting clarification.

Historians of science are always quick to point out that Ronald Fisher, the UK statistician who invented the p-value, never intended it to be the final word on scientific evidence. To Fisher, "statistical significance" meant the hypothesis is worthy of a follow-up investigation:

https://www.vox.com/science-and-health/2017/7/31/16021654/p-values-statistical-significance-redefine-0005

Ioannidis examined the most highly cited medical studies (published over a 15-year interval). Of 45 papers, seven (15%) later showed lower effects, and another seven were flatly contradicted by subsequent research. Nearly a quarter were never tested again, or the number of false results would likely have been higher. This has led to a quip, famous among doctors: "Hurry up and use the drug while it still works."

Statistics is the science that lets you do 20 experiments a year and publish one false result in Nature… Statistically significant and publishable results can occur, even though they are not true…

--The Half Life of Facts: Why Everything We Know Has an Expiration Date (2012), Samuel Arbesman

According to "Redefine Statistical Significance" (last edited July 2, 2018), Daniel J. Benjamin and over 70 [sic] other authors:

In biomedical research, 96% of a sample of recent papers claim statistically significant results with the $p < 0.05$ threshold. However, replication rates were very low for these studies…

Statistical standards of evidence for claiming new discoveries in many fields of science are simply too low. Associating "statistically significant" findings with $p < 0.05$ results in a high rate of false positives…

Ronald Fisher understood that the choice of 0.05 was arbitrary when he introduced it. Since then, theory and empirical evidence have demonstrated that a lower threshold is needed.

--https://psyarxiv.com/mky9j

Thirty-five percent of published re-analyses led to changes in findings that implied conclusions different from those of the original article about the types and number of patients who should be treated.

--JAMA, September 10, 2014

Another large coalition of 72 methodologists recently proposed a specific, simple move: lowering the routine p-value threshold for claiming statistical significance from .05 to.005 for new discoveries… P-values are misinterpreted, over trusted, and misuse… Multiple misinterpretations exist, but the most common one is that they represent the "probability that the studied hypothesis is true." … passing a statistical significance threshold is wrongly equated with a finding or an outcome being true, valid and worth acting on… Most claims supported with p-values slightly below .05 are probably false… Even among those claims that are true, few are worth acting on in medicine and health care.

--John P. Ioannidis in JAMA, April 10, 2018

As a pedestrian example, between Super Bowl XLIII and XLVII, the coin flip came up heads five times in a row. This probability is 1 in 2^5 = 1 in 32 = 3.2%. This is "more significant" than the p-value that the HSCA obtained. So, should we regard this coin flip as beyond random chance? If so, who controlled this bizarre outcome? Was God playing favorites?

A good friend and colleague recently sent me the result (Table 5 below) of his work on early detection of lung cancer. Notice the amazingly low p-value of 10^{-8}. This is evidence that respectable work is possible in biomedicine. But it is bad news for China, which suffers about 625,000 lung cancer deaths every year. If most of these diagnoses instead were cured via early detection, then the Chinese dependency ratio would escalate even more by 2050 than the predicted 44%. [Dependency Ratio = (population of dependents) ÷ (population aged 15 to 64).] This looming disaster has occurred due to a baby boom under Mao Zedong, followed by 36 years of a one-child policy.

Table 2. Summary of variants with significantly higher AF in lung cancer patients as compared to the general population (represented in 1KG)

AF cut-off	P value (Binomial test <)	# Alleles (>)	# Variants
0.1%	1×10^{-8}	9	231
5%		50	517

Table 5. Here is a p-value we would like to see more in the medical literature.

Of course, Thomas did not incorporate (into his p-value) the contrary evidence in Appendix 8. Almost any one of these items, by itself, might demolish Thomas's case.

To his credit I should emphasize that Thomas was indeed aware (in his 2010 book) of the limitations of p = 0.05. For example, he states (p. 626):

The true purpose of a statistical analysis is not to prove a hypothesis is correct [emphasis added], but rather to show that the empirical results are unlikely to be an accident…

And a few sentences later he states:

The five percent probability is the industrial standard among scientists for statistical significance, which does not mean that the hypothesis is mathematically proven [emphasis added].

As a pertinent aside, Paul Hoch adds an amusing anecdote. For his physics PhD

he measured the mass difference between the K^{*+} and K^{*-} mesons, using the bubble chamber data of Alvarez's group. His thesis was approved, but then later someone published a more accurate number. Hoch's had been wrong by three standard deviations (which corresponds to a 99.7% probability).

In summary, it is likely that Thomas's $p < 0.05$ cannot decide this case. And it may well not be replicable. In particular, Michael O'Dell has shown that many other rifle and microphone locations give comparable matches. And when the many, many arguments against it (Appendices 8 and 11) are included, especially the reductio ad absurdum, the acoustics data simply cannot be trusted. Furthermore, this data is not even required to make a powerful case for conspiracy. Some of that formidable evidence was discussed in the earlier part of this review, where Thomas and I often agreed.

One last corollary must be emphasized. Because the acoustic data are so unreliable, synchronization of that data with the Z-film is necessarily doomed to deceive, despite well-intentioned efforts by a few researchers. Oddly enough, this conclusion does not even require disbelief in the Z-film.

ACKNOWLEDGMENTS

Paul Hoch, PhD (physics) provided many links to the world of the acoustic skeptics. This review would have been destitute without them. He also corrected Thomas's 75% error rate in identifying Hoch and three others (incorrectly) as students of Alvarez. Michael O'Dell provided the revolution time for the Dictabelt, which I could not locate—in Thomas's work, or anywhere else. I must also thank (and applaud) him for sharing the results of his acoustic model of Dealey Plaza. Milicent Cranor, as usual, offered her own matchless critiques (especially about JFK's throat wound), which improved both clarity and accuracy. Denise Hazelwood contributed a detailed 7-page critique. Albert Rossi supplied useful insights about social constructivism. For my 2011 review, Ralph Linsker sent me their letters to Science and Justice. He noted that both had been cited in Science & Justice, Volume 46, no. 3, p. 199 (2006).

APPENDIX 1. Abbreviations used in this review

* AGC: automatic gain control
* AJP: American Journal of Physics
* ARRB: Assassination Records Review Board
* BBN: Bolt, Baranek & Newman
* BRSW: Barger, Robinson, Schmidt, and Wolf (of BBN)
* CE: Commission Exhibit (of the Warren Commission)
* Ch-1: Channel 1, used for general police business (the Dictabelt)
* Ch-2: Channel 2, dedicated to the motorcade
* DM: David Mantik
* DPD: Dallas Police Department
* DRE: a CIA-funded, Cuban exile group
* GK: grassy knoll
* 6H42: volume 6, page 42 of the ancillary Warren Commission volumes
* HSCA: House Select Committee on Assassinations
* 8HSCA95, 106: volume 8, pages 95 and 106 of the ancillary HSCA volumes
* JAMA: Journal of the American Medical Association
* JBC: John B. Connally
* LG: Linsker and Garwin, the first two authors of the recent NRC follow-up report
* LHO: Lee Harvey Oswald
* M-C: Mannlicher-Carcano

* NAA: neutron activation analysis
* NARA: National Archives and Records Administration (i.e., the National Archives)
* NPIC: National Photographic and Interpretation Center
* NRC: National Research Council
* SBT: single bullet theory
* TSBD: Texas School Book Depository
* WA: Weiss and Aschkenasy
* WC: Warren Commission
* WR: Warren Report
* Z-film: Zapruder film

APPENDIX 2. Definitions (some from Wikipedia)

* Caliber: American weapons use inches for caliber, whereas Europeans use mm.
* For example, the .30-.30 has a caliber of 0.30 inches = 7.62 mm.
* Carbine: usually shorter than a rifle and of lower velocity.
* Photogrammetry: determination of the geometric properties of objects, based on photographic images.
* Stereo photogrammetry: estimating the 3D coordinates of points on an object via measurements from two or more photographic images, taken from different directions.

APPENDIX 3. Ammunition terminology (from Wikipedia)

* Blank: cartridge without the bullet
* Bullet: just the projectile, which may have a jacket (not shown in the illustration)
* Cartridge (or round): the whole ensemble, as seen before firing
* Case: also known as casing or shell or hull—what is left behind after the projectile leaves
* Slug: the projectile, a term most often used with shotgun projectiles

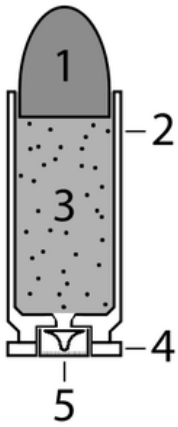


Figure 22. An image of modern ammunition.

A modern cartridge consists of the following

1. The bullet itself, which serves as the projectile:
2. The case, which holds all parts together;
3. The propellant, for example gunpowder or cordite;
4. The rim, part of the casing used for loading;
5. The primer, which ignites the propellant.

APPENDIX 4. Citations for the acoustic debate

1. "Reexamination of Acoustic Evidence in the Kennedy Assassination," Committee on Ballistic Acoustics, National Research Council (1982):

http://jfk-records.com/NRC_Science/science.htm

The committee drew these conclusions and made these statements:

A. Analyses of the acoustic evidence do not demonstrate that there was a grassy knoll shot, and there is no acoustic basis for the claim of 95 percent probability of such a shot.

B. The acoustic impulses attributed to gunshots were recorded about 1-minute after President Kennedy had been shot and the motorcade had been instructed to go to the hospital.

C. Therefore, reliable acoustic data do not support a conclusion that there was a second gunman.

D. …features of the recorded sounds strongly suggest that the open microphone was not in Dealey Plaza at the time of the assassination.

E. The impulses selected for the BRSW study were not always the largest ones.

F. weak spikes on the Dictabelt often are selected to correspond to strong patterns in the test patterns, and vice versa.

G. No siren sounds are heard on channel I at a time when they should have been heard by an open microphone in the motorcade; sirens are not heard for approximately 2 minutes…

H. James Bowles, police communications supervisor at the time of the assassination, suggests that it [the stuck microphone] was on a motorcycle parked at the police command post near the Trade Mart, where it would be natural to have adjacent police radios tuned to different channels.

I. Furthermore, even if it were granted that the hypothesis of randomly located impulses on relevant portions of the tape were in serious doubt, it would not follow that the alternative of gunfire from the grassy knoll was convincing.

All plausible alternatives to both hypotheses would have to be eliminated, and no convincing effort has been made in this direction.

J. the "hold everything" segment of channel II is present on channel I at the same location as the acoustic impulses [i.e., suspect gunshots].

K. These different forms of evidence are all compatible with the recordings [of sounds] made at the same time [i.e., "…Hold everything secure…" and the suspect gunshots], and some are incompatible with the hypothesis of later superposed recordings by audio or direct electrical coupling.

L. …the Committee on Ballistic Acoustics unanimously concluded that the acoustic impulses attributed to gunshots were recorded about 1 minute after the President had been shot and the motorcade had been instructed to go to the hospital…

2. Stephan N. Barber, "Double Decker

3. HB. McLain. See JFK: First Day Evidence 1993, Gary Savage, pp. 265-279 and pp. 313-410. The latter pages are by James C. Bowles (1979) and consist of his comments, along with detailed transcripts of both Ch-1 and Ch-2. Also see HSCA 180-10107-10184: staff interview of Hollis B. McLain, September 26, 1977 and 5 HSCA 629 and 5 HSCA 630. McLain's online interview is at:

https://kennedysandking.com/videos-and-interviews/interview-with-h-b-mclain

4. DB. Thomas, "Echo correlation analysis and the acoustic evidence in the Kennedy assassination revisited," Science and Justice, Volume 41: 217. -32 (2001) at:

https://www.jfklancer.com/pdf/Thomas.pdf

5. Dale K. Myers, "Secrets of a Homicide," at:

http:jfkfiles.com/jfk/html/acoustics_10.htm and

http://www.jfkfiles.com/jfk/html/acoustics_2.htm

6. Michael O'Dell, "The acoustic evidence in the Kennedy assassination"

Also see http://www.jfk-records.com for many excellent citations.

7. Steve Barber, "Of Crosstalk and Bells: A Rebuttal to Don Thomas's 'Debugging Bugliosi,'" (July 19, 2007) at:

http://jfkfiles.blogspot.com/2007/07/of-crosstalk-and-bells-rebuttal-to-don.html

8. Vincent Bugliosi, Reclaiming History 2008, Endnotes 381.

9. R. Linsker, R.L. Garwin, H. Chernoff, P. Horowitz, and N.F. Ramsey, "Synchronization of the acoustic evidence in the assassination of President Kennedy," Science & Justice, Volume 45, No. 4 (2005) 207-226 at:

http://jfk-records.com/ScienceAndJustice_45%284%29_207-226%282005%29.pdf

10. D.B. Thomas, "Correspondence received in relation to LG: 'Synchronization of the acoustic evidence in the assassination of President Kennedy,'" Science & Justice, Volume 45, No. 4 (2005) 207-226 at:

https://fas.org/rlg/ThomasComment__correspondence1.pdf

11. R. Linsker, R.L. Garwin, H. Chernoff, and N.F. Ramsey, "Acoustic synchronization: Rebuttal of Thomas' reply to Linsker et al." at:

https://fas.org/rlg/RL9b02_WithFigNums&Preamble_RL6818_JFKReply(+FullPageFigures).pdf

12. Charles Olsen and Scott Martin, "Analysis of the Dallas Police Department Dictabelt Recording related to the Assassination of President John F. Kennedy," March 25, 2013 at:

https://www.thekennedyhalfcentury.com/pdf/Kennedy-Half-Century-Audio-Research.pdf

13. Donald Thomas, "Sabato, Sonalyst's & Sophistry" (undated), at:

https://www.maryferrell.org/pages/Essay_-_Sabato_Sonalysts_Sophistry.html

14. Charles Olsen and Lee Ann Maryeski, "Further Research, Analysis, and Commentary on the Dallas Police Department Recordings of November 22, 1963," (6 June 2014) at:

http://thekennedyhalfcentury.com/pdf/Further_research_analysis_and_commentary_on_the_Dallas_Police_Department_recordings_of_November_22_1963.pdf

APPENDIX 5. The Sellier article (in German) on bullet deflection

The angle of deflection for three different bullet types, through aluminum, brass, and steel of thicknesses 1/16" and 1/8," ranged from 30 to 66 º, with the highest value for brass. For wood, the corresponding angles ranged from 12 to 36 º, depending on the

bullet type. Interestingly, in wood the 7.65 bullet gave the highest deflection (of 36 º). Ribs produced rather little deflection. None of these data, in my opinion, seem pertinent to the case of a bullet striking a skull, so it is not clear why Thomas cites this paper at all. Sellier also cites work done on deflections from concrete, asphalt, and grass. These data might be germane to the bullets that struck the street surface in Dealey Plaza, but Thomas does not open that door, nor have I explored it. Interestingly, Sellier cites work by M. Jauhari: "Bullet ricochet from metal plates," J. Crim. Law Pol. Sci. 60, 387-394 (1969). Since this paper is in English, and it appears to be the actual source of the metal deflection data (discussed by Sellier), it is odd that Thomas did not explicitly cite it. Another English language paper not cited by Thomas (but present in Sellier's bibliography) is this one: "FBI: Bouncing bullets." FBI Law Enforcement Bulletin, S. 2-6 u. 20-23. Washington, Sept./Oct. 1969. This is the paper that tests deflections from concrete, asphalt, and grass. One can only wonder: Was that work triggered by the bouncing bullets in Dealey Plaza?

APPENDIX 6. List of audio tracks (from Reference 9 in my Appendix 4, p. 209)

Channel No.	Track No.	Source
1	1	Bowles
2	2	Bowles
2	3	Bowles
2	4	"miscellaneous short segments"
1	5	FBI playback of Ch-1
2	6	from FBI turntable playback @ 33.3 rpm (recorded and played back @ 7.5 ips)
2	7	FBI turntable playback @ 33.3 rpm (recorded @ 7.5 ips, but played back @ 3.75 ips)
1	6B	portion of Bowles Ch-1, after end of Track 1

APPENDIX 7. Sources of uncertainty in the timelines of the audio tracks

1. The recording stopped, typically after 4 seconds of silence (in either channel).
2. Skips or repeats occurred during recording (Ch-2, Gray Audograph).
3. Playback caused skips or repeats (Ch-2).

4. A standard phonograph table (for Ch-2) was required for playback.
5. The police tape playback speed (Ch-1) was 5% too fast.
6. Crosstalk events might be misidentified.

APPENDIX 8. Challenges to the Acoustics Data—a Long Laundry List

NOTE: All of these issues were excluded from Thomas's calculation of his p-value.

1. The synchronization that Thomas used in his original (2001) article is not the one that he now supports. This change was triggered by information that came later, i.e., a correction in the timeline that the NRC had initially proposed:

http://mcadams.posc.mu.edu/odell.

Thomas's change of tactics evokes a disappointing image, i.e., one of repeating an experiment until the "right result" appears. (For an analogy, see the history of the measurement of the charge of the electron:

https://en.wikipedia.org/wiki/Oil_drop_experiment.

Thomas might even recognize this historic gambit as social constructivism at play.) But now the question becomes: How did Thomas get the right result in 2001 by using the wrong timeline? Thomas says rather little about this unexpected change in tactics—nor does he comment on the odd consequence of getting the same result from different assumptions.

2. The overlap of the "Double Decker" utterance with the "gunshots" can only occur via skipback on the Dictabelt. But to explain away the overlap the skipback must have been at least 30 seconds (and possibly as many as 60 seconds). Each groove on the Dictabelt (Ch-1) required a transit time (i.e., one revolution) of 3.2 seconds. Thus a skipback of 30 seconds requires a skipback of at least 9 grooves (30 ÷ 3.2 = 9), and possibly as many as 19 (i.e., 60 ÷ 3.2 = 19 seconds). But LG have demonstrated that even two-groove skipbacks on the Dictabelt are very unlikely (Appendix 4, Reference 9). In short, Thomas has an almost insurmountable challenge here. But what is truly strange is that he never addresses this curious situation directly—or quantitatively. Furthermore, other Dictabelts (of that same day) could have been searched for similar (major) skipbacks—but that was not done. I have not even discovered an interview with the DPD staff (e.g., the dispatchers, who logged these recordings) about skipbacks. [For comparison, each groove on the Audograph (Ch-2) required a transit time (i.e., one revolution) of 3.6 seconds.]
3. Over 80 witnesses (sic) recalled a "first sound" that was very different (Thomas, p. 702) from the subsequent sounds:

http://www.assassinationresearch.com/v5n1/v5n1costella.pdf.

Of these witnesses, 21 specifically described the first sound as either "loud," or "very loud," or as due to a "large" firecracker, or even due to a "giant" firecracker. Ten of these 80+ witnesses were reminded of a "backfire," and six of the 80+ specifically cited an "explosion." On the other hand, based on the impulse pattern, Thomas tells us that this first sound was quieter than those that followed. He also notes that this first sound had only about four echoes. But why wouldn't any loud sound produce the usual number of echoes? Had someone moved the buildings in the Plaza at this precise moment? Most likely, this first sound on the Dictabelt cannot represent a sound in Dealey Plaza. Because this first sound did not match any test pattern, BBN expunged it. Some researchers have suggested that this first sound (as heard by the witnesses—not as seen in the Dictabelt impulses) was due to a frontal bullet crashing through the limousine windshield. Douglas Weldon, especially, has written and lectured extensively about such a bullet. I have proposed that a glass shard from this event caused JFK's throat wound—and that it also caused (via tinier glass shards) the three tiny holes in JFK's cheek. The throat wound—other than an implausible SBT—has otherwise been very difficult to explain. Recent work on trajectories (by David Josephs and others) has suggested that the first shot occurred very near the Elm and Houston intersection. If so, that would make a windshield shot even more likely. (The higher elevation would make a bullet from the South Knoll more likely to hit the windshield.)

4. The same DPD radio system (as used in 1963) was available in 1978 (according to Bowles), but the HSCA did not record a single gunshot using the 1963 system—even though, according to Bowles, gunshots were typically easy to hear on such dictabelt recordings. (The HSCA instead used an old Motorola radio, an FM receiver, and a Dictaphone machine like that used in 1963.) Nor did they try to record a firecracker or a motor vehicle backfire on the 1963 radio system. Any one of these would have been rather simple to do—and might even have been enlightening, if not revolutionary. Even more interesting, of course (in view of the odd first sound recalled by so many witnesses), would have been an oscilloscope display of the sound of a bullet penetrating a windshield—using the 1963 DPD system. Such a sound (and display) can be heard (and seen) here:

   https://www.pond5.com/sound-effects/1/bullet-through-glass.html.

5. After hearing the first (real) shot, McLain promptly turned on his siren, but no siren is heard on Ch-1 (the Dictabelt).

6. No other siren is heard either on Ch-1 during the "gunshots."

7. On the other hand, a siren is heard on Ch-1 at 11:55 AM; a siren is also heard in the background of Curry's GO utterance (Ch-2).

8. No crowd noise is heard on Ch-2 during the "shots," even though such noise is easy to hear during Curry's transmissions from downtown.

9. McLain recalls that his departure from Dealey Plaza was triggered (promptly) by hearing Curry's order, "*Go to the hospital*" on Ch-2. But since McLain was supposedly tuned to Ch-1, he should not have heard this order on Ch-2. Nor could he have overheard this order from a nearby motorcycle—according to Dale

Myers, none were close enough. Furthermore, this announcement, if that loud (and due to a nearby motorcycle), should have crossed over to Ch-1, but it is not there.

10. The "shots" on Ch-1 (the Dictabelt) were not audible as gunshots.
11. Test shots (by the HSCA) were readily identified as gunshots.
12. The HSCA claimed the Dictabelt "shots" were unique, and not found elsewhere on the Dictabelt. (The Sonalysts disagree with this statement—see Appendix 11.) But parts of this recording were quite noisy, so similar sounds on other parts of the belt were not guaranteed to be detectable. Dr. Barger even stated, *"We found that there was one other sequence of impulsive events."* It differed from the purported shots *"...in that its timespan was less than 5 seconds."* This event occurred about a minute after the "gunshots." Its cause remains unknown. Furthermore, many other Dictabelts (over a dozen) were recorded that same day, but none were searched. One can only wonder: What other "gunshots" might have appeared in these other Dictabelts? For more (negative) comments on this issue, see Appendix 11.
13. The work of WA focused on the Grassy Knoll, to the exclusion of other sites. This emphasis unavoidably biased (or privileged) this site, which necessarily made a positive result more likely. O'Dell's acoustic model demonstrates the fallacy of this approach.
14. For detecting matches, only a Binary Correlation Coefficient of $r \geq 0.6$ was used. This was an arbitrary cutoff. What p-values would have resulted had somewhat different r-values been used? No one really knows. Is it fair for such an arbitrary selection to determine the final result? Thomas does not discuss this issue. Similar concerns arise with the coincidence windows used.
15. Both HSCA "shot" #1 and "shot" #2 (both said to be from the TSBD) also ironically matched to the GK—with $r = 0.7$ and $r = 0.6$, respectively. But the HSCA did not accept these as GK shots, even though the microphone that matched to the GK (for "shot" #2) was rather far from the TSBD microphones. Of all the parameters, the microphone position was the most critical, while the origin of the shot was the least critical. In fact, Thomas states (p. 589):

A gunshot from almost anywhere in Dealey Plaza was bound to be similar in pattern to gunshots from many if not most places in the Plaza.

If that is true, how then can anyone be quite certain where any of the four HSCA shots originated? After all, they only tried the TSBD and the GK! Finally, their "shot" #3 (claimed to be the GK shot) ironically also matched to the TSBD at two (adjacent) microphone positions (both with $r = 0.7$), while the GK site matched at only one microphone position (with $r = 0.8$). Despite this, the HSCA disallowed this shot #3 match to the TSBD. In yet one more anomaly (Thomas, p. 581), one of the evidence patterns matched to target #4 (Tague)!

16. It is often forgotten that false matches (between the test tape and the Dictabelt) were not only possible, but rather likely. BBN (Bolt, Baranek & Newman) had expected 13 such false alarms, but they found only six. In view of this, BBN stated, *"It is not unreasonable to expect that there are seven more* [false alarms] …" They therefore concluded that, of the remaining nine correlations, each was *"…equally likely to represent a detection or a false alarm"* (8HSCA 95,106). Furthermore, according to the Sonalysts (Appendix 11), nine (other) similar impulses were found within a three minute interval. BRSW claimed that three of these were gunfire, while the other six were not. However, the Sonalysists found that their waveforms suggest that all nine derived from the same source!

17. McLain recalls that he briefly stopped after the first shot, but the Dictabelt shows 30 seconds of silence (i.e., no motorcycle movement) during this much longer interval.

18. Based on his last visual sighting (near Houston and Main), McLain could not have reached Thomas's required site (near Houston and Elm) in time. McLain would have had to travel at 20-25 mph (instead of 6-8 mph, the motorcade speed) in order to arrive at Thomas's required site near Elm St.—and McLain would have had no reason for this odd acceleration.

19. On the Dictabelt, the motorcycle sounds decrease 3 seconds before the first "shot," but, paradoxically, McLain required all this time (or more) in order to reach Thomas's designated site in time.

20. No photograph shows McLain's position at the critical moment of the "shots."

21. McLain swears that he left Dealey Plaza promptly, but Thomas insists that he delayed by 30 sec.

22. McLain arrived at the hospital in time to assist Jackie from the limousine. To get there in time, he might have had to exceed his maximum motorcycle speed of 90 mph. This would have been especially challenging if he had waited for 30 seconds in Dealey Plaza, as Thomas claims he did.

23. The motorcycle sounded like a three-wheeler, but McLain had only two wheels.

24. Someone whistled a tune on Ch-1 at 12:31:56 and at 12:32:42. McLain was not known to whistle, but Leslie Beilharz was a frequent whistler, and he was not in Dealey Plaza during that time. Oddly enough, Beilharz supposedly used a three-wheeler.

25. Denise Hazelwood tells me (e-mail of March 4, 2019) that she contacted Thomas about the diagram of the test microphones in Dealey Plaza. He told her that the diagram was "created" by someone from BBN, but that this individual was not actually present during the event. Instead, the diagram was based on "street features." No other source seems to be available.

26. According to Linsker, *"Hold everything secure"* (HOLD) by Decker is crosstalk (concurrent with the suspect shots—as shown by PCC). The utterance YOU is also crosstalk. If true, Thomas's case is dead.

27. On Ch-2, within one second after CHECK, the utterance "10-4" is heard—by both Sonalysts and by Bowles. But Thomas claims that CHECK1 is heard as cross talk on Ch-1 from CHECK. So, if this CHECK crosstalk occurred due to a stuck microphone, why then would "10-4" not also be heard as crosstalk—within the next second? This gets worse. According to both Sonalysts and Bowles, four more utterances (including *"At the triple underpass*") occurred on Ch-2—within the next 14 seconds after "10-4." But if the microphone had remained stuck, why did none of these utterances cross over to Ch-1? Thomas does not address this strange outcome.

28. But here is a final crippling blow for Thomas. On Ch-1, the time interval between CHECK1 and HOLD is 11.9 seconds (Table 4); but HOLD was a real crosstalk, so we know that the microphone was still stuck at HOLD. Therefore, unless the stuck microphone was rapidly switching between stuck and unstuck (within the 14 seconds after "10-4"), some of these five utterances should have crossed over to Ch-1. None of them do. The recording did not stop on Ch-1—after all, the motorcycle kept it going. According to LG there was no significant skipback on Ch-1 (quite possibly none). But Thomas also insists on no missing time on Ch-2. So, if CHECK1 represents crosstalk, this is an impossible paradox, i.e., the time interval on Ch-1 (CHECK1->HOLD) should equal the time interval on Ch-2 (CHECK->HOLD). This time interval on Ch-1 is either 11.9 seconds or very close to that (see Figure 21). On the other hand, this supposedly identical time interval on Ch-2 is 83.2 seconds (or more, if DTCH is nonzero)—see Table 4. But how can the supposed same time interval of 11.9 seconds (on Ch-1) equal 83.2 seconds (on Ch-2)? Also notice that (by the work of LG and the Sonalysts) the real shots on Ch-1 occur about 60 seconds before the "shots." But only 11.9 seconds exist between CHECK1 and HOLD, so the real shots must have occurred long before CHECK1. On the other hand, on Ch-2, the real shots occur after CHECK. So, if the real shots occur both before CHECK1 and after CHECK, this is a paradox, and CHECK1 cannot represent crosstalk. Therefore, the acoustics case dies yet again.

29. Thomas disagrees with the HSCA on several issues; see Table 6 here, and also the next item in my list just below.

Comparison	Thomas	HSCA
Number of shots	5	4
First shot	Z-175	Z-160
GK shot	hit	miss
Number of gunmen	2-3	2

Table 6. Disagreements between Thomas and the HSCA

30. At Z-224:

    A. Per Thomas—the SBT arrives and Thomas's "gunshot" interval begins.

    B. Per the HSCA—they accepted neither of these conclusions.

31. The statistical analysis only considered random noise as an alternative to gunshots. The possibility of a real physical source of nonrandom noise was not considered. If the source of the gunshots was not random, then the chance of finding a match would likely be greater (than for a random source). This would correspondingly reduce the chance of a live GK gunman. For example, O'Dell has proposed (and demonstrated) that enunciation of the letters K, T, D, or S produce impulses remarkably like the "gunshots." Furthermore, O'Dell found speech patterns on the Dictabelt at the same time as the "gunshots." As another possible explanation (other than real gunshots), Bowles had suggested that persistent replaying of the Dictabelt had created dimples that produced sounds like gunshots. Barger himself wondered about motorcycle engine misfires, ignition system noises, intermittent microphone relay sounds, scratches on the surface of the Dictabelt, electrical or mechanical distortions due to components in the communication system.

32. The matching process only used time intervals; it disregarded the amplitude of the peaks. That is because the automatic gain control compressed the large peaks.

33. WA did not begin with a specific impulse pattern that was to be matched (or not matched) to a specific test pattern. What they did was quite different. Instead, they picked one impulse pattern on the Dictabelt, and then searched throughout the entire ensemble of test patterns until they found one that matched. This is a bit like doing the same experiment repetitively until the desired result obtains. It might be argued that the HSCA had no choice in this matter, but that is false. They could instead have accepted McLain's recollections, placed him at reasonable sites in the motorcade, and then tried to match just those test patterns that were spatially consistent with his successive locations (as he described them). That was never done; if it had been done, it is likely that no gunman would ever have materialized. Instead, the HSCA found their matches—after searching through the entire ensemble—and then decided where to place McLain. This is surely an odd sequence of events.

34. PCC analysis by Linsker shows no prominent peak for CHECK/CHECK1, which argues strongly against this (purported) event as crosstalk.

35. Thomas identifies CHECK and CHECK1 as crosstalk, but Linsker shows two very different spectrograms, as would be expected for two independent events. See Appendix 9 (just below) for images of these "voiceprints."

36. According to Linsker, if utterances CHECK and CHECK1 are assumed to represent crosstalk (as Thomas demands), then a *reductio ad absurdum*

necessarily ensues in Thomas's timeline, and the acoustics case is dead and buried. QED.

37. WA implied that the probability of a GK gunman was 95%. But that was highly misleading. Instead, they should merely have said that their match, based solely on their statistical analysis, had only a 5% chance of arising by chance—and just left it at that. More to the point, their statement ignored all the numbered items in this Appendix 8—and they never pointed that out.

APPENDIX 9. Spectrogram Comparisons.

Spectrogram Comparisons for *"I'll check it…"*

Here are the spectrograms for CHECK and CHECK1 ("*I'll check it,*" by Fisher).

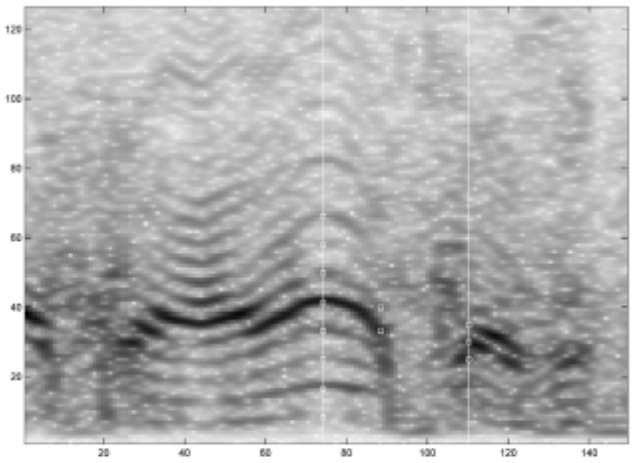


Figure 2 Spectrogram of a portion of Track 7 (Channel 2) containing "I'll check it." Abscissa represents the time bin (each bin starts 64 samples or 7.256ms after the previous bin); ordinate represents the frequency bin (17.23 Hz/bin). Added markings (white) identify selected feature points. See text for details.

Figure 23A. A voiceprint of "I'll check it" from Channel 2 (the Audograph).

Next is the spectrogram for CHECK1 on Ch-1. The actual words are dubious, but almost certainly do not represent crosstalk from CHECK, and Thomas has lost his lifeline.

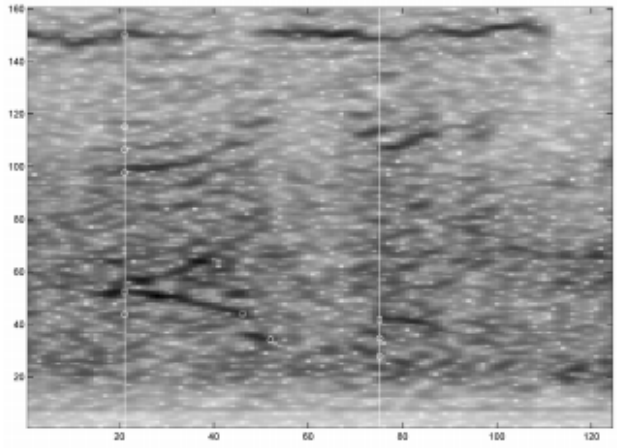


Figure 3 Same as Fig. 2, but for a portion of Track 1 (Channel 1) containing the utterance that has been transcribed as "I'll check it."

Figure 23B. A voiceprint of "I'll check it" from Channel 1 (the Dictabelt). These two spectrograms do not look similar.

The above figures are copied from *Science and Justice*, "Acoustic synchronization: Rebuttal of Thomas' reply to Linsker et al." R Linsker and RL Garwin IBM T.J. Watson Research Center, P. O. Box 218, Yorktown Heights 10598, USA H Chernoff Statistics Department, Harvard University, Cambridge MA 02138, USA NF Ramsey Physics Department, Harvard University, Cambridge MA 02138, USA. (See Appendix 4, Reference 11.)

The Sonalysts overlaid these two spectrograms on a single image (Figure 24). Thomas claims that the two spectrograms are the same.

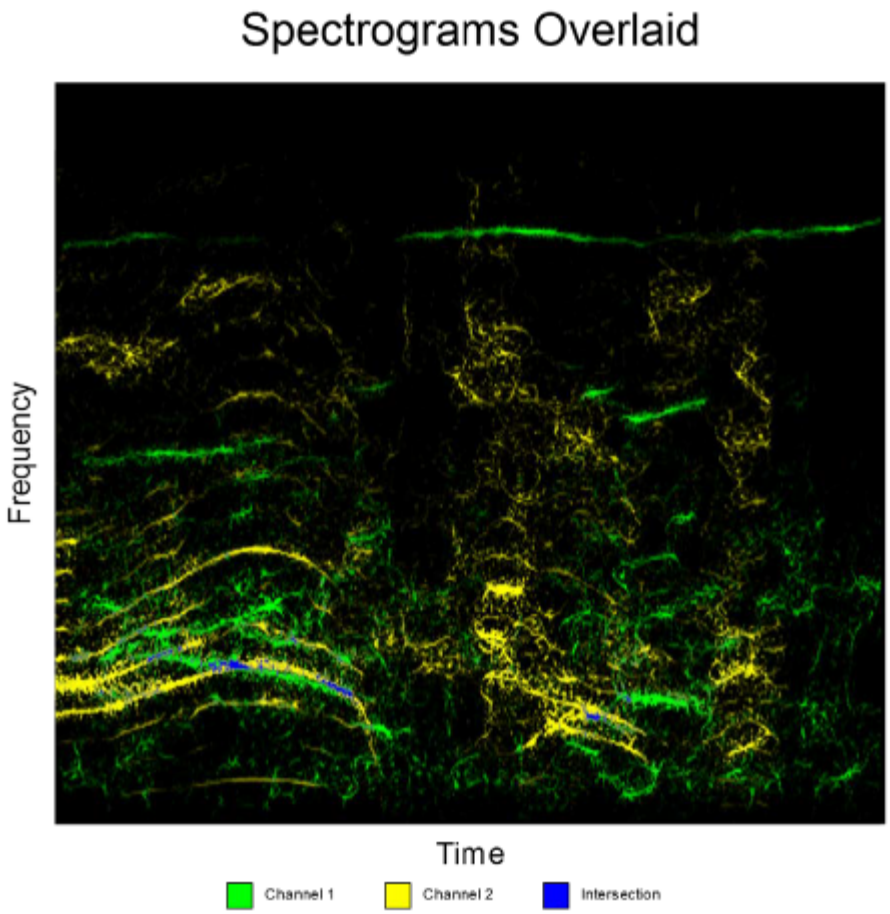


Figure 24. The color spectrograms for both channels are overlaid, as presented by the Sonalysts (Appendix 4, Reference 13). Thomas claims that the green and yellow curves are identical.

APPENDIX 10. So, what really happened in Dallas?

If the HSCA was wrong about McLain's location, then what did happen?

Here is a scenario that offers a better fit.

First, no authentic gunshots appear on the Dictabelt. Those sounds may not even be random; they may instead derive from a real and non-random physical source, such as the pronunciation of certain letters (K, T, D, S), as proposed by O'Dell. Many other possibilities exist as well, as listed by the Sonalysts.

If they are non-random, then statistical calculations that focus solely on randomness are irrelevant. Such a (nonrandom) scenario could explain why the Dictabelt "shots" were not audible, i.e., they were not gunshots at all, but rather derived from some other real source.

The other dictabelts (14 from November 22, 1963) were not thoroughly searched for similar sounds, or other "shots" may have emerged. Authentic gunshots (on other days) were typically easy to hear on the police recordings, just as the HSCA test shots were.

The stuck microphone may have been due to Leslie Beilharz (or his doppelganger), who was on the Stemmons Freeway—not in Dealey Plaza—during the shooting. He even admitted that his transmission button sometimes stuck—and he was known to whistle. Furthermore, he rode a three-wheeler that day, while McLain rode a two-wheeler. Beilharz probably arrived at the Trade Mart in time to record the bell sound. He was also in position there to record the Doppler effect of passing vehicles, an effect heard on the Dictabelt, whereas such an effect would not be expected for the slow moving caravan in Dealey Plaza. Because Beilharz was not in Dealey Plaza, no crowd noise would be expected, and the absence of sirens at the critical moment would be no surprise. Furthermore, McLain would not be required to accelerate from 6-8 mph up to 20-25 mph—for no good reason prior to the "shots." And the absence of photographs of McLain at the designated HSCA site near Elm St. would be no surprise—because he was not there.

McLain's arrival at the hospital (to assist Jackie) is also then no mystery—because he left Dealey Plaza promptly after hearing the first shot, and he had time to get there.

The LG analysis via PCC and fT also then makes sense—the valid crosstalks are HOLD and YOU, but not CHECK.

The appearance of "shots" concurrent with Decker's HOLD (i.e., the Double Decker) is also no longer a mystery, because these were not authentic shots. On the other hand, other physical noises (perhaps nonrandom) concurrent with HOLD would be no paradox.

And finally—and perhaps most importantly—the *reductio ad absurdum* no longer applies, because Thomas's CHECK as a crosstalk has been tossed out.

The coast is now clear to examine the other innumerable corroborations of conspiracy. By now we have had more than enough discussion of the acoustics data.

APPENDIX 11. The Sonalysts's Report

On October 15, 2013 this report was released by Larry Sabato (UVA Center for Politics). The lead article (of several) was authored by Charles Olsen and Scott Martin of Sonalysts, Inc., and titled, "Analysis of the Dallas Police Department Dictabelt Recording related to the Assassination of President John F. Kennedy."

Their conclusions (their p. 14), as listed here, cast grave doubt on the acoustic data.

1. The recording used in this study was the same as used by previous researchers.
2. Thomas's alleged Fisher crosstalk ("I'll check it") does not qualify as crosstalk, due the presence of a heterodyne tone. More likely the phrase is "*five seven."*
3. The measurement of (purported) motorcycle speed versus time disagrees with the known movements of the motorcycle.
4. The apparent movement of the "bike with the mike" during the critical period (in Dealey Plaza) is along an unimpeded route at a constant high speed. It shows no

slowing for turns. Such a deceleration would be expected for an authentic motorcycle in the parade route.

5. The apparent movement of the suspect motorcycle slows after a period of high speed. It then varies, which suggests maneuvering. On the other hand, during this same interval, the motorcycle should race to Parkland Hospital—at a very high speed. The data do not show this.
6. The (supposed) motorcycle speed slows when the sirens begin and remains slow, which suggests that it had stopped, or nearly stopped. This engine data is not consistent with a pause followed by a high speed chase to the hospital.
7. Apparent whistles are heard twice during the siren sounds. No Doppler effect is heard in these. This is more compatible with a stationary officer blowing his whistle. Blowing a whistle while at a high speed seems unlikely.
8. A car horn is heard four times. This sound shows a good deal of Doppler shift, which means that the car was moving with respect to the motorcycle.
9. These data are all consistent with a motorcycle that was stationary (or nearly so), while other vehicles raced by en route to the hospital. The car horn with the Doppler shift may have been part of the motorcade.
10. The suspect motorcycle remained near idle RPM for 15 seconds after the sirens faded away. This means that the motorcycle did not move with the sirens, nor did it join the sirens.
11. The motorcycle with the open microphone was not part of the motorcade, and therefore was not in a position to record authentic gunshots during the assassination.
12. The Dictabelt is irrelevant to gunfire during the assassination.

Another article in this set is “Observations on Properties of Impulses Attributed to Gunfire,” by Charles Olsen, Mark Banforth, and Jonathan Grant of Sonalysts, Inc. Their conclusions are as follows.

1. Some “gunshot” impulses are very similar to others nearby; some even occur within the “gunshot” intervals. Some of the “gunshot” impulses are not even unique.
2. Nine similar impulses were found within a three minute interval. BRSW claimed that three of these were gunfire, while the other six were not. The waveforms suggest that all nine derived from the same source.
3. One impulse—unlike those above—was said to be gunfire, while 3 nearly identical ones nearby were not.
4. The GK “shot” suggests that cyclical phenomena (i.e., damped oscillations) may have been selected as if they were individual transients. These phenomena are found throughout the audio files; additional searching might well locate even more.

5. BRSW's detections were not due to mechanical defects; they were of audio contained in the recording.
6. An adaptive filter that removed motorcycle engine noise changed one waveform—substantially and unexpectedly. Some peaks became much smaller, while others emerged! After such filtering, many of the "gunshots" disappeared almost entirely.
7. The BRSW's binary correlation detector (i.e., the matched filter) was likely overly optimistic in scoring; this might have led to false matches. The use of only timing data, at the expense of amplitude data, may have led to misleading matches. The performance of their algorithm (for matching) was apparently not tested, so it is not possible to know their real risk of false positives.
8. Digital signal processing has advanced greatly since BRSW and WA. More work could still be done (especially on engine noise). Obtaining the BBN test shots would also be useful.

The final article in this set of articles is "Further Research, Analysis, and Commentary on the Dallas Police Department Recordings of November 22, 1963," by Charles Olsen and LeeAnn Maryeski. They reached the following conclusions.

1. The speed of the suspect motorcycle does not agree with Officer H.B. McLain's recollections.
2. After Decker's utterance, three minutes follow during which the engine speeds up and then slows down, each period lasting less than 30 seconds. This cannot be a motorcycle en route to the hospital at a high speed, whereas McLain recalled that he "…was part of the motorcade en route to the hospital."
3. Sirens are heard as the engine slows to near idle RPM, even after the sirens pass. This fits with a motorcycle being overtaken, but does not match a motorcycle racing to the hospital.
4. Thomas's alleged Fisher "crosstalk" does not qualify as crosstalk. This conclusion is based on spectral comparisons as well as the presence of a heterodyne tone on the Dictabelt. The demodulated sound on the Dictabelt sounds nothing like Fisher's utterance (*"I'll check it"*) on Channel 2. Linsker and Garwin agree with this conclusion.
5. The BRSW report was misled by an incorrect assumption. It provides no insight into the assassination.
6. Remarkable advances in audio analysis have occurred since the 1970s. The authors were able to perform measurements that were previously impossible. These data are not consistent with gunfire.
7. Evidence of conspiracy should be sought elsewhere. The Dictabelt is irrelevant