

Research Misconduct Complaint

Submitted to Professor Joseph Rosse, Chair
Standing Committee on Research Misconduct
University of Colorado at Boulder

by Professor Ward Churchill
July 12, 2007

In accordance with the Laws of the Regents of the University of Colorado and the policies issuing therefrom, I am hereby submitting a complaint on grounds of research misconduct against University of Colorado/Boulder (UCB) Professor of Sociology Michael Radelet and four collaborators.¹ The bases of the complaint are several, including misrepresentation/concealment of sources and fabrication. Said offenses occur in a document titled *Report of the Investigative Committee of the Standing Committee on Research Misconduct at the University of Colorado at Boulder concerning Allegations of Academic Misconduct against Professor Ward Churchill* (May 9, 2006). Insofar as it has been posted on an official University website for purposes of broad public distribution and consumption under the University's imprimatur, the document constitutes published scholarship.² Although the document was nominally coauthored by Professor Radelet, UCB Professor of Law Marianne Wesson, UCB Distinguished Professor of History Emeritus Marjorie K. McIntosh, Arizona State University Professor of Law Robert Clinton, and University of Texas Professor of English José Limón—who, together with Professor Radelet, comprised the so-called Investigative Committee—Professor Radelet confirmed his primary authorship of the material specifically at issue in this complaint during his testimony before the University of Colorado's Committee on Privilege and Tenure (P&T) on January 21, 2007.³

¹ Most specifically at issue are the University of Colorado System, *Administrative Policy Statement Concerning Misconduct in Research and Authorship* (available at <http://www.cusys.edu/policies/Academic/misconduct.html>), and University of Colorado at Boulder, *Administrative Policy Statement on Misconduct in Research and Authorship, as Cited on Research Misconduct Rules, Operating Rules and Procedures of the Standing Committee on Research Misconduct* (available at http://www.colorado.edu/Academic/research_misconduct_rules_html). Insofar as the definition of misconduct advanced in the System Statement invokes "current federal regulations regarding scientific research misconduct, for example those promulgated by the National Science Foundation (NSF)," and further provides that these "policies and procedures...apply to University members on all campuses who are conducting research under different circumstances, regardless of whether or not it is in the field of science," the NSF regulations, as codified at 45 CFR, § 689.1 may be seen to apply (available at <http://www.nsf.gov/oig/resmisreg.pdf>). Insofar as Prof. McIntosh is a professional academic historian, the applicable standards are those set forth in the American Historical Association's *Statement on Standards of Professional Conduct* (available at <http://www.historians.org/pubs/Free/ProfessionalStandards.cfm?pv=y>); hereinafter referenced as *AHA Statement on Standards*.

² Publication occurred at the explicit request of UCB Law Prof. Marianne "Mimi" Wesson, who chaired the so-called Investigative Committee, apparently with the concurrence of the other committee members/coauthors. As Prof. Wesson explained in an e-mail communication to Prof. Fay G. Cohen of Dalhousie University on Mar. 23, 2006, "I have secured a commitment from the University administration that our report, unedited by any University officer, will be made public (copy on file). In addition to the *Report of the Investigative Committee Report* (hereinafter cited as *Report*), a audio download of a press conference featuring Prof. Wesson at the time the *Report* was released, and a press summary of the findings contained in the *Report*, are all posted under the University of Colorado imprimatur on an institutional website (all are available at <http://www.colorado.edu/news/reports/churchill/churchillreport051606.html>.) Unless the University wishes to acknowledge that the scholarship of one a senior faculty member was officially-assessed in something *other than* a scholarly fashion, the Investigative Committee's published *Report* must be treated as a work of scholarship, subject to the definitions, rules, and standards set forth in Note 1.

³ At p. 116 of the *Report*, Prof. Marjorie McIntosh is credited, in addition to writing her own section—which constitutes approximately half the page-length therein (sans appendices)—with having "integrated" the sections submitted by each of her coauthors, then "edit[ing] and format[ing] the final document." Prof. McIntosh must thus be seen as having served as lead author of the over *Report*, bearing primary responsibility for its contents. Her coauthors, however, apart from whatever offenses may be reflected in such material as they themselves drafted, must, by virtue of their approval of the "final product" crafted by Prof. McIntosh, must be seen as having been complicit in Prof. McIntosh's research

The Issue

The present complaint concerns Professor Radelet's handling of evidence in the section titled "Allegation C: Captain John Smith and Smallpox in New England, 1614-1618," which appears at pages 33-8 of the *Report*. As Professor Radelet explains at page 33, the focus of his investigation is "a statement made by Professor Churchill in an essay published in 2003: 'An American Holocaust? The Structure of Denial.'⁴ The essay argues, among other things, that Europeans and Euroamericans intentionally introduced the smallpox virus to Native American tribes as part of a larger effort that Professor Churchill contends should be called 'genocide.' After discussing the actions of British General Jeffrey Amherst and others at Fort Pitt in the Ohio River Valley in 1763, Professor Churchill writes, 'It's important not to view what Amherst did as an isolated matter. It wasn't. It's simply the best documented.' Allegation C refers *specifically* to the next sentences [emphasis added]:

There are several earlier cases, one involving Captain John Smith of Pocahontas fame. There's some pretty strong circumstantial evidence that Smith introduced smallpox among the Wampanoags as a means of clearing the way for the invaders. [140]

Note 140 cites Neal Salisbury, *Manitou and Providence: Europeans, Indians, and the Making of New England, 1500-1643*, pp. 96-101."⁵

At no point in his analysis does Professor Radelet question my identification in the preceding sentence of "Smith" as being the same "Captain John Smith" as that "of Pocahontas fame." Nor does he challenge my discussion of the Amherst affair,⁶ or my statement that there were "several earlier instances" of American Indians being deliberately infected with smallpox in a manner comparable to that documented in the Amherst example.⁷ And, while his placement of italics

misconduct. As they themselves put it at p. 10 of the *Report*, other than the discussion at pp. 99-103—which is not at issue in this complaint—"what follows represents a unanimous finding or conclusion of the Committee."

⁴ Ward Churchill, "An American Holocaust? The Structure of Denial," *Socialism and Democracy*, Vol. 17, No. 1 (Winter-Spring 2003) pp. 25-76; material at issue at p. 54. The "essay" is an edited/annotated transcription of several lectures, notably one delivered at the Brecht Forum, New York, in Sept. 1998.

⁵ The full citation is Neal Salisbury, *Manitou and Providence: Europeans, Indians, and the Making of New England, 1500-1643* (New York: Oxford University Press, 1982).

⁶ At p. 33n51 of the *Report*, Prof. Radelet refers readers to "Allegation D below, Section D2... For Amherst and Fort Pitt in 1763." The referenced section, which appears at pp. 55-8 of the *Report*, was written by Prof. McIntosh under the heading "Indian Oral Traditions." Interestingly, although at p. 55n137 she rather mockingly observes that "Professor Churchill provided several examples—from written, not oral sources—of Indian tribes that thought that whites had intentionally spread disease," she herself considers very little else. Indeed, she leads with the Amherst example precisely *because*—as I demonstrably pointed out in the passages quoted by Prof. Radelet at p. 33 of the *Report*—precisely because, as she echoes without acknowledgement at the safe remove of 22 pages, it's so "well-documented" (also see *Report*, p. 55 n 138). This little whiff of plagiarism notwithstanding, Prof. McIntosh's exposition on Amherst serves—without ever saying so directly—mainly to corroborate the accuracy of my own.

⁷ At p. 2n8 of a written submission titled "The Fort Clark Pandemic Revisited: A Case-Study of Genocide and Denial," cited as "Submission H" by Prof. McIntosh at p. 55n137 of the *Report*, I provided the following examples/references to the literature: "citing Sherburne F. Cook's 'The Significance of Disease in the Extinction of the New England Indians' (*Human Biology*, No. 45, 1973), I offered the belief of the Wampanoags and Narragansetts alike that English traders had deliberately infected the people in several villages with smallpox during the period leading up to 'King Philip's War' in 1675-6 [and] that the same pertained to the Pequots at the outset of the Puritans' 1637 war of extermination against them; see Francis Jennings, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill: University of North Carolina Press, 1975) pp. 207-8. Another example in which there is reason to suspect that 'the presence of smallpox germs in English goods was no accident'—a native conclusion that was 'prescient if not true'—concerns an epidemic that devastated the Cherokees in 1738; see Ronald Wright, *Stolen Continents: The Americas Through Indian Eyes Since 1492* (New York: Houghton Mifflin, 1992) pp. 103-4. Yet another concerns a 1752 outbreak of smallpox among the native peoples of the Ohio River Valley; see [E. Wagner] Stearn and [Allen E.] Stearn, *The Effects of Smallpox [on the Destiny of the Amerindian]* (Boston: Bruce Humphreys, 1945) pp. 42-3; Joel N. Shurkin, *The Invisible Fire: The Story of Mankind's Victory Over the Ancient Scourge of Smallpox* (New York: G.P.

around the word genocide appears intended to convey a certain skepticism concerning its appropriateness, he leaves this unchallenged as well (perhaps—although he does not say so—because he’s well aware that many scholars have drawn the same conclusion as I⁸).

Thus, contrary to Professor Radelet’s reference to “sentences” (emphasized above), only a *single* sentence on a *single* page in a *single* essay was/is at issue, i.e.: that in which I point to the existence of “circumstantial evidence,” which I consider to be “pretty strong,” indicating that “Smith introduced smallpox as a means of clearing the way for the invaders.” Yet, in their conclusion, at page 38 of the *Report*, Professor Radelet and his coauthors state that, “The Committee finds, by a preponderance of the evidence, that Professor Churchill misrepresented his sources in *two* essays when describing Captain John Smith and smallpox, a form of falsification. We conclude also the he fabricated his account, because no evidence—not even circumstantial evidence—supports his claim.”

Allegation 1: Falsification/ Misrepresentation of Sources (*re*, my “two essays describing John Smith and smallpox”)

Certain problems, both logical and factual, present themselves in the last two sentences quoted in the section above. First of all, it may be self-evident that one cannot be rightly said to have rendered an “account” in a single sentence. And, of course, I presented none in the sentence at issue. All I said therein is that circumstantial evidence exists. Hence, Professor Radelet appears to arguing that there is “no evidence—not even circumstantial evidence” of circumstantial evidence, and that I somehow “fabricated” the idea that there is. The illogic embodied in such “reasoning” is glaring. Since the interpretation of evidence for purposes of historical analysis is *inherently* subjective⁹—indeed, the Committee itself observes at page 45 of the *Report* that “Professor Churchill is entirely free to present his own reading of the evidence”—a finding of “research misconduct” on the grounds posited by Professor Radelet and his coauthors is not only facially absurd but, as will be amply demonstrated in the allegations below, factually false.

Putnam’s Sons, 1979) pp. 114-5. As one well-respected historian has put it, ‘There seems to have been a backwoods tradition of this sort of germ warfare’; Francis Jennings, *Empire of Fortune: Crowns, Colonies, and Tribes in the Seven Years War* (New York: W.W. Norton, 1988) p. 298-9.” For additional examples, see Adrienne Mayor’s “The Nessus Shirt: Smallpox Blankets in History and Legend” (*Journal of American Folklore*, No. 108 (Winter 1995) pp. 54-77; Elizabeth A. Fenn, “Biological Warfare in Eighteenth Century America: Beyond Jeffrey Amherst,” *Journal of American History*, Vol. 86, No. 4 (Mar. 2000) pp. 1552-80.

⁸ It is unclear from Prof. Radelet’s formulation whether the Committee’s skepticism is directed, à la such outright deniers as Steven Katz, Guenter Lewy and James Axtell, toward the appropriateness of the term “genocide,” per se, in describing the near-total eradication of indigenous American populations after 1492, or more narrowly, with regard to the reduction of such populations by disease. Assuming the former, the best response will be found in David E. Stannard’s “Uniqueness as Denial: The Politics of Genocide Scholarship,” in Alan S. Rosenbaum, ed., *Is the Holocaust Unique? Perspectives on Comparative Genocide* (Boulder: Westview Press, 1996) pp. 163-208. Assuming, on the other hand—as I’m inclined to do—that the narrower meaning was intended, see, as but a few recent examples of scholars sharing my view of the matter, the remarks on U.S. infliction on Indians of “the genocide of starvation, smallpox, tuberculosis, [and] the shooting of villages full of women and children” made by Sidney L. Haring in his *Crow Dog’s Case: American Indian Sovereignty, Tribal Law, and United States Law in the Nineteenth Century* (Cambridge, UK: Cambridge University Press, 1994) p. 251; the contention that “Indian genocide through smallpox-infested blankets was deliberate U.S. government policy” advanced by Anthony Weston in his *An Invitation to Environmental Philosophy* (New York: Oxford University Press, 1999) p. 46; the observation that, “Warfare...occasionally escalated into attempted genocide. More than once smallpox-infected blankets were traded to ‘troublesome’ Indians...” offered by Barry M. Pritzker in his *A Native American Encyclopedia: History, Culture, and Peoples* (New York: Oxford University Press, 2000) p. 401; the attribution of an intent to commit “racial genocide by smallpox” to Jeffrey Amherst in Daniel Barenblatt, *A Plague Upon Humanity: The Secret Genocide of Axis Japan’s Germ Warfare Operation* (New York: HarperCollins, 2004) p. 90. The list could be extended considerably.

⁹ See generally, Michael Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, UK: Cambridge University Press, 1988).

Meanwhile, the second problem imbedded in the Committee’s finding—i.e., its reference to there being two essays in which I’ve “describe[d]” a link between “John Smith and smallpox”—is purely factual. Professor Radelet offers no quote other than the single sentence already discussed to support his contention in this regard, nor even a general reference to where I supposedly said what he and his coauthors claim I said. Since the only other material both authored by me and cited by Professor Radelet in *any* connection are pages 169-70 of my 1997 essay, “Nits Make Lice,”¹⁰ it must be assumed that this is what he had in mind. There is, however, no mention of smallpox on either of the two pages cited by Professor Radelet, nor a connection made between John Smith and smallpox anywhere else in the entire 159-page essay.¹¹

Consequently, as concerns the supposed “second essay,” there are only two possibilities. Either Professor Radelet invented it altogether—in which case both he and his four coauthors, who “unanimously” endorsed his findings,¹² are mutually guilty of outright fabrication—or they are guilty of grossly misrepresenting their source. Either way, they have plainly engaged in evidentiary falsification, a matter defined as “research misconduct” under the standards set forth by the National Science Foundation (NSF) and the Public Health Service (PHS)—both of which are invoked in the University of Colorado’s *Administrative Policy Statement Concerning Misconduct in Research and Authorship*—as well as the American Historical Association’s (AHA’s) current *Statement on Standards of Professional Conduct*.¹³ Insofar as the information advanced in the Committee’s findings on page 38 are thus fraudulent, it follows that they must be considered invalid on this basis alone.

Allegation 2: Falsification/ Misrepresentation of Sources (re, Neal Salisbury’s *Manitou and Providence*)

When, at page 45 of the *Report*, the Committee acknowledged my right “to present [my] own reading of the evidence,” they immediately added the caveat that, “Like scholars in all fields, however, [I am] expected to present an account that is built upon and supported by that evidence.” Setting aside the fact that I have at no point offered anything that might be in any sense accurately described as “an account” of the “John Smith/smallpox connection—i.e., the matter supposedly at issue in the Committee’s “Allegation C”—there is seems little to quibble with in the Committee’s formulation. Nonetheless, it sets the stage for a systematic attempt on the part of Professor Radelet and his coauthors, who proceed as if I *had* in fact rendered “an account,” to create the impression that the evidence I would otherwise have been entitled to interpret as I saw fit simply does not exist (i.e., that I “fabricated” it).

This begins at page 33 of the *Report*, when Professor Radelet quotes/discusses my observation on “some pretty strong circumstantial evidence [suggests that] Smith introduced smallpox among the Wampanoags as a means of clearing the way for the invaders.” With regard to my citation of pages 96-101 of Salisbury’s *Manitou and Providence* in this connection, he concludes page 34 of the *Report* that, “The pages referenced by Professor Churchill in the Salisbury book do not contain the words [*sic*] ‘Wampanoags’ and have no discussion of any disease or epidemic (including smallpox).” He then further asserts that, “This is not an issue of incomplete footnoting

¹⁰ “Nits Make Lice: The Extermination of American Indians, 1607-1996,” in my *A Little Matter of Genocide: Holocaust and Denial in the Americas, 1492 to the Present* (San Francisco: City Lights, 1997) pp. 129-288.

¹¹ Rather paradoxically, Prof. Radelet admits this at p. 35 of the *Report*, observing that “the possibility that the epidemic was ‘smallpox’ was not mentioned’ in my 1997 essay, “nor did [I] claim circumstantial evidence that Smith intentionally introduced the disease.”

¹² “The use of such language as ‘the Committee finds’ or ‘we conclude’ in all parts of this document prior to the last page...indicates...a unanimous finding or conclusion of the Committee”; *Report*, p. 10.

¹³ For citations, see Note 1, above.

or lack of footnoting, but of misleading footnoting. It is simply false to assert that the pages cited from Salisbury's work support the claims made in the relevant passages by Professor Churchill."

Professor Radelet's claims are false in several respects. For example, at page 101 of *Manitou and Providence*, Salisbury states that, "From 1616 through 1618 the Indians were subjected to an epidemic, or series of epidemics, of catastrophic proportions." Similarly, at page 101, Salisbury mentions the Nausets; at pages 102, 103, and 104, he indicates that the "Pokanoket, on the eastern and northern shores of Narragansett Bay" were afflicted; at pages 101 and 103, he mentions the "Squanto's Patuxet" as well as the village of Nemasket. *All* of these are—or were—Wampanoag subgroups, a circumstance which would have been readily apparent to *any* scholar in the least conversant with the subject matter being examined.¹⁴

At one level, it is difficult to imagine a better illustration of the fallacy embodied in former Interim Chancellor (now Provost) Philip DiStefano's recent contention that there was/is no need to be "one bit concerned about the expertise of the committee"—which included neither American Indians, scholars competent in American Indian Studies, nor even a member specializing in the relevant region/historical period—because all five members were "experts in examining a piece of work and [determining] whether it was...falsified or fabricated."¹⁵ Surely, such a blatantly *unscholarly* postulation—that those charged with assessing the accuracy/validity of another scholar's research need know *nothing* about the subject-matter they are to examine—advanced as it was by the current chief academic officer of the University of Colorado/Boulder, should be sufficient to give pause to anyone in the least concerned with questions of scholarly competency and the integrity of the University's investigative process.¹⁶

In the present instance, the most charitable explanation of the situation is that Professor Radelet's near-total unfamiliarity with the subject-matter, and his consequent inability to comprehend what he was reading, led him into the assertion of an egregious falsehood (the vociferousness with which he asserted it being another matter altogether). Whether such charity is warranted seems rather dubious, however. It plainly required no particular expertise for him to recognize the word "epidemic" when he encountered it on page 101 of Salisbury. Nor was any appreciable knowledge of American Indians really necessary for him to have realized when he encountered the word "Pokanoket" in Salisbury that it was just another term for "Wampanoag." After all, at page 169 of my "Nits Make Lice"—a page he purports to have scrutinized rather closely—I recount how in 1602, "an exploratory probe [by the English] of the area around Cape Cod resulted in hostilities with local Wampanoags (Pokanokets)." On the same page, I also mention both the "Patuxet Wampanoags" and the Nausets.¹⁷

¹⁴ Actually, this should have been readily apparent even to a person who was essentially ignorant of the subject matter, assuming s/he bothered to read the elements of my work Prof. Radelet was purportedly critiquing. Therein, it is made quite clear that the word "Pokanoket" is synonymous with the more common term, "Wampanoag," and that the Patuxets and Nausets were both Wampanoag subgroups. See the essay, "Nits Make Lice: The Extermination of North American Indians, 1607-1996," in my *A Little Matter of Genocide: Holocaust and Denial in the Americas, 1492 to the Present* (San Francisco: City Lights, 1997) at p. 169. It seems curious that Prof. Radelet managed to miss this, since he devoted more than 2 pages of the *Report* to parsing things said on pp. 169-70 of this very essay.

¹⁵ Quoted in Jefferson Dodge, "Debate over Churchill case persists: P&T report to go to President Brown next week," *Silver and Gold Record* (Mar. 29, 2007).

¹⁶ Among other things, Provost DiStefano's remark stands in direct contradiction to principle articulated by the SCRM itself in June 2006, that those charged with vetting scholarship even in matters far less serious than an investigation of possible research misconduct should invariably be "individuals with as much expertise as possible in the reviewed individual's area of inquiry"; Joseph Rosse, et al., *Report and Recommendations of the Standing Committee on Research Misconduct Concerning Allegations of Research Misconduct by Professor Ward Churchill* (June 13, 2006; hereinafter referred to as *SCRM Report*) p. 18.

¹⁷ Lest there be any confusion with regard to the latter term, "About 1620, the...Pokanoket (or Wampanoag)...comprised a group of allied villages in eastern Rhode Island and in southeastern Massachusetts, south of Marshfield and Brockton. Gookin (1972: 8-9) includes all of Cape Cod, Martha's Vineyard, and Nantucket within the

At page 21 of *Manitou and Providence*, moreover, Salisbury himself refers to “the Pokanokets (or Wampanoags),” situating them geographically in “southeastern Massachusetts and the eastern side of Narragansett Bay,” as well as “Cape Cod, Martha’s Vineyard, and Nantucket.”¹⁸ Plainly, there was ample basis in the material he purported to have read rather closely for Professor Radelet to have known that the words Pokanoket and Wampanoag are essentially synonyms. That being so, his categorical denial that either an epidemic or Wampanoags are mentioned in the relevant pages of Salisbury *cannot* have resulted from simple “error,” as is now contended (see Allegation 3, below). Either Professor Radelet failed to read material he claimed in the *Report* to have read, or he misrepresented it, grossly and deliberately. Irrespective of which of these two possibilities is the more accurate, he and his coauthors are mutually guilty of falsification.¹⁹

Allegation 3: Falsification/Suppression of Disconfirming Evidence (*re*, the matter of Professor Churchill’s sources)

At page 35 of the *Report*, Professor Radelet observed that, “In an interview with our Committee on April 1, 2006, Professor Churchill said that in preparing his 2003 essay for publication (it was a composite of several oral presentations), he had added footnotes by referring back to [an earlier] essay. He told us that he erroneously copied in a reference to Salisbury when he should have cited another source. He did not tell us what the correct reference should have been...”²⁰ In a follow-up letter dated 9 April 2007—that is, shortly after Cornell University Professor Eric Cheyfitz had made public certain of the misrepresentations of Salisbury addressed in Allegation 2²¹—Professor Wesson, who chaired the Investigative Committee, offered a “clarification.”

In our report, we addressed Professor Churchill’s reference to a book by Neal Salisbury (citing pages 96-101) in support of his claim that “strong circumstantial evidence” pointed to the conclusion that Captain John Smith intentionally introduced smallpox among the Wampanoag people of New England and thus caused an epidemic of the disease. We wrote (on page 34 of the report): “The pages referenced in the Salisbury book do not contain the words “Wampanoag” and have no discussion of a disease or epidemic (including smallpox).” In this assertion we were incorrect. There is, beginning at page 101 of Salisbury’s work, a discussion of a disease epidemic that began in 1616 among the native peoples of New England. There is also at page 102 (beyond the range of Professor Churchill’s citation, but still a part of the same discussion) a reference to the “Poka-

borders of this group. Swanton (1952: 21-7), following Speck (1928d), assigns the Cape Cod groups a separate ‘Nauset’ tribal identity, which may, in reality, reflect only the postcolonial situation; Bruce Salwin, “Indians of Southern New England and Long Island: Early Period,” in Bruce G. Trigger, *Handbook of North American Indians, Vol. 15, Northeast* (Washington, D.C.: Smithsonian Institution, 1978) p. 171; citing Frank G. Speck, “Territorial Subdivisions and Boundaries of the Wampanoag, Massachusetts, and Nauset Indians,” *Indian Notes and Monographs* (New York: Museum of the American Indian, Heye Foundation, Misc. Ser. 44, 1928); John R. Swanton, *The Indian Tribes of North America* (Washington, D.C.: Smithsonian Institution, Bureau of American Ethnology Bulletin 145, 1952). Interestingly, there is no bibliographical entry for “Gookin, 1972.” In any case, the Nausets are generally considered to be “Cape Wampanoags,” both by the Wampanoags themselves, and by non-Indian scholars.

¹⁸ Lest it be argued that it would be “unreasonable” to expect to examine portions of Salisbury outside the range of pages I myself cited—i.e., pp. 96-100—it should be emphasized that he consistently did so for other purposes; *Report*, pp. 36n63 (“Salisbury, *Manitou*, pp. 102-03”), 36n64 (“Salisbury, *Manitou*, p. 57”), 36n64 (“Salisbury, *Manitou*, p. 58”), 36n66 (“Salisbury, *Manitou*, pp. 76 and 101”), 37n68 (“Salisbury, *Manitou*, pp. 101-02”).

¹⁹ “The use of such language as ‘the Committee finds’ or ‘we conclude’ in all parts of this document prior to the last page...indicates...a unanimous finding or conclusion of the Committee”; *Report*, p. 10.

²⁰ The “earlier essay,” was “Nits Make Lice” (see Note 10, above).

²¹ Prof. Cheyfitz is quoted extensively in Dodge, “Debate over Churchill case persists.” Also see Professors Vijay Gupta, Margaret LeCompte, Paul Levitt, Thomas Mayer, Emma Perez, Michael Yellow Bird, Eric Cheyfitz, Elisa Facio, Martin Walter, Leonard Baca, and Brenda Romero, “A Filing of Research Misconduct Charges Against the Churchill Investigating Committee” (submitted to the SCRM, May 10, 2007) p. 6.

noket” as one of the peoples who suffered greatly from the epidemic. Professor Cheyfitz has reportedly said that the Pokanoket are (or are a branch of) the Wampanoag. Thus our statement was literally incorrect concerning the absence of any mention of disease, and (if Professor Cheyfitz is correct) it did not take account of the possibility that the people mentioned in the ensuing discussion were part of the Wampanoag tribe.²²

Having thus made a (decidedly partial) concession concerning the *Report*’s inaccuracies in this regard, Professor Wesson waxes eloquent on her belief “that willingness to acknowledge one’s errors is an important aspect of research integrity,” concluding that she “regret[ed] this error and wish[ed] to correct it...for the scholarly record.”²³ Meanwhile, however, she had contrived to cast an appearance that the onus of responsibility for the “error” was mine rather than that of the Committee itself. Embellishing upon Professor Radelet’s observation, quoted at the beginning of this Allegation, she wrote that:

As the report explains, Professor Churchill’s response to the Committee’s questions concerning the use of Salisbury as a source for his claims was not to make the points now argued by Professor Cheyfitz (who did not appear before the Committee), but to disclaim any reliance upon Salisbury, saying that the reference to Salisbury had been a mistake caused by hasty writing. Possibly he made this choice because Salisbury makes clear that Smith left New England in June of 1614, never to return, and that the epidemic did not begin until some time in 1616. I encourage those who would see [the Committee’s] error as an important factor in our findings to read the report with care.²⁴

By this point, one needs a veritable scorecard to keep track of the falsehoods and misdirections with which Professor Wesson’s two-page missive is laden. First, for reasons thoroughly explained in Allegation 2, above, there can be no legitimate question as to *whether* “Professor Cheyfitz is correct” that the words “Pokanoket” and “Wampanoag” refer to the same people, or that Professor Radelet—and by extension his coauthors, including Professor Wesson herself—were guilty of mere “error” when they claimed the opposite. Second, while page 102 of Salisbury is indeed “beyond the page-range” of my citation, it is not beyond the page-range cited by Professor Radelet and his coauthors; *they* referred not only to the pages within my page-range, but pages 102 and 103 as well.²⁵ Third, there is the issue of why, assuming the Committee believed that I’d “disavowed any reliance upon Salisbury,” as Professor Wesson now claims, Professor Radelet relied all but exclusively upon *Manitou and Providence* while “investigating the integrity” of my work, leaving the source upon which they purportedly believed I *did* rely all but unmentioned.²⁶

²² Wesson to Profs. Katherine Eggert, Daniel Kim, and Margaret LeCompte (Apr. 9, 2007) p. 2.

²³ Wesson to Eggert, et al., p. 2. Prof. Wesson’s framing of the *Report* in terms of “the scholarly record” should retire once and for all the “controversy” over whether it must be viewed as a “work of scholarship” published under the University’s imprimatur and therefore subject to the same standards of assessment it employs. It is, on all counts.

²⁴ Wesson to Eggert, et al., p. 2.

²⁵ See Note 18, above.

²⁶ As I informed the Committee during a colloquy conducted on Apr. 1, 2006, I’d erred in the process of transposing notes from my 1997 “Nits Make Lice” to the 2003 transcription of my talks, “An American Holocaust?,” inserting the reference to Salisbury’s *Manitou and Providence* into the latter as Note 104 when the better citation from the 1997 essay would, for my purposes in 2003, would have been Ian K. Steele’s *Warpaths: Invasions of North America* (New York: Oxford University Press, 1994). Professor Radelet’s only mention of the Steele book is an acknowledgement at p. 35n59 that I had in fact cited Steele—“appropriately”—in 1997. Although he requested and I provided my working copy of *Warpaths*, Professor Radelet gives no indication in the *Report* that he actually read any portion of it. By contrast, he cites *Manitou and Providence* on 7 occasions in the *Report*, each time on substantive points. Such an emphasis on Salisbury, endorsed by the entire Committee, hardly supports the idea that the members believed I’d “disavowed any reliance” on him. Quite the opposite, in fact.

Fourth, and most significantly, no matter *how* much “care” others might invest in reading the *Report*, they would gain no insight whatsoever into the actual nature of the April 1, 2006, exchange between the Committee and me, upon which the positions taken by both Professor Radelet and Professor Wesson may be seen to hinge. This is because, although testimony by expert witnesses as well as my own discussions with the Committee are cited throughout the *Report*, transcripts were never made available for public inspection.²⁷ “Others” are thereby left with no practical means by which to assess the accuracy—or lack of it—with which the Committee has quoted/described/summarized what was said.

The withholding of the primary documents and other such materials upon which the *Report* was purportedly based is itself a serious form of research misconduct, violating as it does the most basic requirements of professional scholarship and academic integrity. As the matter is framed in the *AHA Statement on Standards*, on which the Committee claimed at p. 10 of the *Report* to have subscribed, scholars have a fundamental obligation to “make available their sources, evidence and data” in a manner ensuring “free, open, equal, and nondiscriminatory access” to materials not otherwise publicly available.²⁸ Both the NSF standards and those of the PHS contain similar provisions requiring that primary research materials be rendered easily accessible—not least for verification purposes—at the time the results of the research are published.²⁹ And, somewhat ironically, the Committee itself, quoting the *AHA Statement on Standards*, makes a similar point—thereby demonstrating its awareness of “the rules”—at page 11n15 of the *Report*.³⁰

The Committee’s collective failure to comply with the fundamental requirements applying to scholars engaged in primary research cannot be reasonably described as an “oversight.” Since their work product was published by the University electronically, there could be no appreciable financial constraints their simultaneously posting the primary materials upon which they claimed to have relied as attachments to the *Report* itself. This would plainly have been the most efficient, coherent, and responsible manner in which to proceed.³¹ Had the transcripts and related items cited throughout the *Report*—and neatly listed in Appendix C as evidence considered by the Committee³²—been made thus readily-available, however, readers would have been equipped with the means to detect the extent to which the evidence was often twisted or literally falsified in the body of the document.

As illustration of how radically the record has been distorted, consider what was *actually* said in the April 1, 2006, colloquy invoked both by Professor Radelet (in the *Report*) and by Professor

²⁷ The same pertains to my various written submissions, listed as Submissions A-H at p. 155 of the *Report*. Self-evidently, all such materials could have been posted right along with the *Report* itself, when the latter was electronically published by the University in May 2006. That this was not done can only have resulted from a conscious choice on the part of the Investigative Committee to withhold the material from public scrutiny.

²⁸ See Note 1, above, for full citation of the *AHA Statement on Standards*. For a thorough discussion, see Jon Wiener, *Historians in Trouble: Plagiarism, Fraud, and Politics in the Ivory Tower* (New York: New Press, 2005) pp. 31-57.

²⁹ It is worth reiterating that the NSF and PHS standards are incorporated by reference into the current *Policy Statement Concerning Misconduct in Research and Authorship* of the University of Colorado (full citation in Note 1, above).

³⁰ At p. 80n202, the Committee returns to the theme—and this time to identical effect—observing that the *AHA Statement on Standards* “stresses the importance of...making [one’s evidence] available to other scholars.”

³¹ It should be noted that during her testimony before a P&T grievance panel on June 8, 2007 (transcript not yet available), Prof. Wesson was confronted with this very issue. In response, she sought—as always—to shift the burden of responsibility onto me, observing that I, personally, had received copies of all transcripts and the like, and that I’ve been “free to release them” at any time. As should be obvious, however, it is not—and was never—in *any* sense *my* responsibility to fulfill the *Committee’s* scholarly obligations. Nor, under the circumstances, is it at all obvious how she thinks *I* might have been able to have such material posted on the University website, in conjunction with the *Report*.

³² *Report*, pp. 114-6.

Wesson (in defending its “scholarly integrity”). Quoted directly from the transcript, with extraneous dialogue/details omitted, and clarifying language inserted in brackets, here are the two exchanges at

PROFESSOR CHURCHILL: [In 2003,] I cite[d] the wrong of the two sources in conjunction with the statement [about John Smith in “An American Holocaust?”]... I probably looked at the wrong note in [“Nits Make Lice” when transposing annotation from it to the later essay] and wrote in [*Manitou and Providence*] rather than the one I was hanging my argument on primarily... The better source would have been *Warpaths* [by] Ian Steele...³³

PROFESSOR WESSON: I’m sorry, would you say that again[?] The better source would have been[?]

PROFESSOR CHURCHILL: Ian Steele, a book called *Warpaths*.... It’s not that [Salisbury’s *Manitou and Providence*] doesn’t support [my premise] in terms of providing circumstantial evidence...but the other one [Steele’s *Warpaths*] is clearer...³⁴

At a later point in the same session, Professor McIntosh returned momentarily to the topic, characterizing the earlier exchange in terms of my having “acknowledge[d]” that my citation of Salisbury “was a mistake.” In reply, I reiterated what I’d said earlier, i.e.: that in my estimation, his was simply “not the stronger of the two sources,” going on to add that, ultimately, “there’s no particular reason that I would have cited Salisbury as opposed to Steele,” or vice versa, although “my assumption here is that I thought I was citing Steele.” Professor Radelet then responded, “Okay.... Thanks,” thereby indicating rather clearly that he understood what I had just explained.³⁵

Thus, contrary to the claim advanced by Professor Radelet on page 35 of the *Report*, I plainly *did* inform the Investigative Committee “what the correct source should have been”—not once but three times, *twice* on the very page of the transcript he appears to have paraphrased when framing his assertion to the contrary.³⁶ This, in combination with his personal acknowledgement of the information about Steele, quoted above, makes it abundantly clear that Professor Radelet knew full-well that the statement he entered into the *Report* was false.³⁷ As was the case in Allegation 2, the possibility that Professor Radelet’s misrepresent-

³³ The full citation is Ian K. Steele, *Warpaths: Invasions of North America* (New York: Oxford University Press, 1994). I quoted from p. 89 of the book. The “earlier essay” in which the quote appears—and from which I transposed annotation to my 2003 piece in *Socialism and Democracy*—is

³⁴ *Investigative Committee Transcript* (Apr. 1, 2006) pp. 155-6.

³⁵ *Ibid.*, pp. 173-4.

³⁶ I say “apparently” because Prof. Radelet cites *nothing* in support/corroboration of his description of what was said during my “interview with [the] Committee on April 1, 2006.” It should be mentioned, however, that, throughout the *Report*, the Committee routinely cites transcript, exhibits, and other such items, without providing the page numbers on which various quotes, paraphrased language, and facts supposedly appear. The Committee’s adoption of the “unconventional referencing style [of] referencing a lengthy source without pinpoint citation [makes it] far more difficult [for] other researchers to check [their] claims,” even assuming “other researchers” had access to the materials at issue. I am, of course, quoting—and adapting to my own purposes—the Committee’s criticism of *my* citational practices; *Report*, pp. 23, 25. There is certainly no shortage of examples of the Committee doing exactly what it complains about; see *Report*, Notes 34, 38, 48, 67, 80, 98, 99, 101, 102, 114, 115, 137, 144, 162, 163, 164, 166, 168, 169, 170, 172, 173, 174, 175, 176, 183, 184, 185, 186, 190, 193, 194, 198, 201, 209, 211, 214, 224, 227, 228, 238, 242, 244. Prof. McIntosh in particular is also prone to employing the same “unconventional” referencing style more broadly, e.g.: in her *Working Women in English Society, 1300-1600* (Cambridge, UK: Cambridge University Press, 2005) a total of 92 books and 388 articles and book chapters are cited in their entirety.

³⁷ The record, as I’ve quoted it, clearly reveals yet another pair of falsehoods imbedded in the version of my exchange with the Committee offered by Prof. Radelet at p. 35 of the *Report*. These are, first, that he determined *on his own* that Steele’s *Warpaths* was among the sources upon which I’d relied in my 1997 essay, and, second, that I’d placed an equal reliance upon an “article by Snow and Lanphear.” While I did in fact cite the latter as an additional reference in

tation of fact resulted from mere “error” is thereby eliminated by the nature of the very record upon which he purportedly relied.

Equally clearly, I did *not*, contrary to Professor Wesson’s more recent claims, “disclaim...reliance upon Salisbury.” Quite the opposite, as the transcript confirms, I *repeatedly* stated that I viewed Salisbury as supporting my position even as I pointed out that I considered Steele to be “clearer” and therefore the “better source” for my purposes in 2003. It is also worth emphasizing that a few pages later in the April 1, 2006, transcript, Professor Radelet and I are recorded as discussing my *ongoing reliance* upon Salisbury in certain matters, a circumstance even more decisively disconfirming of Professor Wesson’s contention that I “disclaimed any reliance” on him.³⁸ In the end, the sheer consistency with which Professor Wesson reverses the facts contained in the very record she invokes serves to eliminate the possibility that her misrepresentation(s) result from mere “error.”

That Professor Radelet, Professor Wesson, and their coauthors, are guilty of falsification in this instance is unquestionable. Both the manner in which the initial falsification was undertaken—i.e., the withholding of evidence invoked to make the Committee’s misrepresentation *seem* credible—and the fact that it is still being actively defended on the same basis, lend an appearance that the falsifications at issue in the present Allegation are not isolated, but, rather, are elements of a broader and carefully-calculated pattern of research fraud. If there is anything good to be said of the situation, it is that Professor Wesson’s remarks about the Committee’s desire to correct its “error” for “the scholarly record” serves to retire any lingering doubts as to whether the *Report* “really” constitutes scholarship, per se, to which scholarly standards of assessment may be properly applied.

Allegation 4: Falsification/Fabrication (re, prior incidence of epidemic disease among the Wampanoags)

At page 35 of the *Report*, Professor Radelet twice quotes a sentence from page 169 of my 1997 essay “Nits Make Lice,” wherein I observe that “the Indians had had close contact with Europeans for years without getting sick—epidemics broke out in the aftermath of Smith’s expedition.” He neglects to mention, however, that the sentence quoted was devoted explicitly to the Wampanoags—indeed, it is predicated by reference to the “Patuxet Wampanoags and Nausets” in the preceding sentence—a matter making it rather clear who was at issue when I then employed more general term “Indians.”³⁹ Having thus glossed this rather crucial distinction, Professor Radelet sets about disproving this element of my “circumstantial evidence” against John Smith by observing that while it “is true that many Europeans visited southern New England before John Smith...*New England Indians* themselves believed that European contact had already had disastrous health effects [emphasis added].”⁴⁰

the 1997 essay, the Snow/Lanphear piece went altogether unmentioned during April 1, 2006, colloquy. I gave no indication whatsoever, as the transcript makes clear, of having I’d relied upon Snow/Lanphear to any appreciable extent, certainly not in a measure resembling—less still, *equaling*—that to which I’d relied upon *either* Steele or Salisbury. The article in question is Dean R. Snow and Kim M. Lanphear, “European Contact and the Depopulation of the Northeast: The Timing of the First Epidemics,” *Ethnohistory*, Vol. 35, No. 1 (Winter 1988) pp. 15-33.

³⁸ *Investigative Committee Transcript* (Apr. 1, 2006) pp. 162-3.

³⁹ “Nits Make Lice,” p. 169. In “An American Holocaust?” I refer simply—and exclusively—to “Wampanoags.” These, as Prof. Radelet acknowledged during our colloquy on Apr. 1, 2006, are the only two instances in my published writing where I’ve mentioned the 1616-20 epidemic(s). There can thus be no pretense that Prof. Radelet was “confused” as to *which* “Indians” were at issue. See *Investigative Committee Transcript* (Apr. 1, 2006) p. 153.

⁴⁰ *Report*, p. 36. It should be emphasized that during my colloquy with the Investigative Committee on Apr. 1, 2006, I pointed out the inappropriateness of Prof. Radelet’s inclination to conflate the particular (Wampanoags) with the general (i.e., “New England Indians” or “Indians of New England”) for analytical purposes. When he sought to challenge my depiction of the disease afflicting the Wampanoags by pointing to Salisbury’s diagnosis of bubonic plague, I re-

To illustrate, Professor Radelet observes that, “As early as 1610, one [Micmac, or Mi’kmaq, as the people in question prefer to spell it] leader told a French visitor ‘that in his youth his people had been as thickly planted there as the hairs upon his head,’ but that since the French had come their numbers had diminished radically under the impact of disease.”⁴¹ He then amplifies the point by noting that “Biard’s estimate of 3000-3500 Micmacs living in 1611 represents, according to Dean Snow’s estimate, about one-fourth of the population a century earlier” before summing up that “Professor Churchill’s assertion that ‘there is no disease epidemic occurring before 1616’ appears to be incorrect.”⁴²

Once again, the problems with Professor Radelet’s argument are both several and serious. First of all, the awkward formulation “disease epidemic” attributed to me in the last sentence, is *not* one I would have employed.⁴³ Although purportedly quoting me, Professor Radelet provides no reference as to where the language at issue supposedly appears in my writing. It will certainly not be found on any of the three pages of my text he was supposedly scrutinizing. Nor does a computer search turn it up, either in the broader essays wherein the scrutinized pages appear, or in any of the handful of other articles and essays wherein I’ve mentioned disease. It is thus a virtual certainty that Professor Radelet simply fabricated this “quotation” for effect, thereby engaging in a clearly-defined form of research misconduct.⁴⁴

Second, Professor Radelet’s substitution of the far more general category of “New England Indians” for my specific focus upon the Wampanoags, discussed in the opening paragraph of this Allegation, also appears to have been designed/intended to accomplish a particular effect, one that was not only useful but necessary to his argument. Even were he to be accorded a considerable benefit of the doubt regarding this substantial distortion of what I actually written—i.e., allowing him once again to fall back on the feeble excuse that he suffered a truly woeful ignorance of the subject-matter he claimed to be “summing up”⁴⁵—the same latitude cannot be extended to his having then proceeded almost immediately to substitute Indians from *outside* New England for the “New England Indians” with which he’d already replaced the Wampanoags. So, too, those who endorsed his “careful analysis.” There are, after all, limits to any shell game, and this last maneuver far exceeded any within which the Investigative Committee must be seen as having been legitimately constrained.

No discernable conversance with American Indian subject matters is required to find grounds for pause in the notion that the impact of *the French* might be principally at issue in a region referred to as *New England*.⁴⁶ The sheer incongruity of such a proposition might be reasonably ex-

joined that he was “talking about all of New England now,” which might well have been hit by a “whole series of epidemics [including] “the plague and chickenpox and smallpox” between 1616 and 1620, but my focus was specifically upon the Wampanoags; *Investigative Committee Transcript* (Apr. 1, 2006) pp. 162-3.

⁴¹ Ibid. Prof. Radelet cites Salisbury, *Manitou and Providence*, p. 57. Although Prof. Radelet identifies neither the “Indian leader” or the “French visitor,” according to Salisbury they were Membertou, a Micmac, and Marc Lescarbot.

⁴² *Report*, p. 36; citing Salisbury, *Manitou and Providence*, p. 57.

⁴³ I do sometimes refer to “epidemic diseases,” but never to “disease epidemics.” Frankly, I consider the latter phrasing to be not only awkward. but a redundancy.

⁴⁴ To adapt a rather colorful turn of the phrase recently attributed to Prof. Radelet in the *Silver and Gold Record*, “if he can cite the source of the ‘quote’ he attributes to me, I’ll eat it at the 50 yard line.” See Dodge, “Debate over Churchill case persists.”

⁴⁵ Actually, in view of the exchange between Prof. Radelet and I discussed in Notes 39 and 40, above, such an excuse would be rather implausible in the connection at hand. It follows that whatever ignorance may have underpinned Prof. Radelet’s polemically useful conflation, it would seem more “willful” than “woeful.”

⁴⁶ There is simply no basis in the literature to support Prof. Radelet’s contention, advanced in our verbal exchange during the investigation, that “most people” feel “the weight of evidence suggests” that the 1616-20 epidemic was sparked by “the French”; *Investigative Committee Transcript* (Apr. 1, 2006) p. 175. As will be demonstrated in Allegation 3,

pected to have prompt any responsible scholar—*especially* those otherwise ill-versed in the subject-matter—to inquire further. It would be prudent, for instance, to ascertain where, given their obvious proximity to Frenchmen, the Mi’kmaq were actually situated before deploying them, as Professor Radelet did, as one’s sole illustration of what the “New England Indians themselves believed...as early as 1610” (all the more so when one has also narrowed the geographical parameters under consideration to “*southern* New England [emphasis added]”).

No strenuous research is necessary to acquire the necessary information. Indeed, it will be found in a map appearing at pages 104-5 of Steele’s *Warpaths*, a copy of which I provided to the Investigative Committee upon request,⁴⁷ and with which Professor Radelet professes a certain familiarity at page 35n59 of the *Report*.⁴⁸ Or, since he cited Salisbury’s *Manitou and Providence* to support his remarks on the toll taken by disease upon the Mi’kmaq prior to 1616, and since it is stated therein that the events described centered on the French settlement at Port Royal,⁴⁹ Professor Radelet might have consulted the very source upon which he was otherwise relying to ascertain the town’s location. Had he done so—no more than a glance at the index was required—he’d have found Port Royal clearly identified as having been in the French Colony of Acadia (now the Canadian province of Nova Scotia).⁵⁰

This is to say that it—and the Mi’kmaq—were situated in what was then referred to as *New France* rather than New England. Turning once again to Steele’s map—or to Google—Professor Radelet would have discovered that the Mi’kmaq around Port Royal were literally hundreds of miles distant from *southern* New England, a portion of which comprises the sole geographical locus of those among *my* observations he was supposedly investigating.⁵¹ Thus, while the information he presents about the Mi’kmaq is accurate, is no more relevant to the circumstances of

below, this notion appears to be anchored in a very serious distortion of several sources cited by Salisbury, and, consequently, of Salisbury himself.

⁴⁷ The following exchange is recorded in the *Investigative Committee Transcript* (Apr. 1, 2006) at p. 156:

PROFESSOR CHURCHILL [identifying a source]: Ian Steele, a book called *Warpaths*.

DR. MCINTOSH: This is what you’ll be giving us on Monday, right?

PROFESSOR CHURCHILL: Yeah...

⁴⁸ The same information is readily available in any number other sources, both scholarly and popular. See as examples, Carl Waldman, *Atlas of the American Indian* (New York: Facts on File, 1985) p. 32; Richard Collins, ed., *The Native Americans: Indigenous People of North America* (New York: Smithmark, 1991) p. 224; Frederick E. Hoxie, ed., *Encyclopedia of North American Indians: Native American History, Culture, and Life from the Paleo-Indians to the Present Time* (New York: Houghton Mifflin, 1996) p. 285.

⁴⁹ “[T]he local Micmac under Membertou became the dominant power among Indians trading with the French because of their proximity to Port Royal”; Salisbury, *Manitou and Providence*, p. 57. This, it should be emphasized, is the very page cited by Prof. Radelet in support of his remarks; see *Report*, p. 36n64.

⁵⁰ This requires no more than a glance at the book’s index. The entry, which appears at p. 311, reads “Port Royal, Acadia.” The location of Port Royal is also clearly marked on the map appearing in Steele’s *Warpaths* at pp. 104-5. A copy of Steele’s map is attached hereto as Exhibit 1A, while a pair of maps taken from the relevant volume of Smithsonian *Handbook of North American Indians*—another presumably “relevant” source—appear as Exhibits 1B and 1C.

⁵¹ The distance separating the Wampanoag homeland around Cape Cod from the Canadian city of Annapolis, Nova Scotia—as Port Royal, Acadia is now called—is well over 400 miles. Yet, at p. 36 of the *Report*, Prof. Radelet deploys Mi’kmaq examples from Acadia to “illustrate” circumstances in Wampanoag territory, which he himself describes as being in “*southern* New England [emphasis added].” To this should be added the fact that at p. 23 of a written submission titled “Responses Left Unfinished Because of Time Constraints Imposed by the Committee,” I emphasized that, far from being concerned with New England as a whole—less still New France—mine was a “much narrower focus” upon the Wampanoags in/around what in 1620 became the “Plymouth Plantation” (i.e., the Massachusetts Bay/Cape Cod area). Receipt of this submission is acknowledged at p. 116 of the *Report*. All this being so, Prof. Radelet’s conflation of peoples/regions can hardly have been “accidental.”

the *Wampanoags* than would be equally accurate information on pre-1616 outbreaks of disease among the Indians in Jamestown or Spanish Florida.

Actually, it may well be that Professor Radelet's "Mi'kmaq example" is irrelevant to New England as a whole. Indeed, he appears to have selected it for the simple reason that it is the sole instance he could find of disease being at play in an area proximate to *some* portion of New England—albeit, the extreme north—prior to 1616. Salisbury, his source, discusses none in New England proper. Nor do such authorities as Stearn and Stearn,⁵² Ashburn,⁵³ Vaughan,⁵⁴ Salwin,⁵⁵ Kupperman,⁵⁶ Cronon,⁵⁷ Malone,⁵⁸ Morgan,⁵⁹ Steele,⁶⁰ Russell,⁶¹ Dobyns,⁶² Thornton,⁶³

⁵² The New England epidemic(s) beginning in 1616 was "the first great Indian plague of North America on record for the seventeenth century"; E. Wagner Stearn and Allen E. Stearn, *The Effect of Smallpox on the Destiny of the Amerindian* (Boston: Bruce Humphries, 1945) p. 21.

⁵³ The earliest disease in New England referenced by Ashburn is "the epidemic that destroyed the Indians around Plymouth before the Pilgrims landed," i.e.: the 1616-20 epidemic(s); P.M. Ashburn *The Ranks of Death: A Medical History of the Conquest of America* (New York: Coward McCann, 1947) p. 22.

⁵⁴ The first incidence of disease among the peoples of New England mentioned by Vaughan is dated 1616-1617; Alden T. Vaughan, *The New England Frontier: Puritans and Indians, 1620-1675* (New York: W.W. Norton, [2nd ed.] 1976) pp. xxviii, 21.

⁵⁵ While he records their being visited by Giovanni da Verrazano (1524), Bartholomew Gosnold (1602), Martin Pring (1603), Samuel de Champlain (1605, 1606), Henry Hudson (1609), Edward Harlow (1611), and Nicholas Hobson (1611), among others, the first disease mentioned by Salwin as afflicting the New England peoples is the "epidemic [or "epidemics"] of 1617-1619"; Bert Salwin, "Indians of Southern New England and Long Island: Early Period," in Bruce G. Trigger, ed., *Handbook of the North American Indians, Vol. 15: Northeast* (Washington, D.C.: Smithsonian Institution, 1978) pp. 170-1.

⁵⁶ The earliest incidence of epidemic disease in New England mentioned by Kupperman is "the great wave [of] 1616"; *Indians and English*, p. 34. Although Kupperman does not address the question so directly in her *Settling with the Indians: The Meeting of English and Indian Cultures in America, 1580-1640* (London: Dent, 1980), nothing therein contradicts such a dating.

⁵⁷ "The first recorded epidemic in the south began in 1616 and raged for three years on the coast between Cape Cod and Penobscot Bay, reaching villages twenty or thirty miles inland but sparing both the deeper interior and coast west of Narragansett Bay"; William Cronon, *Changes in the Land: Indians, Colonists, and the Ecology of New England* (New York: Hill and Wang, 1983) p. 87.

⁵⁸ While he, like Salwin (see Note 55, above), discusses visits to the area by Verrazano, Gosnold, and Pring—as well as George Weymouth (1604)—Malone's first mention of disease reads: "About 1616, a European visitor brought a...disastrous plague...to the coast of New England"; Patrick M. Malone, *The Skulking Way of War: Technology and Tactics Among the New England Indians* (Baltimore: Johns Hopkins University Press, 1991) pp. 33-5.

⁵⁹ While he discusses an exploration of the coast from Maine to Cape Cod by Champlain in 1605, the first disease mentioned by Morgan as afflicting the peoples of New England is described thus: "Several years before the Pilgrims arrived on the same coast in 1620, many of these Indians had died in an epidemic, making it possible for the Indians to settle there"; Ted Morgan, *Wilderness at Dawn: The Settling of the North American Continent* (New York: Simon & Schuster, 1993) pp. 92-3.

⁶⁰ Steele in fact points out that the epidemic(s) beginning in 1616 did *not* reach "the more dispersed Micmac hunters to the north" of New England; Steele, *Warpaths*, p. 84.

⁶¹ Russell dates the first outbreak(s) of disease among the New England peoples as occurring between John Smith's 1614 presence and the arrival of the Plymouth colonists in 1620; Howard S. Russell, *Indian New England Before the Mayflower* (Hanover, NH: University Press of New England, 1980) pp. 10-1.

⁶² In a chapter titled "The Widowing of the Land," Dobyns presents what may be the most comprehensive chronology of epidemics afflicting North American Indians, broken out under headings for smallpox, measles, bubonic plague, influenza, diphtheria, typhus, cholera, and scarlet fever. He records no outbreaks of any of the diseases listed among the peoples of New England prior to 1616; Henry F. Dobyns, *Their Number Become Thinned: Native American Population Dynamics in Eastern North America* (Knoxville: University of Tennessee Press, 1983) pp. 8-22. At p. 23, does list an epidemic of an "unknown" type as afflicting unspecified "New England tribes" between 1564 and 1570. As is revealed at pp. 314-20, the only indication that this epidemic—which he suggests was measles—may have occurred derives

Watts,⁶⁴ and Jones.⁶⁵ It is also worth emphasizing that none of the latter sources—e.g., Thornton, who is also cited by Professor Radelet (at page 37n70 of the *Report*)⁶⁶—suggest that the French played any role whatsoever in sparking the 1616-20 epidemic(s).⁶⁷

In sum, and contrary to his assertion(s), Professor Radelet actually presented *nothing* which might be properly construed as “disconfirming” my work. What the Mi’kmaq may already have suffered and therefore did/didn’t believe as early as 1610 is no more relevant to the situation/beliefs of the *Wampanoags*—and perhaps the “New England Indians” more generally—than are the experiences/resulting beliefs at that point of the Timucuan and Guales in Spanish Florida, or those of Shawnees in the Ohio River Valley a century later. In order to cast an aura of validity over what was apparently a preordained conclusion, Professor Radelet engaged in—and his coauthors endorsed—serial misrepresentations of fact, systematically conflating geographic areas, colonizing powers, and indigenous peoples as a means of fostering the desired impression. This adds up to falsification of the most egregious sort, the more so in view of the fact that the entire exercise was couched in terms of enforcing “scholarly integrity.”

Allegation 5: Falsification (re, evidence of smallpox)

At page 37 of the *Report*, Professor Radelet asserts that, “The Committee’s reading of relevant literature indicates that there is no clear evidence about the exact nature of the [1616-20] epidemic and nothing that points specifically to smallpox. Professor Churchill does not provide even ‘circumstantial evidence’ to support his claim that the disease was smallpox or tell his readers by what logic he reached this conclusion.” Quite apart from such obvious ambiguities as how much “reading” he and his collaborators actually did, what literature they might have considered “relevant,” and what they might have deemed to be “clear evidence,” Professor Radelet’s asser-

from a single letter, written in 1634, wherein colonist Roger Williams interpreted a Narragansett oral history pertaining to earthquakes; John Russell Bartlett, ed., *The Letters of Roger Williams* (Providence: Rhode Island Historical Society, 1874) p. 99. William Cronon, for one has rather unequivocally “reject[ed] this as evidence of earlier epidemics”; Cronon, *Changes in the Land*, p. 189n8. There is nothing in the *Report*, moreover, suggesting that Prof. Radelet and his coauthors were aware of this rather arcane debate, much less had in mind, when they advanced their finding that the “Indians of New England suffered from imported diseases prior to 1616.

⁶³ Thornton observes that the “extent to which the Indians of New England and New York had been depopulated before colonies were founded...is anybody’s guess,” but he follows Stearns and Stearns in dating the outbreak of the first colonially-associated epidemic to 1616 (see Note 52, above); Russell Thornton, *American Indian Holocaust and Survival: A Population History Since 1492* (Norman: University of Oklahoma Press, 1987) pp. 70-1.

⁶⁴ Although he mentions a smallpox epidemic reported by Jacques Cartier to have devastated the native population of Stadacona—in New France—as early as 1534, the first instance of disease in New England noted by Watts is that which “wiped out the Patuxet people just before the Pilgrims arrived at Plymouth in 1620”; Sheldon Watts, *Epidemics in History: Disease, Power and Imperialism* (New Haven, CT: Yale University Press, 1997) pp. 92-3.

⁶⁵ At p. 26 of his *Rationalizing Epidemics: Meanings and Uses of American Indian Mortality since 1600* (Cambridge, MA: Harvard University Press, 2004), medical historian David S. Jones dates the earliest epidemic(s) in New England from “the winter of 1616 and 1617.”

⁶⁶ Quite the contrary, at p. 71n8 of *American Indian Holocaust and Survival*, Thornton refers his readers to sources “discuss[ing] possible early English voyages to the New England area.”

⁶⁷ To be fair, it should be mentioned that, in a manner similar to Salisbury, Colin G. Calloway indicates that “the Indians of Maine and Nova Scotia were no strangers to disease as early as 1616,” and that this was likely the result of trading with the French; Colin G. Calloway, “Introduction: Dawnland Frontiers,” in Colin G. Calloway, ed., *Dawnland Encounters: Indians and Europeans in Northern New England* (Hanover, NH: University Press of New England, 1991) p. 12. At p. 49, he also mentions that Fr. Biard reported that some Indians along the Penobscot River, in Maine, had been “stricken by disease” in 1611 or ’12. Maine, of course, is part of New England. Unlike Professor Radelet, however, Calloway is careful to distinguish the (far) northern part of the region from the southern, and gives no indication that disease was at large among peoples in the latter area prior to 1616.

tion seems altogether peculiar, given that at page 11 of the *Report* the Investigative Committee as a whole had clearly acknowledged that “detailed references are not required in broad accounts of the kind Professor Churchill has written,”⁶⁸ and, at page 12, that it would therefore be assumed that I “may have been drawing upon sources I did not cite specifically” in any given instance.⁶⁹

The questions we have asked are whether there is any reliable basis upon which a reasonable scholar could have come to the conclusion[s] Professor Churchill presented, even if he did not cite it, and, if so, whether Professor Churchill relied on these sources in writing his essays.⁷⁰

Thus framed, the question before Professor Radelet was *never* whether I’d initially deployed sufficient evidence to sustain every point made in my “broad accounts” under the most intensive sort scrutiny, but whether such evidence exists. The Committee claimed, moreover, to have in no sense restricted itself to considering evidence I myself provided, but, rather, “undertook its own examination of the relevant primary sources and both oral and written traditions,” examining “the sources in some detail in order to assess whether there is an evidentiary basis for [my] claims.”⁷¹ Such an inquiry, it was further asserted, “required a considerable amount of research on the topics in question” by individual Committee members.⁷² For his part, Professor Radelet, during his testimony before the P&T review panel on January 21, 2007, described in some detail the extent of the research he supposedly conducted at UCB’s Norlin Library during the spring of 2006.⁷³

This forms an appropriate backdrop against which to evaluate one of Professor Radelet’s own contentions, twice repeated: 1) that “nothing [in the] relevant literature points specifically to smallpox” as having been the disease that decimated the Wampanoags, *circa* 1616-20, and, 2) that “no evidence—not even circumstantial evidence—supports [my] claim” in this regard.⁷⁴ Such contentions are quite astonishing, given that as a means of confusing the issue, Professor Radelet quotes Russell Thornton’s observation, advanced on page 71 of his *American Indian Holocaust and Survival*, that, “The nature of the epidemic has also been debated. According to different scholars, it was perhaps bubonic plague or yellow fever; it may have been smallpox.”⁷⁵ Professor Radelet leaves unmentioned the fact that in his *very next sentence*, Thornton recounts

⁶⁸ “Because the essays [at issue in the investigation] offer broad accounts, extensive notes would not be expected or required”; *Report*, p. 10.

⁶⁹ In his testimony before the P&T Review Panel, Prof. Radelet claimed—falsely—that the proviso concerning detailed references not being required pertained only to my “polemics” rather than work I “submitted as academic scholarship,” observing that I myself had “made the distinction.” My professional CV does indeed contain separate sections listing publications I classify as “Polemics,” as well as “Scholarly Essays (Refereed)” and “Scholarly Essays (Unrefereed).” The problem with Prof. Radelet’s argument is that the great bulk of the material scrutinized by the Investigative Committee has never been listed under either of the “scholarly” headings. His testimony was thus false on a second count. See *In Re: Dismissal for Cause Hearing for Professor Ward Churchill* (hereinafter referenced as *P&T Review Transcript*), transcript for Jan. 21, 2007. pp. 2133-5.

⁷⁰ *Report*, p. 12.

⁷¹ *Ibid.*

⁷² *Ibid.*

⁷³ “You know, I remember doing a subject index at Norlin and pulling up John Smith, which is not a very easy subject, ‘cause I wanted *the* John Smith, not every other John Smith since Adam and Eve [and] I...got a pile of these books, probably, you know, a dozen books, and went through just to see if...there was other stuff there that was citing these accusation. And I could find none”; Radelet testimony, *P&T Review Transcript* (Jan. 21, 2007) p. 2127. Also see the *Investigative Committee Transcript* (Feb. 18, 2006) at p. 81.

⁷⁴ *Report*, pp. 37, 38.

⁷⁵ *Ibid.*, p. 37.

how, “Donald R. Hopkins stated that smallpox from 1617-1619 ‘wiped out nine-tenths of the Indian population along the Massachusetts coast.’”⁷⁶

Similarly, Professor Radelet neglects to mention that in his *Rotting Face: Smallpox and the American Indian*—another source discussed and cited several times in the *Report*⁷⁷—R.G. Robertson notes that “Shurkin...insists the epidemic of 1617 through 1619 was smallpox.”⁷⁸ Not only do these passages appear in the Committee’s own sources, but the books at issue—Hopkins’ *Princes and Peasants: Smallpox in History*, and Joel Shurkin’s *Invisible Fire: The Story of Mankind’s Victory Over the Ancient Scourge of Smallpox*—are generally viewed as being standard references on the topic.⁷⁹ Even a cursory survey reveals, moreover, that the Hopkins/Shurkin diagnosis is quite consistent with that most often found in what must by any reasonable estimation be considered the most “relevant” literature published during the twentieth century.

Examples begin at least as early as 1909, with H.U. Williams’ seminal essay, “The Epidemic of the Indians of New England, 1616-1620,” in which the author’s conclusions were based first-hand observations by the English sea captain, Thomas Dermer.⁸⁰ Still more emphatic was S.B. Woodward’s 1932 article, “The Story of Smallpox in Massachusetts,” published in the *New England Journal of Medicine*, in which it is declaimed that “neither malaria, nor yellow fever, nor the plague, but smallpox was the blessing in disguise that gave our emigrant ancestors an opportunity to found the State.”⁸¹ A slightly qualified, but nonetheless concurring, opinion was offered fifteen years later by P.M. Ashburn, in his benchmark “medical history of the conquest of America.”

The settlement at Plymouth...might well have been impossible had the Indians been capable of resisting it.... Governor Bradford wrote that the were “not many, being dead and abundantly wasted in the late mortality which fell in all these Parts about three years before the coming of the English; wherein thousands of them dyed.” This plague was probably smallpox in 1617, as Captain Dermer in a letter to Reverend Samuel Purchas wrote of “The sores of some that had escaped, who described the Spots of such as usually die.”⁸²

⁷⁶ Thornton, *American Indian Holocaust and Survival*, p. 71; citing Donald R. Hopkins, *Princes and Peasants: Smallpox in History* (Chicago: University of Chicago Press, 1983) p. 234.

⁷⁷ For discussion, see *Report*, p. 76; for citations, see Notes 157, 158, 191 196, 197, 199.

⁷⁸ R.G. Robertson, *Rotting Face: Smallpox and the American Indian* (Caldwell, ID: Caxton Press, 2001) p. 133n18; citing Shurkin, *Invisible Fire*, p. 146.

⁷⁹ Certainly, the Hopkins and Shurkin studies are far more “standard” references for non-medical practitioners than the “standard compendium” offered by Prof. Radelet at p. 34n54 of the *Report*, i.e.: “Frank Fenner, D.A. Henderson. Et al., *Smallpox and Its Eradication* (Geneva: World Health Organization, 1988).”

⁸⁰ Herbert U. Williams, “The Epidemic of the Indians of New England, 1616-1620, with Remarks on Native American Infections” *Johns Hopkins Hospital Bulletin*, No. 20 (1909) esp. p. 348; cited by Hopkins. *Princes and Peasants*, at p. 234. As Hopkins observes, Williams rendered a split verdict; given the conflicting symptoms reported, he concluded that the disease was either plague or smallpox. It does not appear to have occurred to either Williams or Hopkins that there might have been more than one epidemic, featuring more than one disease, going on at the same time. See Note 101, below.

⁸¹ S.B. Woodward, “The Story of Smallpox in Massachusetts,” *New England Journal of Medicine*, No. 206 (1932) p. 1182; quoted in Hopkins, *Princes and Peasants*, at pp. 234-5.

⁸² Ashburn, *The Ranks of Death*, p. 88; citing Williams, “Epidemic of the Indians of New England.” A still more qualified verdict had been offered two years earlier by E. Wagner Stearn and Allen E. Stearn in their study, *The Effect of Smallpox on the Destiny of the Amerindian*. At p. 21, they observe that while “[m]uch has been written to show that this epidemic...was not smallpox,” the mere facts that “white settlers were not attacked and did not call it smallpox cannot be called proof positive, since in the records of epidemics we find similar instances.” While they ultimately adopt the “safe” posture of leaving “the nature of the malady...unidentified,” it is plain by virtue of their having discussed it in a book devoted exclusively to smallpox that they themselves are inclined to diagnose “the malady” as such. It should be noted, moreover, that there were no “white settlers” in New England to be infected, *circa* 1616. Rendering the “not pox” argument still less credible is the fact that in 1633-34, when the Indians were unquestionably beset by a devastating smallpox epidemic, with many settlers close at hand, the latter were not “attacked” by the disease. Indeed, as the

Plainly, both Captain Dremer’s eyewitness description of the symptoms of disease afflicting the Wampanoags—i.e., the *only* Indians directly associated with “Plymouth”⁸³—in 1617, and Dr. Ashburn’s resulting characterization of it, contradict Salisbury’s rather misleading assertion, quoted approvingly and all but exclusively by Professor Radelet, that “[a]ttempts by medical historians to diagnose the malady have floundered on the inconclusive nature of surviving descriptions. The only first-hand European witnesses whose observations survive...referred to the disease as simply ‘the plague’...”⁸⁴ The symptoms of “the plague” were, after all—according to historian James Wilson—described rather graphically by William Bradford, Governor of the Plymouth Plantation. These included:

...pox breaking and mattering and running one into another, [the] skin cleaving by reason thereof to the mats [the victims] lie on. When they turn them, a whole side will flay off at once as it were, and they will be all of a gore blood, most fearful to behold. And then being very sore, what with cold and other distempers, they die like rotten sheep.⁸⁵

The obvious conclusion has been duly drawn from such descriptions. In his 1991 secondary school text, *Smallpox and the American Indian*, Arthur Diamond attributes the loss of “90 percent of the Indian population...along the Massachusetts coast...between 1616 and 1619” to the disease.⁸⁶ Much the same formulation appears in numerous college-level texts and nonfiction works written for the general public. “A smallpox epidemic in 1617-1617...killed enough Natives to eliminate them as a major threat to Plymouth Colony after 1620,” according to an essay included in a widely-assigned Greenwood survey collection coedited by John E. Finding and Frank W. Thackery.⁸⁷ “In 1616-1619, a smallpox epidemic cut down almost nine-tenths of the Indian population of the Massachusetts Bay area,” adds Jonathan B. Tucker, author of a recent study of this “once and future threat.”⁸⁸

While the preceding quotes are specific to the Wampanoags, a number of standard sources indicate that smallpox was the disease afflicting *all* of the “New England Indians” decimated by the epidemic(s) of 1616-20. A map captioned “Epidemics Among Indians” in Carl Waldman’s

first Governor of the Massachusetts Bay Colony reported in 1634, while “‘the natives [were by then] near all dead of the smallpox,’...the Puritan settlers, numbering at the time ‘in all about four thousand souls and upward’ were in remarkably good health: ‘through the Lord’s special providence...there have not died above two or three grown persons and about so many children all last year, it being very rare to hear of any sick of agues and other diseases’”; David E. Stannard, *American Holocaust: Columbus and the Conquest of the New World* (New York: Oxford University Press, 1992) p. 109; quoting George Percy, “A Trewe Relacyon of the Proceedings and Occurrentes of Moment which have hapned in Virginia,” *Tyler’s Quarterly Historical and Genealogical Magazine*, No. 3 (1922) pp. 272-3.

⁸³ There is nothing in the least ambiguous about this. The location of “Plymouth” was originally—and quite literally—the Wampanoag village of Patuxet, John Smith having effected the renaming on maps he prepared in 1614. Both the Puritan town of Plymouth and the surrounding “Plantation” over which it was chartered to preside were thus explicitly intended from the outset to be situated exclusively on Wampanoag land; Salisbury, *Manitou and Providence*, p. 109.

⁸⁴ Salisbury, *Manitou and Providence*, pp. 101-2; quoted by Prof. Radelet at p. 37 of the *Report*.

⁸⁵ William Bradford, *History of Plymouth Plantation, 1606-1646* (New York: Scribner’s, 1908) p. 312; quoted in James Wilson, *The Earth Shall Weep: A History of Native America* (New York: Grove Press, 1998) p. 75. Wilson seems to suggest that Bradford was describing the effects of the 1616-20 “plague,” although my own understanding has always been that the description pertained to the smallpox epidemic commencing in 1633. Most likely—although it is less than clear in his handling—Wilson meant simply to indicate that the symptoms manifested during the 1633-34 smallpox epidemic would have been identical to those attending the 1616-20 “plague” because the Indians were afflicted with the same disease in both instances.

⁸⁶ Arthur Diamond, *Smallpox and the American Indian* (San Diego: Lucent, 1991) p. 20.

⁸⁷ John E. Finding and Frank W. Thackery, *Events That Changed America Through the Seventeenth Century* (Westport, CT: Greenwood Press, 2000) p. 75.

⁸⁸ Jonathan B. Tucker, *Scourge: The Once and Future Threat of Smallpox* (New York: Atlantic Monthly, 2001) p. 11.

Atlas of the North American Indian, for example, depicts smallpox—and *only* smallpox—as having raged through the region during those years.⁸⁹ Similarly, in a chronology included by Judith Nies in her *Native American History*, the entry for 1616-1619 states that, “A smallpox epidemic ravaged coastal New England from Massachusetts to Maine, killing off entire tribes.”⁹⁰ “New England Indians, from Massachusetts to Maine, suffered a smallpox epidemic from 1616-1619,” echoes Sana Loue in another widely-adopted college text.⁹¹ The entry for 1616 in the timeline provided in still another, Kathleen Tracy’s *Plymouth Colony*, reads simply, “Smallpox decimates New England Indians.”⁹² The list of such references could be extended at considerable length.⁹³

Noted historians like Angie Debo have often been little more equivocal, as is witnessed by her 1970 observation that the 1616-20 “plague” was “probably smallpox brought on English ships.”⁹⁴ Alvin M. Josephy, Jr., offered a similar assessment in 1982, recounting how, during “the winter of 1616-1617, a devastating epidemic, probably smallpox, killed 50 to 70 percent of the population of many of New England’s coastal Indian groups, striking particularly hard from eastern Massachusetts northward.”⁹⁵ Colin G. Galloway, in his *Dawnland Encounters*, appears at first to be somewhat undecided in his diagnosis, cautiously announcing that, “A terrible epidemic of smallpox or plague devastated the New England tribes between 1616 and 1619.” In his next sentence, however, he clearly reveals which of the diseases he believes it was, observing that, “*Smallpox hit again* in 1633-1634 and in 1639...[emphasis added].”⁹⁶

R.G. Robertson, in his more recent study, adopts much the same pose as Calloway, first acknowledging that “whether [the] disease was smallpox or plague is unclear,” but then commenting only a few sentence later—with specific reference to the Wampanoags—upon how English colonists simply took over “Indian fields made empty by smallpox.”⁹⁷ Once again, the offering of comparable examples might be considerably extended.⁹⁸ For purposes of brevity, however, I will perhaps be forgiven for simply concurring with Sheldon Watts’ assessment in *Epidemics in His-*

⁸⁹ The entry reads simply “Smallpox 1616-20”; Waldman, *Atlas of the North American Indians*, p. 166, Table 6.1.

⁹⁰ Judith Nies, *Native American History* (New York: Ballantine, 1996) p. 123.

⁹¹ Sana Loue, *Gender, Ethnicity, and Health Research* (New York: Kluwer Academic, 1999) p. 136.

⁹² Kathleen Tracy, *Plymouth Colony: The Pilgrims Settle in New England* (Hockessin, DE: Mitchell Lane, 2006) p. 45.

⁹³ A far from exhaustive roster of additional examples includes Sheldon Watts, *Epidemics and History: Disease, Power and Imperialism* (New Haven, CT: Yale University Press, 1997) pp. 92-3; David Lea, Colette Milward, and Anna Marie Rowe, *A Political Chronology of the Americas* (London: Europa, 2001) p. 225; Emory Elliot, *The Cambridge Introduction to Early American Literature* (Cambridge, UK: Cambridge University Press, 2002) p. 37; Randy Peffer, Kim Grant, Andrew Rebold, and John Spelman, *New England* (Oakland, CA: Lonely Planet, [3rd ed.] 2002) p. 13; John F. Richards, *The Unending Frontier: An Environmental History of the Early Modern World* (Berkeley: University of California Press, 2003) p. 503; Larissa Juliet Taylor, ed., *Great Events in History: The 17th Century* (Pasadena, CA: Salem Press, 2005) front matter; William Marder, *Indians in the Americas: The Untold Story* (San Diego, CA: Book Tree, 2005) p. 91; John W. Wright, ed., *The New York Times Almanac, 2007: The Almanac of Record* (New York: Penguin, 2006) p. 78; Paul Karr, Herbert Bailey Livesey, Marie Morris, and Laura M. Reckford, *Frommer’s New England* (New York: John Wiley, 2006) back matter.

⁹⁴ Angie Debo, *A History of the Indians of the United States* (Norman: University of Oklahoma Press, 1970) p.

⁹⁵ Alvin M. Josephy, Jr., *Now That the Buffalo’s Gone: A Study of Today’s Indians* (New York: Alfred A. Knopf, 1982) pp. 42-3.

⁹⁶ Calloway *Dawnland Encounters*, p. 12.

⁹⁷ Robertson, *Rotting Face*, p. 106.

⁹⁸ There is no better way to underscore this point than simply to quote a passage from an award-winning work of children’s fiction: “We [all] knew that Pilgrims had given the Native Americans blankets with smallpox on them”; Jacqueline Woodson, *The Dear One* (New York: Delacorte Press, 1991) p. 18. The implication, of course, is that something known by the average grade-schooler would be unlikely to come as a complete surprise to 5-of-5 professors forming the Committee.

tory, that many “scholars recognize as smallpox [the] epidemic disease which...wiped out the Patuxet people just before the Pilgrims arrived at Plymouth in 1620.”⁹⁹

It is true that, as Professor Radelet points out at page 37 of *Report*, in *Manitou and Providence* Salisbury argues that “the epidemic represents a strain of the plague” in a literal sense.¹⁰⁰ It is *not* true, however, that, as Professor Radelet also claims on the same page, “Salisbury cites three journal articles to support [the] sentence,” in which he says this.¹⁰¹ It is also true, although Professor Radelet seems unaware of it, that Howard S. Russell, among others, has claimed that “the infection that depopulated the Plymouth hillsides,” which was “formerly thought to have been smallpox,” has more recently come to be “thought perhaps [to have been] bubonic plague.”¹⁰² Strikingly, however, the note with which Russell attends his assertion makes an all but diametrically opposing case:

The depopulating disease was formerly thought to have been yellow fever (surviving Indians said the bodies turned yellow). Recent studies rule this out and suggest smallpox, chicken pox, or influenza, caught from sailors carrying germs to which their own systems were immune. A recent article concludes: “The best definition is probably that of [Plymouth’s governor William] Bradford—an infectious fever.”¹⁰³

Like Salisbury, Russell cited nothing upon which to anchor his preferred diagnosis, to say nothing of validating his insinuation that it was widely shared by other scholars.¹⁰⁴ While Professor Radelet may have overlooked Russell’s contribution to the debate, the record of our colloquy with on April 1, 2006, provides strong indication that he was not only familiar with but embraced R.G. Robertson’s even more extravagant claim that “consensus places the blame [for the 1616-20 epidemic] on either bubonic or pneumonic plague.”¹⁰⁵

⁹⁹ Watts, *Epidemics in History*, pp. 92-3.

¹⁰⁰ Salisbury, *Manitou and Providence*, p. 102.

¹⁰¹ Prof. Radelet cites p. 267n33 in *Manitou and Providence*. As was indicated in Notes 80 and 82, above, one of the articles cited by Salisbury in this connection—Williams’ “Epidemic of the Indians of New England”—actually shares Ashburn’s diagnosis of smallpox, and is cited by Hopkins for the same reason. Williams does, however, discuss the fact that the available evidence concerning the “New England Epidemic of 1616-20” is confusing in that, overall, it does not conform to the characteristics associated with *any* given disease. So, too, the second source cited by Salisbury, Sherburne F. Cook’s “The Significance of Disease in the Extinction of the New England Indians,” *Human Biology*, No. 45 (1973) pp. 485-508. This lays a groundwork for speculation, advanced in the third source cited by Salisbury, that rather than “an epidemic,” a whole *series* epidemics—involving several diseases, appearing at different times and afflicting different peoples—ran their respective courses in various parts of New England between 1616 and 1620; see Alfred W. Crosby’s “God...Would Destroy Them, and Give Their Country to Another People...,” *American Heritage*, Vol. 29, No. 6 (1978) pp. 38-43. Crosby’s premise, incorporated by Salisbury into his text at p. 101, is entirely consistent with the fact—unmentioned by Prof. Radelet—that Salisbury’s first reference in the note at issue is to John Smith’s contemporaneous observation that the peoples of New England had suffered “three plagues in three yeares successively.” For this, Salisbury cites Edward Arbur, ed., *Travels and Works of Captain John Smith*, 2 vols. (Edinburgh: 1910) Vol. 2, p. 933. In sum, Prof. Radelet has radically misrepresented his—or, rather, Salisbury’s—sources. Also see Notes 121 and 126, below.

¹⁰² Russell, *Indian New England*, p. 11.

¹⁰³ *Ibid.*, p. 217n6; quoting Billie Hoornbeck, “An Investigation into the Cause or Causes of the Epidemic Which Decimated the Indian Population of New England, 1616-1619,” *New Hampshire Archaeologist*, No. 19 (1976-77).

¹⁰⁴ Actually, in fairness, it should be mentioned that two of the sources cited by Salisbury at p. 267n33 of *Manitou and Providence*—Williams’ “Epidemic of the Indians of New England” and Cook’s “Significance of Disease”—do address the prospect that either bubonic or pneumonic plague was at work. Cook’s diagnosis is offered amidst speculation about multiple epidemics, however. On the Williams essay, see Note 80, above.

¹⁰⁵ Robertson, *Rotting Face*, p. 106.

DR. RADELET: [One question is] whether or not the disease was smallpox[. T]here are some who say smallpox, there are others, probably the majority, [who] would say some sort of plague, and most people would say, Hell, we don't know what it was.

PROFESSOR CHURCHILL: Well, the source I cite here...

DR. RADELET: Salisbury?

PROFESSOR CHURCHILL: Yeah... He thinks it was plague, or based on other people's post-mortems on pathology from descriptions of symptoms and such...he says it could have been plague. That's what a lot of people think. On the other hand, it could have been something else, and it could have been a whole series of epidemics. So it might have been the plague *and* chickenpox *and* smallpox. I just came across another [source saying the] Wampanoags died off of smallpox last night. So that's in the literature [emphasis added].

DR. RADELET: Yes, but most people probably say plague.

PROFESSOR CHURCHILL: I don't know [about that].¹⁰⁶

Insofar as Professor Radelet's approach to assessing the "smallpox question" thus appears to have been influenced to a considerable extent by Robertson's announcement of a scholarly "consensus" in behalf of the plague diagnosis, it seems appropriate to examine the basis for Robertson's assertion (and thus the quality of the material upon which Professor Radelet's critique may be said in no small part to hinge). Here, it is necessary only to turn to page 133n18 of *Rotting Face*, wherein Robertson contends that "Axtell, *Invasion Within*, 219-20; Hopkins, *Princes and Peasants*, 234; Josephy, *Indian Heritage*, 302; Calvin Martin, 'Wildlife Diseases as a Factor in the Depopulation of the North American Indian,' *The Western Historical Quarterly*, 7, no. 1 (January 1976) 50-1; and Ramenofsky, *Vectors of Death*, 99-100 [all] state that plague was the probable cause."

Certain problems should already be apparent, beginning with the fact that, as has already been discussed, Hopkins says precisely the opposite.¹⁰⁷ Similarly, Alvin Josephy's published view that the disease was "probably *smallpox* [emphasis added]" has already been quoted.¹⁰⁸ As for Axtell's *Invasion Within*, there is no mention of the 1616-20 epidemic on page 200, and all that appears on page 219 is the following:

[I]n 1616-18 a shipborne "plague" carried off between seventy and ninety percent of the coastal Massachusetts—including virtually the whole Patuxet tribe at Plymouth—and struck so many other natives that "they died on heapes, as they lay in their houses." Their unburied remains reminded one early settler of "a new found Golgotha."¹⁰⁹

¹⁰⁶ *Investigative Committee Transcript* (Apr. 1, 2006), pp. 162-3. It must be acknowledged that my contention that Prof. Radelet was here relying up Robertson—who remains uncited in the relevant section of the *Report*—is speculative. My reasoning is based on the facts that Salisbury—who serves as Prof. Radelet's main reference—makes *no* claim to "consensus" (or even that "most people" share his view), that such claims are equally absent from the other sources cited by Prof. Radelet, and that the Committee, including Prof. Radelet, relied upon Robertson's material at other points in the *Report* (see Note 77 and attendant text, above).

¹⁰⁷ See the text attended by Note 76, above. What Hopkins actually says on p. 234 of *Peasants and Princes* is: "Around 1617-19, a devastating epidemic wiped out nine-tenths of the Indian population on the Massachusetts coast. The cause of this epidemic, which was almost certainly a foreign disease introduced by Europeans, is disputed, but it appears to have been smallpox's debut in America north of Mexico." Further down the page, he discounts those favoring a diagnosis of plague, describing them as having been "misled" by the "outbreak's unusual virulence in the Indians."

¹⁰⁸ See Note 95 and attendant text, above.

¹⁰⁹ James Axtell, *The Invasion Within: The Contest of Cultures in Colonial North America* (New York: Oxford University Press, 1985), p. 219; citing Evarts B. Greene and Virginia D. Harrington, *American Population before the Federal Census of 1790* (New York: Columbia University Press, 1932) pp. 9, 13. It should be mentioned that, despite his lofty reputation as an "expert" on Indian/white relations during the colonial period, Axtell seems to have been almost as unversed in the subject-matter as were/are Prof. Radelet and his coauthors. He appears to have been completely un-

Given his enclosure of the word “plague” in quotes, Axtell plainly intended the word not to convey a specific medical diagnosis, but rather in its colloquial sense, i.e.: as a synonym for “pestilence,” that is, to describe any “disease that spreads very rapidly, infecting very large numbers of people and killing a great many of them, or an outbreak of such disease.”¹¹⁰ For her part, Ramenofsky offers no opinion on the matter at pages 99-100 (or elsewhere in her book). Instead, she points out that the late Sherburne Cook—who Robertson doesn’t cite—once “*suggested* that the 1616-1617 New England epidemic was plague [emphasis added],” and observes that “*if* the identification is correct [emphasis added],” certain things would follow.¹¹¹

Of Robertson’s five citations, this leaves only the article by Calvin Martin, which actually *does* posit “plague [as] the probable cause.” Needless to say, no mention is made in the *Report* of the overstated and misleading—some might even say “fraudulent”—nature of the claims of “consensus” advanced by Robertson (and, as was demonstrated above, by Professor Radelet himself).¹¹² Conversely, although they were informed of this in writing,¹¹³ Professor Radelet and his coauthors offer no hint that the “plague hypothesis” has been declared untenable by a number of scholars, notably former AHA President William Cronon:

The first recorded epidemic in [southern New England] began in 1616 and raged for three years on the coast between Cape Cod and Penobscot Bay... Although contemporary observers described it as “the plague,” New England lacked the rats and population densities necessary to sustain that disease. Chicken pox seems a more likely cause, since its virus requires only a small population to remain in circulation.¹¹⁴

It should be noted that while he rules out the plague altogether, there is nothing in the criterion advanced by Cronon to anchor his diagnosis of chickenpox that might serve to preclude

aware that the Patuxets were Wampanoags, not “coastal Massachusetts,” for instance, or that they were a village-based subgroup within a “tribe”—i.e., “people”—rather than being a “tribe” in their own right. Nor does he seem to have been aware that they took their name from the name of their village, which is to say that Patuxet was not “at Plymouth.” Rather, Patuxet *was* “Plymouth.” See Note 83, above.

¹¹⁰ The latter definition was as difficult to obtain as clicking the icon on my MS Word Thesaurus.

¹¹¹ Ann F. Ramenofsky, *Vectors of Death: The Archaeology of European Contact* (Albuquerque: University of New Mexico Press, 1987) pp. 99-100; citing Cook, “Significance of Disease.” It should be emphasized that Ramenofsky seems a bit skeptical of Cook’s suggestion, since she stresses that, “diffusion of the [plague] would depend on the migration of rats [and a]ll other recorded seventeenth-century epidemics were smallpox.” In support of the latter statement, she references “Stearn and Stearn 1945,” but without providing a pinpoint citation.

¹¹² Lest it be argued that to have “examined [Robertson’s] train of citations” in the manner undertaken here might be considered an unreasonably high expectation to be placed upon the Investigating Committee, it should be emphasized that they did exactly that when it served their purposes; see *Report*, p. 76n199 and attendant text. The only discernable difference between the matter at hand and that matter discussed on p. 76 of the *Report* is that the Committee’s accepting Robinson’s contention at face value worked against me in the present instance while, had they left the information at issue on p. 76 unchallenged, the result would have been in my favor. It should be mentioned that Prof. McIntosh, who by her own account traced/analyzed Robertson’s train of citations in the latter instance, was apparently unable to understand what was said in his sources, and was therefore categorically incorrect in her assessment. That, however, will be discussed in a separate complaint. For the moment, it is sufficient to observe that the bias evident in the juxtaposition of examples—signifying as it does the overall tenor of the investigation—does much to disconfirm the Committee’s pretence that it conducted “an information-seeking, nonadversarial proceeding”; *Report*, p. 9.

¹¹³ See my “Unfinished Responses,” p. 23-4.

¹¹⁴ Cronon, *Changes in the Land*, p. 87. Observe the similarity between Cronon’s objection to the plague diagnosis—i.e., the absence of rats in sufficient numbers—and the concern expressed by Ramenofsky vis-à-vis Sherburne Cook’s earlier “suggestion” that 1616-20 disease might have been plague (see Note 111, above). Also see the grounds for Noble David Cook’s rejection of both plague and typhus as tenable diagnoses in the quote attending Note 126, below. Suffice it to say that numerous other scholars share the view at issue.

smallpox—or measles, as Alden T. Vaughan, among others, would have it¹¹⁵—and he is otherwise silent on the matter.¹¹⁶ All of which serves to underscore the fact that, as medical historian David S. Jones, recalling earlier-quoted passages from both Salisbury and Thornton,¹¹⁷ has recently lamented, “the nature of the ‘plague’ remains unclear (smallpox? chicken pox? hepatitis?).”¹¹⁸ Such “ambiguity,” as Professor Radelet puts it at page 37 of the *Report*, will undoubtedly continue so long as scholars persist in trying to identify *the* disease causing an epidemic ravaging very nearly *all* of New England from 1616 to 1620.

As I explained to the Committee, both verbally and in writing, it was *this* issue that prompted me to cite Salisbury as a general reference in the only sketch I ever offered of the 1616-20 situation in New England.¹¹⁹ To be specific, at page 101 of *Manitou and Providence*—that is, *within* the range of my citation, but left conspicuously unmentioned by Professor Radelet in the *Report* (and Professor Wesson in her recent letter of “clarification”¹²⁰)—Salisbury explains that, “From 1616 through 1618 the Indians of New England were subjected to an epidemic, or *series of epidemics*, of catastrophic proportions [emphasis added].”¹²¹ The idea that there may well have been several epidemics involving *different* diseases during the crucial periods eliminates any need for the hopelessly contradictory array of symptoms exhibited by Indians in various quarters of New England to be reconciled with the effects of a particular pathogen, thereby cutting the diagnostic Gordian Knot that has flummoxed analysts for the past century or more.

While the idea did not originate with Salisbury—Vaughan surfaced it three years earlier in his *New England Frontier*¹²²—it was *Manitou and Providence* that seems to have given it “legs.” By the mid-90s, it was not uncommon to encounter descriptions in popular histories similar to that offered by James Wilson: “[From 1616-20,] an epidemic—or series of epidemics—of ‘the

¹¹⁵ Vaughan, *New England Frontier*, p. 22.

¹¹⁶ At p. 189n8 of *Changes in the Land*, Cronon cites Cook’s “Significance of Disease” in its entirety; Hoornbeck’s “Investigation into the Cause or Causes of the Epidemic,” pp. 35-46; the Samuel Elliot Morison edition of Bradford’s *Of Plymouth Plantation* (New York: Alfred A. Knopf, 1952) p. 87; and Charles Francis Adams’ 1897 *Three Episodes of Massachusetts History* (New York: Russell and Russell, 1965) pp. 1-12. None really explains how he arrived at his diagnostic conclusions.

¹¹⁷ See quotes attending Notes 75 and 84, above.

¹¹⁸ Jones, *Rationalizing Epidemics*, p. 29.

¹¹⁹ “Salisbury’s is perhaps the most sophisticated yet easily accessible treatment of the epidemic(s) that devastated the native peoples of New England from 1616-19”; “Unfinished Responses,” p. 23. For verbal explanation, see Note 106 and attendant text, above. The “sketch,” of course, appears at pp. 169-70 of my 1997 “Nits Make Lice,” which consists of a block quote from Steele, attended by 4 sentences and 2 footnotes.

¹²⁰ See Allegation 3. Observe that Prof. Wesson *still* insists on framing the matter strictly in terms of a “disease epidemic” rather than acknowledging Salisbury’s postulation of a “*series* of epidemics.” It stretches credulity to suggest that a senior law professor and former prosecutor like Prof. Wesson would simply remain oblivious to the significance of the distinction to the Committee’s case.

¹²¹ This sentence, as well as the one focused upon by Prof. Radelet—wherein Salisbury diagnoses “a strain of plague”—are attended by the same endnote (p. 267n33). For the reasons discussed in Note 101, above, it is a veritable certainty that Salisbury cited the three articles counted—but apparently not read—by Prof. Radelet primarily in support of the sentence quoted here rather than to support his diagnosis of plague.

¹²² Remarking upon the “vague and contradictory” nature of the available evidence, Vaughan observes that the epidemic “may have been measles, bubonic plague, or even, perhaps, a combination of diseases that hit various tribes simultaneously”; Vaughn, *New England Frontier*, p. 22; citing Bradford, *History of the Plymouth Plantation*, Vol. 1, p. 221n; Adams’ original *Three Episodes in Massachusetts History*, Vol. 1, p. 104; Oliver Wendell Holmes’ “The Medical Profession in Massachusetts,” in *Early History of Massachusetts* (Boston: Massachusetts Historical Society, 1869) pp. 260-1; and John Duffy, *Epidemics in Colonial America* (Baton Rouge: University of Louisiana Press, 1953) pp. 43, 141. Vaughan makes no reference to either Williams or Sherburne Cook (see Note 115 and attendant text, above), and Salisbury makes no reference to Vaughan in support of his own formulation.

plague' (perhaps smallpox or chickenpox)...swept through the coastal tribes [of New England], killing ninety percent or more of the population in some villages."¹²³ Or, to offer another example, Susan Hazen-Hammond's observation in her *Timelines of Native American History* that from "1615-1616[, a] five-year epidemic of smallpox, plague, and other diseases brought to the Americas by Europeans kill[ed] between 75 and 95 percent of the...Massachusetts, Wampanoag, and Pawtucket peoples."¹²⁴ And still another: "Mysteriously—the Indians had had close contact with Europeans for years without getting sick—*epidemics* broke out in the immediate aftermath of Smith's expedition [emphasis added]."¹²⁵

As should be readily apparent, my own framing—including my solitary reference in 2003 to the Wampanoags being afflicted with smallpox—have been in line not only with the diagnosis advanced by the preponderance of scholars over the past century, but also with the more refined analyses constituting current benchmarks in the relevant historical scholarship. As a final illustration of that fact, consider the following, drawn from Noble David Cook's 1998 study of "disease and New World conquest."

What was this illness, or series of sicknesses of 1616-20 that so severely converged on Amerindians of northeastern North America? Some have offered arguments to identify smallpox, others measles, bubonic plague, and even, incorrectly, yellow fever. Regardless of the exact element, the epidemic clearly extracted a large toll in human lives. Given the extremely deadly nature of the series, it would seem that the principal culprit was smallpox in various forms and with generally high levels of mortality. It may have coincided with measles, with age-specific mortality elevated to 20 percent. If the series had included bubonic plague or typhus, the English would also have faced noteworthy deaths and would have mentioned the fact...¹²⁶

With this state of the art elaboration firmly in mind, as well as the to all intents and purposes identical counterparts appearing in the three-dozen-odd quotes/cites I've deployed herein, it seems fitting to revisit the standard of assessment supposedly employed by the Investigative Committee, this time quoting the pertinent sentence in full.

Authors of overviews commonly make statements or present explanations on the basis of works they do not cite specifically: if their narratives and interpretations are *in accordance with established academic accounts*, detailed references are not considered necessary [emphasis added].¹²⁷

As has been abundantly demonstrated, the fact of the matter is that even if I *had* described the "1616-20 New England epidemic" *in its entirety* as having consisted *exclusively* of smallpox, rather than constraining my observation(s) to the "immediate area" of the Plymouth Plantation—i.e., to the Wampanoags—my characterization of the situation would *still* have been fully "in ac-

¹²³ Wilson, *The Earth Shall Weep*, p. 73.

¹²⁴ Susan Hazen-Hammond, *Timelines of Native American History* (New York: Perigree Books, 1997) p. 53. Hazen-Hammond is, so far as I'm aware, the only scholar to date the beginning of the epidemic(s) from 1615. Since she cites no sources in this regard, and offers no explanation, I've been unable to ascertain the basis for her dating.

¹²⁵ This quote is, of course, from my own "Nits Make Lice" at p. 169. The question of immediacy will be addressed in Allegation 7, below.

¹²⁶ Noble David Cook, *Born to Die: Disease and New World Conquest, 1492-1650* (Cambridge, UK: Cambridge University Press, 1998) pp. 172; citing Williams, "Epidemics among Indians." It is worth pointing out once again that the Williams piece is one of the 3 articles cited by Salisbury at p. 267n33 of *Manitou and Providence*, misrepresented by Prof. Radelet (see Notes 101 and 121, above). It should also be mentioned that Vaughan, too, has ruled out yellow fever, along with typhoid and jaundice; Vaughan, *New England Frontier*, p. 22. I can in fact find no indication that *any* epidemic of yellow fever has ever been recorded as occurring as far north as New England. Crosby, for example, reports it only with regard to "Amerindians in the tropics," observing that it also cost the British roughly 40,000 about troops "in the Caribbean theater" between 1793-96; Alfred W. Crosby, *Ecological Imperialism: The Biological Expansion of Europe, 900-1900* (Cambridge, UK: Cambridge University Press, 1986) p. 140.

¹²⁷ *Report*, p. 10.

cordance with established academic accounts.” That being so, I was obliged, as the Committee itself repeatedly acknowledged, I obliged to provide no documentation at all.¹²⁸ Certainly, I was under no obligation to provide more evidence than I did or otherwise “tell [my] readers by what logic [I] reached [the] conclusion” that smallpox was at issue, as Professor Radelet contends at page 37 of the *Report*. Plainly, the standard of assessment employed by Professor Radelet in this instance to arrive at an adverse finding directly contradicts the standard he and his colleagues claimed to be applying. Such misrepresentation constitutes a form of falsification.¹²⁹

Equally serious is the falsification—in this case it would be more accurate to describe it as an unvarnished lie—lodged in Professor Radelet’s assertion that there is “nothing pointing specifically to smallpox” in “the relevant literature.” As has been shown, such references are so abundant that it would be virtually impossible for even the most casual reader engaging in a survey of said literature to have missed them. This circumstance one of two possible explanations: either 1) Professor Radelet came across such evidence in the course of the research he claims to have done, but elected to actively deny its existence, or 2) Professor Radelet was consciously engaged in factual misrepresentation when he claimed to have performed any “research” at all. In either case, he is guilty of falsification.

While Professor Radelet’s handling of the “consensus” issue may not constitute research misconduct, per se—mainly because he did not bring it up in the *Report*—it certainly points to a substantial lapse in personal ethics. His related misrepresentation of the three articles cited by Salisbury, on the other hand, *does* add up to yet another instance in which is guilty of falsifying the

¹²⁸ *Report*, pp. 10, 11. Also Note 68 and attendant quote, above.

¹²⁹ The problem runs far deeper than it may appear. Although both my attorney, David Lane, and I began requesting clarification as to what standards were to be used even before the investigation commenced, the Investigative Committee never provided an answer. Witness the following excerpts from an exchange recorded in the *Investigative Committee Transcript* (Feb. 18, 2006) at pp. 72-80:

MR. LANE: I’m still not certain as to what...the standard of proof is, who the burden of proof is on.

PROFESSOR WESSON: ...[I]f you’re asking for something like jury instructions, we’re not in a position to tell you that right now. We’re still struggling with the questions of how to understand our mission and the precise relationship between historical truth, if it’s ascertainable at all, and the accusations of research misconduct... We don’t know the answers either, and we continue to discuss them, [and] when we reach the point of coming to our conclusions and documenting them, we’ll try to be as transparent as we can be about the standards we’re applying to this question....

PROFESSOR CHURCHILL: ...the larger point is that I’m making a case, [but] I don’t know where the bar is set until after the fact.

DR. MCINTOSH: That’s why we submitted seven precise questions, more precise questions about Allegation 3, and an additional question about Allegation 9.

PROFESSOR CHURCHILL: I understand the questions, but I don’t understand the burden of proof or what would be a satisfactory sort of response to the committee. That will [all] be determined after the fact. That’s the problem.

DR. MCINTOSH: ...[A]s Mimi has said, we’re feeling our way, too. The University’s guidelines do not refer specifically to some of the issues we’re dealing with. We have some rules that are very precise, other areas that are open, [and] I recognize that your position is difficult because you don’t know exactly what you have to defend yourself against...

Even in its final *Report*, the Committee remain evasive about the standards it had actually employed, observing at p. 10 that, “Our assessment has...been informed by [the *AHA Statement on Standards*], though we have made no decisions based solely upon it.” Six months later still, during the P&T review hearings, not a single member of the Investigative Committee could explain exactly what standards *had* been applied (nor could a consulting “expert on academic standards” hired by the University to fill in the gaps). To all appearances, then, the Committee simply invented “rules” as they went along, pretending all the while that the opposite was true.

“facts” upon which his findings were predicated.¹³⁰ All told, the pattern of deliberate distortion is once again so pronounced, and undertaken so systematically, that it is impossible to avoid the conclusion that he and his coauthors were engaged in outright research fraud.¹³¹ The gravity of the situation thus presented is aptly conveyed in an observation by the SCRM, quoted by University of Colorado President Hank Brown on May 25, 2007: “The nature of the offenses here involving...falsification and fabrication goes to the heart of the academic enterprise and undermines public faith in the University of Colorado and universities more generally.”¹³²

Allegation 6: Falsification (*re*, on the matter of “circumstantial evidence”)

At p. 33n53 of the *Report*, Professor Radelet explains the manner in which the term “circumstantial evidence” should be understood for purposes of assessing my use of it as regards the relationship of John Smith to the smallpox epidemic suffered by the Wampanoags.

Circumstantial evidence is usually contrasted with “direct evidence,” which is a statement by a witness who heard or saw something. According to *Black’s Law Dictionary*, circumstantial evidence is “Testimony not based on actual personal knowledge or observation of the facts in controversy, but other facts from which deductions are drawn, showing indirectly the facts sought to be proved” (5th edition, 1980, p. 221). Thus circumstantial evidence involves putting together chain links to infer a possible or plausible conclusion. As such, the more implausible the conclusion the more circumstantial evidence is needed to buttress it.

Once again, serious problems present themselves. These begin with the self-evident fact that, even using a proper legal definition, any given piece of circumstantial evidence remains circumstantial evidence, *irrespective* of whether it is linked to other pieces, and even if no other pieces exist. Thus, the term “circumstantial evidence” does *not* by definition “involve putting together chain links,” as Professor Radelet asserts. It is only in the *employment* of such evidence that “putting together chain links”—or “connecting dots,” as I’ve been known to describe it—comes into play (i.e., the evidence exists, independent of its usage). On this score, however, it cannot be shown that I’ve ever claimed to have “proved” anything in the relevant connection. *Had* I done so, moreover, it would have been in the context of historical analysis, an arena in which the definitional requirements attending legal procedure are plainly irrelevant.

What *was*—and remains—relevant is the way in which the term at issue, and evidence itself, is employed within the scholarly arena in which my work is performed. As has been explained elsewhere, “Ideas often emerge out of confluences of circumstantial evidence in the minds of those steeped in a problem, a discourse or technology. Often those individuals have a feeling, a hunch that this way or that way is the right way to proceed, without being able to [fully] articulate its evidential providence.”¹³³ This is perfectly consistent with the observation of historian Peter

¹³⁰ This matter is discussed in Note 101, above. Also see Notes 121 and 126.

¹³¹ “The use of such language as ‘the Committee finds’ or ‘we conclude’ in all parts of this document prior to the last page...represents a unanimous finding of the Committee”; *Report*, p. 10.

¹³² Brown to Patricia “Pat” Hayes, Chair, Board of Regents of the University of Colorado (May 25, 2007) p. 9; quoting “Report and Recommendations of the Standing Committee on Research Misconduct Concerning Allegations of Research Misconduct by Professor Ward Churchill” (June 13, 2006). It should be mentioned that, in making this observation, Pres. Brown—having disagreed with the findings of the subsequent P&T Review Panel—was explicitly relying on the Investigative Committee’s *Report*.

¹³³ Richard Pring and Gary Thomas, *Evidence-based Practice in Education: Conducting Educational Research* (New York: Open University Press, 2004) p. 2. This very principle was discussed during a colloquy; *Investigative Committee Transcript* (Apr. 16, 2006) pp. 163-5.

Novick, that the evidence leading an historical researcher to arrive at particular interpretation or conclusion is seldom deployed in full, and often simply not fully deployable.

The footnote has various functions, rhetorical and others, but...it only weakly assimilates historical scholarship to the scientific norm of replicability [i.e., “proof”].... [W]hen citations [are] illustrative of a synthetic interpretation arrived at through “deep immersion,” even the demonstration that several citations are faulty is far from constituting a refutation of the thesis they underpin, just as their verification does little to sustain it.¹³⁴

The principle at issue is especially true in a discipline such as mine, wherein explicitly “decolonizing” methodologies are involved. In this regard, since none of the Committee members were familiar with the methods at issue, I provided an entire book on the topic at the request of Professor McIntosh (and would have provided more such material, had it been requested).¹³⁵ Put succinctly, the context is in part that:

As in any emancipatory project, deconstruction and linguistic analysis of dominant texts and discourse is a key tool [in American Indian Studies]. Because the relative historical absence/silence of natives, women and subalterns make ethnographic, feminist and social historians necessarily reliant on alien, hostile, or indifferent texts which require critical reading, across the grain of their often repugnant politics and morality.¹³⁶

For this reason, to offer a politically *conservative* explanation of accepted analytical procedure, “when scholars are attempting to challenge established historical beliefs about topics for which evidence is limited, ‘The aim is to convince without being able to demonstrate. In the circumstances, disputants are forced to take liberties with the evidence and presentation, for to main-

¹³⁴ Novick, *Noble Dream*, p. 220. This passage from Prof. Novick was repeatedly quoted and otherwise brought to the Committee’s attention (see, e.g., “Unfinished Responses,” p. 7). Yet is neither quoted nor cited in the *Report*.

¹³⁵ The book in question is Linda Tuhiwai Smith, *Decolonizing Methodologies: Research and Indigenous Peoples* (London/Dunedin: Zed Press/University of Otago Press, 1999). Prof. McIntosh actually cites pp. 34-5 and 137-40 of Smith’s material at p. 46n100 of the *Report*, indicating familiarity with at least those pages of it. My own exposition on method, which will be found at pp. 2-4 and 7-9 of my “Unfinished Responses,” is, however, neither quoted nor cited. Nor are any of the additional materials referenced therein. These included Kay Boyle, *The Long Walk at San Francisco State and Other Essays* (New York: Evergreen, 1970); August Meier and Elliott Rudwick, *Black History and the Historical Profession. 1915-1980* (Urbana: University of Illinois Press, 1986) esp. pp. 177-229; Vine Deloria, Jr., *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact* (New York: Scribner’s, 1995); Thomas Biolsi and Larry J. Zimmerman, eds., *Indians and Anthropologists: Vine Deloria Jr. and the Critique of Anthropology* (Tucson: University of Arizona Press, 1997) esp. Herbert T. Hoover’s essay, “Vine Deloria, Jr., in American Historiography” (pp. 27-34); Devon Mihesuah, ed., *Natives and Academics: Researching and Writing About American Indians* (Lincoln: University of Nebraska Press, 1998) esp. Donald Fixico’s essay, “Ethics in Writing American Indian History” (pp. 84-99); Rodolfo Acuña, *Sometimes There is No Other Side: Chicanos and the Myth of Equality* (South Bend, IN: University of Notre Dame Press, 1998); Immanuel Wallerstein, *Unthinking Social Science: The Limits of Nineteenth Century Paradigms* (Philadelphia: Temple University Press, 2001); Donald Fixico, *The American Indian Mind in a Linear World: American Indian Studies and Traditional Knowledge* (Routledge, 2003); Brian Baker, Boatano Mosupyoe, Robert Munoz, Jr., Wayne Maeda, Eric Verga, and Gregory Mark, eds., *Introduction to Ethnic Studies* (Dubuque, IO: Kendall-Hunt, 2004); Clara Sue Kidwell and Alan Velie, *Native American Studies* (Lincoln: University of Nebraska Press, 2005) esp. pp. 54-5. There is therefore little indication that the Committee members actually attempted to overcome their collective lack of competence in Ethnic Studies or, more specifically, American Indian Studies methodologies—as opposed to those pertaining to literature (Prof. Limón) and federal Indian law (Prof. Clinton)—when assessing the integrity of my work.

¹³⁶ Bonwen Douglas, *Across the Great Divide: Journeys in History and Anthropology* (New York: Routledge, 1998) p. 178. Much the same point is made throughout the relevant literature. See, as examples, Robert Young, *White Mythologies: Writing History and the West* (New York: Routledge, 1990); Robert I. Simon, *Teaching Against the Grain: Texts for a Pedagogy of Possibility* (New York: Bergin and Garvey, 1992); Henry A. Giroux and Peter McLaren, eds., *Between Borders: Pedagogy and the Politics of Cultural Studies* (New York: Routledge, 1994); Peter McLaren, ed., *Revolutionary Multiculturalism: Pedagogies of Dissent for the New Millennium* (Boulder, CO: Westview Press, 1997).

tain rigorous standards would be to abandon the contest as unwinnable.”¹³⁷ I myself, following Novick, argued repeatedly—and, I think, demonstrated—that rather than a relatively high degree of “rigorousness” in the application of evidentiary standards to “established” historical scholarship, there is something far more akin a collective *pretense* that this is so.¹³⁸ Indeed, I quoted the rather establishment-oriented historian Peter Charles Hoffer, a member of the AHA Professional Division, to the effect that “the fabrications [and] falsehoods” infesting “established historical belief” has “actually immunized it from criticism in elite and learned circles.”¹³⁹

Overall, there can be no serious question as to whether Professor Radelet and his coauthors were aware of the points just raised—they were, and were in fact obliged to be—but that they actively suppressed the record of direct evidence that would have served to illuminate them in the *Report*. Not only is the relevant transcript of oral testimony received by the Investigative Committee not rendered accessible for inspection by readers—as required under the AHA’s Statement on Professional Standards—but the relevant testimony is not so much as mentioned. Yet, on April 1, 2006, the record reflects the following exchange between Professor Radelet and I.

PROFESSOR CHURCHILL: [With respect to John Smith’s role in sparking the epidemic, the evidence is] not conclusive. It’s circumstantial evidence. Is it strong? Well, that’s subjective. I consider it strong...

DR. RADELET: Okay. So, you know, where we’re going with this is, you know, I’ve got the *Black’s Law Dictionary* definition of circumstantial evidence and, you know, all this kind of stuff that...

PROFESSOR CHURCHILL: Well, I wasn’t writing a legal piece here.¹⁴⁰

This is followed, a few pages later by a more extensive exchange.

DR. RADELET: ...the question is more about your writing style, your rhetorical style.

PROFESSOR CHURCHILL: Well, here’s the simple answer[: Y]ou just explained how you think I should have said [what I said, but] that’s what I thought I said. I mean, that’s the implication of the term to me.... I think the notion of circumstantial evidence in historical analysis [is different from that pertaining in] legal analysis, [where] you’ve got real well defined connotations to terms. *Black’s Law Dictionary*, if I were writing a legal treatise, I suppose I might have looked there, but I’m using it in a more colloquial way. Circumstantial evidence means that there’s indication thereof, not conclusive proof. That’s...how I intended it...

DR. RADELET: Yeah. I understand there’s different types of scholarships. The type of writing you do is different than—you know, if I publish in a law review, or if Mimi [Wesson] or Bob [Clinton] does, we do very different stuff than for an historical journal, and I understand that

¹³⁷ Report, pp. 45-6n98; quoting David Henige, *Numbers From Nowhere: The American Indian Contact Population Debate* (Norman: University of Oklahoma Press, 1998) p. 8. The Committee observes that in my “Submission C”—i.e., “A Few Thoughts on Sources and Interpretation”—I quoted the same passage from Henige “approvingly.” It is then stated that the “Committee does not agree” with this approach. I submit that the Committee’s opinion on the matter was/is utterly irrelevant to the *only* questions before it, i.e.: whether the practice at issue is accepted within sectors of the scholarly community cognate to my own discipline, and whether I adhered to it in my work. The answers are that it is—as Henige demonstrates rather exhaustively—and I did. The Committee enjoyed no prerogative whatsoever to simply disregard the methods/standards prevailing in my field in favor of practices and procedures they themselves prefer.

¹³⁸ See esp. Novick’s handling of the David Abraham case in *Noble Dream*, at pp. 612-21. This is covered in my “Thoughts on Sources and Interpretation” (pp. 12-4), and discussed during a colloquy; Investigative Committee Transcript (Apr. 16, 2006) pp. 151-2. The Committee makes no mention of such matters in its *Report*, however.

¹³⁹ Peter Charles Hoffer, *Past Imperfect: Facts, Fictions, Fraud—American History from Bancroft and Parkman to Ambrose, Bellesiles, Ellis, and Goodwin* (New York: PublicAffairs, 2004) p. 30. The quote from Hoffer, paraphrased at various points in my colloquies with the Committee, is quoted verbatim on p. 28 of my “Unfinished Responses.”

¹⁴⁰ *Investigative Committee Transcript* (Apr. 1, 2006) pp. 158-9.

there's different ways of coming at [a] conclusion. And your stuff is more...putting it together, connecting dots, and in the bigger scheme of things, the John Smith thing is not a particularly big dot...

PROFESSOR CHURCHILL: ...There might have been a better way to get the point across, but circumstantial to me is the qualifier. I'm not accusing [Smith] of doing it. I say I suspect him, basically. Here's the evidence that causes the suspicion, but that's all it is[:] a suspicion.¹⁴¹

There follows several further pages of increasingly detailed discussion of the matter before Professor Radelet effectively wrapped up the exchange by observing that, "I guess we can chalk it up just the matters of phrasing."¹⁴² To be sure, Professor Radelet and his coauthors are of course entitled to disagree with my "rhetorical style" and "phrasing." They may even have been within bounds when they presumed to explain—quite inaccurately—how "most people" would have framed things, i.e.: "...most people would have said, you know, while the weight of the evidence suggests that the French did it, you know, another theory might be, you know, blah, blah, blah."¹⁴³ What they were *not* entitled to do was to simply ignore my differing appreciation of the term "circumstantial evidence," withhold all mention of the fact that I'd explained my understanding/usage at length, thereby conjuring the false impression both that their preferred usage the was uncontested and, by implication, that the standards/protocols of legal rather than historical scholarship might be seen as applying to the aspect of my work presently at issue.¹⁴⁴

Only through such manipulation of the available evidence could differences over what Professor Radelet himself openly acknowledged as being "matters of phrasing" and "rhetorical style" be (re)cast as "research misconduct" on my part. This constitutes a form of falsification under the "omitting data" provision of the National Science Foundation standards set forth in the Code of Federal Regulations at 45 CFR, § 689.1(a)(2),¹⁴⁵ applicable under the University of Colorado's own rules to *all* faculty members "on all campuses who are engaged in research under different circumstances, regardless of whether or not it is in the field of science."¹⁴⁶ As always, by virtue of

¹⁴¹ Ibid., pp. 167-9 (extraneous dialogue omitted).

¹⁴² Ibid., p. 173.

¹⁴³ Ibid. As was demonstrated rather thoroughly in Allegation 3, "most people" not only would not, but *have* not, framed things in this manner. This is most especially true with respect to Prof. Radelet's proposition that "the weight of evidence suggests the French did it," an hypothesis for which—as was discussed in Note 67, above—a paucity of evidence has been deployed, and which has *not* been endorsed by the great majority of scholars working in the field. A further question is how, exactly, Prof. Radelet thought all the "blah, blah, blah" he was suggesting I should have presented might have been fit into the *two sentences* which comprise my sole reference to the John Smith/smallpox premise, a remark made in passing within a much broader narrative with regard to what even Prof. Radelet described as a "small dot...within the bigger scheme of things." Alternatively, the question becomes the extent of explanatory annotation might reasonably expected be to attend any/every such passing remark. In this instance, my 36-page narrative text was already attended by 180 endnotes encumbering a further 14 pages. It's worth mentioning in this connection that I'm not infrequently criticized for "*overannotating*" my material (even to point of being "anal" about it).

¹⁴⁴ In his testimony before the P&T Review Panel, Prof. Radelet acknowledged that he did in fact recall our colloquy on the meaning of the term "circumstantial evidence" during the investigative proceedings, that he was/is aware that the term holds colloquial as well a technical meanings, and that he never bothered to consult such more generalized references as *Miriam Webster's Dictionary*—"I'm not sure if circumstantial evidence is defined in those sources"—before imposing the definition deployed in *Black's Law Dictionary* upon my use of it in what he admittedly understood to be a context *other than* legal scholarship; *P&T Review Transcript* (Jan. 21, 2007) pp. 2139-41.

¹⁴⁵ The relevant passage reads in full, "Falsification means manipulating research materials, equipment, or processes, or changing or omitting data or results such that the research is not accurately represented in the research record" (see Note 1, above, for access information).

¹⁴⁶ *Administrative Policy Statement Concerning Misconduct in Research and Authorship* (see Note 1, above, for access information).

their unanimous endorsement of Professor Radelet's findings, his four coauthors share in his misconduct.¹⁴⁷

Allegation 7: Falsification
(re, the matter of Captain John Smith)

At page 36 of the *Report*, Professor Radelet states that “Professor Churchill does not connect the dots in his proposed set of circumstantial evidence, describing the chain of events that might have served to connect [John] Smith to the [1616-20] epidemic.” This would perhaps be an accurate assessment, *if* I had at any point asserted that Smith had *in fact* “deliberately introduced smallpox among the Wampanoags,” i.e.: that this had been *proven*. All I ever wrote, however, is there is “some pretty strong circumstantial evidence” to that effect.¹⁴⁸ In other words, I indicated that sufficient evidence exists to raise suspicions in this regard—i.e., pointing to the possibility of Smith’s connection—a matter which might reasonably be construed as casting doubt upon comfortable assumptions constituting “consensus” understandings of how the epidemic originated, and underscoring the need for further research on the topic.

This, it seems to me, is *precisely* the purpose to be served by critical inquiry/scholarship of the sort I was hired, tenured, and promoted by the University of Colorado for pursuing. Further, my use of circumstantial evidence, *not* to “prove a case,” but rather to cast doubt upon what might well be accepted as “proven fact” by pointing out alternative possibilities, conforms perfectly well even to the interpretation of *Black’s* legal definition advanced—indeed, imposed—by Professor Radelet, i.e.: “*to infer a possible or plausible conclusion* [my emphasis].” To make it appear that my evidentiary burden could not be met even to this extent, it was necessary for Professor Radelet to assert that “no evidence—not even circumstantial evidence—supports” my “speculation.”¹⁴⁹ This, in turn, required not only that he engage in the various falsifications already addressed in this complaint, but repeat these offenses on the “John Smith Question” itself.

The crux of Professor Radelet’s stated position is the “bewildering” fact that, although Captain Smith “left New England in 1614 and never returned,” the epidemic did not break out until 1616, an interval of “eighteen or more months.”¹⁵⁰ “We have found no evidence,” he sums up, “that the time between exposure and the outbreak of the epidemic could have been so long.”¹⁵¹ This framing insinuates a pair of *very* important fallacies: 1) That the only manner in which the Wampanoags could have been infected with smallpox was by direct exposure to someone either ill with or at least carrying the disease, and, therefore, that 2) for me to have legitimately raised the possibility that Smith was responsible for introducing the disease, the outbreak would have had to have occurred either while he was present or at least within two weeks of his departure.¹⁵²

There can be no question but that Professor Radelet knew full-well that the first of these two contentions was false. During their testimonies on February 18, 2006, Dr. Michael Trimble and

¹⁴⁷ “The use of such language as ‘the Committee finds’ or ‘we conclude’ in all parts of this document prior to the last page...represents a unanimous finding of the Committee”; *Report*, p. 10.

¹⁴⁸ “An American Holocaust?,” p. 54.

¹⁴⁹ *Report*, pp. 38, 36.

¹⁵⁰ *Ibid.*, p. 36; quoting/citing Salisbury, *Manitou and Providence*, pp. 76, 101.

¹⁵¹ *Report*, p. 36.

¹⁵² At p. 36n87, to establish this interval, Prof. Radelet quotes p. 37 of Clyde Dollar’s “The High Plains Smallpox Epidemic of 1837-38,” *Western Historical Quarterly*, No. 8 (1977): “...the incubation period (from exposure to the virus until the first symptoms appear) varies from eight to fourteen days.” Without providing direct citation of the *Investigative Committee Transcript*, Prof. Radelet also observes that, “On February 18, 2006, Mark J. Timbrook of Minot State University told our Committee that the incubation period for smallpox is usually 12-14 days.”

Mr. Mark J. Timbrook, witnesses selected by the Investigative Committee itself on the basis of their mutual expertise concerning the epidemiology of smallpox among American Indians in “frontier” settings, each devoted considerable time to discussing how the disease was sometimes transmitted to Indians—in at least one case deliberately—by “formites,” i.e., pus and scabrous matter contaminating bedding, garments, and so on used by persons suffering active cases of smallpox.¹⁵³ Both witnesses, Dr. Trimble quite graphically, also described a method of immunization known as “variolaion.”

They would literally take the crusts, the scabs as the scabs fall off the smallpox blisters. They literally call it harvesting the scabs. They would take them as they would fall off from people, people that have survived, and they have an attenuated form of smallpox in them, and you could store these things in bottles. And then what you would do is crush them up, grind it into a powder, and you’d scratch somebody’s arm, and you’d literally rub [powdered smallpox scabs] into the open wound, and that’s called variolaion....¹⁵⁴

Generally speaking, formites contain the smallpox virus in an active—i.e., unattenuated—form, while the matter used for purposes of variolaion has become weakened or inactive, and is thus much reduced in virulence. Whether attenuated or not, such matter can be—and was—stored in sealed containers, usually vials or small bottles, and, if stored away from direct exposure to sunlight, retains its potency for periods extending up to several years.¹⁵⁵ Even when not “harvested” and stored in this manner, moreover, formites contained in blankets and other such items remain infectious for years.¹⁵⁶ While variolaion was not “officially” adopted in English medical practice until the early-eighteenth century,¹⁵⁷ the principle appears to have been understood—and

¹⁵³ *Investigative Committee Transcript* (Feb. 18, 2006): Trimble testimony at pp. 8-11; Timbrook testimony at pp. 117-20, 122-3, 129-30, 213. For corroboration, see Shurkin, *Invisible Fire*, p. 187; Hopkins, *Princes and Peasants*, pp. 114-5, 179, 189, 273; Ann F. Ramenofsky, *Vectors of Death: The Archaeology of European Contact* (Albuquerque: University of New Mexico Press, 1987) pp. 146-8

¹⁵⁴ Trimble testimony, *Investigative Committee Transcript* (Feb. 18, 2006) p. 14. Surprisingly, Mr. Timbrook, when purportedly explaining how smallpox “vaccine” was made during the early 19th century, described the variolaion technique of collecting pus from those stricken with the disease; *Investigative Committee Transcript* (Feb. 18, 2006) p. 194. At the time, smallpox vaccine was not derived from smallpox matter, but rather from that of cowpox; see generally, Robertson, *Rotting Face*, pp. 55-7; citing Frederick F. Cartwright and Michael D. Bidiss, *Disease and History* (New York: Thomas Y. Crowell, 1972) pp. 126-8.

¹⁵⁵ The Chinese were using this method of storage at least as early as the 15th century; Hopkins, *Princes and Peasants*, pp. 114-5. A typical estimate of shelf-life is “eighteen to twenty-four months”; Robertson, *Rotting Face*, p. 124.

¹⁵⁶ Ramenofsky, *Vectors of Death*, p. 146; citing A.W. Downie, “Smallpox,” in S. Redson, A.W. Downie, F.O. MacCollum, and C.H. Stuart-Harris, eds., *Viral and Rickettsial Diseases of Man* (London: Edward Arnold, 1967) pp. 84-111; S. Upham, “Smallpox and Climate in the American Southwest,” *American Anthropologist*, Vol. 88 (1986) pp. 115-28.

¹⁵⁷ See generally, Clifford D. Conner, *A People’s History of Science: Miners, Midwives, and “Low Mechanics”* (New York: Nation Books, 2005) pp. 101-6.

widely applied—by laypeople “for centuries.”¹⁵⁸ Such understanding would *necessarily* have pertained to the transmission by formites of full-blown smallpox as well.¹⁵⁹

It follows, since the means of doing so months or even *years* after the fact were both understood and available, that smallpox need not have broken out within two weeks of Smith’s departure for him to be reasonably suspected of having “deliberately introduced” it. Several methods of accomplishing this present themselves, the simplest being for Smith to have left a “stay behind” party with instructions to use infect the Wampanoags at some convenient moment (using matter carried from England in vials to infest blankets, garments, or other such items, then traded or distributed as gifts, for instance¹⁶⁰). And, as Salisbury observes, Captain Smith *did* in fact “leave part of his crew behind under Thomas Hunt,” ostensibly to “finish up fishing operations and carry the catch to [the Spanish port city of] Malaga” (albeit Hunt’s *real* “catch” seems to have consisted of a group of “about twenty” Patuxet and seven Nauset Wampanoags, to be sold as slaves).¹⁶¹

The timing of Hunt’s departure is a bit murky, since Squanto, one of the Patuxets Hunt tried unsuccessfully to sell in Malaga, is first recorded as appearing in London in 1617.¹⁶² This, of course, leaves open the prospect that Hunt was originally assigned to remain in Cape Cod until Smith’s scheduled return in 1615,¹⁶³ and that he may not actually have left until some point in

¹⁵⁸ In Danish “physician Thomas Bartholin wrote in 1675 that peasants in his country were practicing variolation. Soon physicians in England were reporting that it had been going on there for centuries. It was an ancient custom, according to Dr. Perrot Williams in the Royal Society’s *Philosophical Transactions* in 1722”; Shurkin, *Invisible Fire*, p. 121. Other sources indicate that Bartholin’s first description of the Danish practice was published in 1666, and that variolation was reported to have long-since become a “popular folk practice” among the peasants of Poland, Greece, and elsewhere in Europe, at about the same time; Hopkins, *Princes and Peasants*, p. 46; citing G. Miller, *The Adoption of Inoculation for Smallpox in England and France* (Philadelphia: University of Pennsylvania Press, 1957); C.W. Dixon, *Smallpox* (London: Churchill, 1962); C. Kahn, “History of Smallpox and Its Prevention,” *American Journal of Disabled Children*, No. 107 (1963) pp. 597-609; F. Henschen, *The History and Geography of Disease* (New York: Delacorte Press, 1966). Hopkins provides no pinpoint citations.

¹⁵⁹ Such understanding would obviously be essential to knowing which phase of the illness was appropriate to the harvesting of scabs and other matter for purposes of variolation. There is also evidence that Europeans had been using disease as a weapon—i.e., deliberately transmitting it—long before 1616. See, e.g., Adrienne Mayor, *Greek Fire, Poison Arrows and Scorpion Bombs: Biological and Chemical Warfare in the Ancient World* (New York: Woodstock/Peter Mayor, 2003); esp. pp. 132, 136, 139-41; Mark Wheelis, “Biological Warfare in medieval and Renaissance Siege Warfare,” in Erhart Geissler and John Ellis van Courtland Moon, eds., *Biological and Toxin Weapons: Research, Development and Use from the Middle Ages to 1945* (New York: Oxford University Press, 1999) pp. 10-6.

¹⁶⁰ The earliest account in *American Indian* tradition of smallpox matter being used in this way dates from 1493, and was first committed to writing in 1613; Ronald Wright, *Stolen Continents: The Americas through Indian Eyes since 1492* (Boston: Houghton-Mifflin, 1992) pp. 73-4. While the perpetrators would in this instance have been Spanish rather than English, as Salisbury, following Edmund S. Morgan, observes, “Smith’s model in handling Indians was Cortez,” a matter suggesting that he was strongly influenced by—and thus knowledgeable of—Spanish practice over the preceding 110 years; Salisbury, *Manitou and Providence*, p. 99; citing Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York: W.W. Norton, 1975) p. 77.

¹⁶¹ Salisbury, *Manitou and Providence*, p. 101. Why, exactly, the seafaring Spanish might have been interested in buying *fish* from their English competitors is left unexplained in this and other accounts. Suffice it here to say that the proposition seems rather dubious on its face, and thus smacks of a cover story, designed to conceal the Smith expedition’s actual agenda, i.e.: “exploring, mapping, [and] colonizing” (Salisbury himself suggests this at p. 97). Assuming that the original explanations were deliberately misleading—as there seems ample reason to do in this instance—of course opens up many interpretative possibilities.

¹⁶² Salisbury, *Manitou and Providence*, p. 107.

¹⁶³ Smith’s return to England in 1614 was undertaken for purposes of securing the financial backing necessary to organize a more substantial force with which to return to Cape Cod the following year, commencing in earnest the process of English conquest and colonization. To this end, he entered into a collaboration with Sir Ferdinando Gorges during the winter of 1614-15, and by “summer they were ready to sail.” The “expedition fell apart,” however, when Smith’s was waylaid by French privateers and the Captain himself “remained a French prisoner for the rest of the year”; Salisbury, *Manitou and Providence*, pp. 98, 101; citing Arber, *Smith’s Travels and Works*, Vol. 1, pp. 193, 218; Philip L. Barbour, *The Three Worlds of Captain John Smith* (New York: Macmillan, 1964) pp. 315-24.

1616.¹⁶⁴ Such ambiguities aside, it is certain that an expedition under Captain Richard Vines was dispatched to the area in 1616 by Smith and Sir Ferdinando Gorges, who had by then become partners in the colonizing enterprise.¹⁶⁵ So much for the supposed interval separating “Smith”—i.e., subordinates/men acting on his orders—from the outbreak of smallpox among the Wampanoags, about which Professor Radelet professed such “bewilderment.” In actuality, no such time-lapse occurred.

Of at least equal significance is the fact that Smith’s own writings demonstrate quite clearly that he left “New England,” as he himself had named the area,¹⁶⁶ fully expecting that the Wampanoag population would shortly undergo a drastic decline. In writings he began to circulate almost immediately upon his return to England in 1614, he described “the coast from Massachusetts Bay...to Plymouth Bay” as being the “Paradise of all those parts,” given that it was “under almost complete cultivation and habitation.”¹⁶⁷

[S]o planted with Gardens and Corne fields, and so well inhabited with a goodly, strong and well proportion people [is the area,] who can but approve this a most excellent place, both for health and fertility? ... [C]ould I have but meanes to transport a Colonie, I would rather live here then [sic] any where.¹⁶⁸

Yet, in the same breath, Smith assured prospective colonists that upon arrival in the area, they would find that it was “not inhabited,” i.e.: that they would be able, in effect, to simply walk in and take over fields which had already been cleared and the fertility of which had already been demonstrated.¹⁶⁹ Indeed, he reckoned that a military force of only “thirty to forty men” would be sufficient to overcome such native resistance as would likely be encountered during the establishment of the Plymouth Plantation squarely atop what at the time of his departure had been the most densely populated area in all of New England.¹⁷⁰ What to make of these incongruous elements of what by a reasonable definition must be considered *circumstantial evidence*? The answer was best framed, perhaps, by the noted historian Francis Jennings, a close student of the atti-

¹⁶⁴ Salisbury, like most scholars, accepts the premise that Hunt reached Malága by the end of 1614. The only “direct” source for this information I’ve encountered, however, is that cited by Salisbury at p. 269n47 of *Manitou and Providence*, i.e.: a single mention in Arber, *Smith’s Travels and Works*, Vol. 2, p. 933. Salisbury also cites Thomas Morton’s 1637 *New English Canaan* (New York: Franklin, 1967 reprint of 1883 ed.) pp. 130-2 and n., but Morton’s work obviously derives from Smith’s on this point. Absent corroboration, Smith’s material cannot—for a number of reasons—be considered reliable.

¹⁶⁵ Salisbury, *Manitou and Providence*, p. 108. On the Smith/Gorges partnership, see Notes 165, 176, and attendant text, above.

¹⁶⁶ Salisbury, *Manitou and Providence*, p. 98. Although the first *published* version of Smith’s material did not appear until 1616, portions of it was being circulated in manuscript form among potential funders virtually from the moment of his arrival in England (perhaps earlier).

¹⁶⁷ Salisbury, *Manitou and Providence*, p. 103; quoting/citing Arber, *Smith’s Travels and Works*, Vol. 1, p. 204.

¹⁶⁸ Arber, *Smith’s Travels and Works*, Vol. 2, pp. 891-2, 937; quoted in Salisbury, *Manitou and Providence*, p. 98.

¹⁶⁹ Arber, *Smith’s Travels and Works*, Vol. 2, p. 937; quoted in Salisbury, *Manitou and Providence*, p. 98. In the event, the Pilgrims proclaimed “the lack of Indians and the resulting vacant land as a gift from God. It was a view that other, later whites would also have as they too took up plows on Indian fields made empty by smallpox”; Robertson, *Rotting Face*, p. 106; citing Edward C. Rozwenc, *The Making of American Society*, 2 vols. (Boston: Allyn and Bacon, 1972) Vol. 1, pp. 26-8; Robert M. Utley and Wilcomb E. Washburn, *Indian Wars* (Boston: Houghton Mifflin, 1987) p. 33. Also see Steele, *Warpaths*, p. 84.

¹⁷⁰ Salisbury, *Manitou and Providence*, p. 100; citing Arber, *Smith’s Travels and Works*, Vol. 1, pp. 218-9. On population density and estimated decline in the 90th percentile, see, e.g., Alfred Crosby, “God...Would Destroy Them,” p. 40.

tudes and processes at work in the European “settlement” of North America: “Sometimes disease struck Indians [just a bit] too conveniently for an inquirer’s peace of mind.”¹⁷¹

Allegation 8: Falsification
(re, the question of intentionality)

Professor Jennings’ observation of course presumes the honesty and scholarly integrity of “inquirers,” qualifications which—given their performance, as described/documented in the preceding seven allegations—must be considered highly tentative where Professor Radelet and his coauthors are concerned. The basis for such an unfortunate conclusion is strengthened immensely when it is considered that there was yet another evidentiary element—this one of the “direct” variety—underpinning my suspicions about John Smith, that the Investigative Committee as a whole was certainly aware of it, but which they left *entirely* unmentioned in their *Report* (see again the “omitting data” provision contained in 45 CFR, § 689.1(a)(2)), quoted at Note 95, herein, and reflected in CU’s formal policy on research misconduct).

In my colloquy with Professor Radelet on April 1, 2006, I observed that in 1621 the earlier-mentioned Patuxet, Squanto, is recorded “in the literature” as having informed the survivors among his people that “the Englishmen” had deliberately infected them with the devastating “plague” they’d suffered between 1616 and 1620.¹⁷² Subsequently, in a written submission, I explained that this was originally chronicled by Governor William Bradford in his *Of Plymouth Plantation*,¹⁷³ and provided three pinpoint citations of its appearance in the contemporary literature,¹⁷⁴ leaving unmentioned a fourth such instance, in Salisbury’s *Manitou and Providence*, since Professor Radelet was purportedly engaged in a close reading of that book.¹⁷⁵

Squanto, it will be recalled, had ended up in London in 1617 as the result of Smith lieutenant Thomas Hunt’s slave-trading endeavor. In 1620, having already traversed the Atlantic aboard English ships on multiple occasions, Squanto returned to his home on a vessel commanded by Captain Thomas Dermer—also mentioned earlier—yet another employee of Smith’s partner, Ferdinando Gorges (which is in effect to say, by Smith himself).¹⁷⁶ Shortly thereafter, he began explaining to the surviving Wampanoags—his own village, Patuxet, which Smith had already renamed “Plymouth,” is recorded as having been completely “depopulated”¹⁷⁷—that “the settlers

¹⁷¹ Francis Jennings, *The Founders of America, from the Earliest Migrations to the Present* (New York: W.W. Norton, 1993) p. 347.

¹⁷² *Investigative Committee Transcript* (Apr. 1, 2006) p. 160.

¹⁷³ On this, I erred. Bradford, together with Edward Winslow, originally chronicled the matter in 1622, at pp. 53-9 of *A Relation or Journall of the English Plantation settled at Plimoth in New England* (also known as *Mourt’s Relation*). Two years later, Winslow recounted in again at pp. 5-11 of his *Good Newes from New England*. In any case, it comes up again at pp. 98-9 of Bradford’s *Of Plymouth Plantation*; see Karen Ordahl Kupperman, *Indians and English: Facing Off in Early America* (Ithaca, NY: Cornell University Press, 2000) p. 281n52.

¹⁷⁴ “Responses Left Unfinished,” p. 24; citing Robertson, *Rotting Face*, pp. 106-7; Kupperman, *Indians and English*, p. 191; and James Axtell, *The European and the Indian: Essays in the Ethnohistory of North America* (New York: Oxford University Press, 1981) pp. 252-3.

¹⁷⁵ Salisbury, *Manitou and Providence*, p. 269n47.

¹⁷⁶ Although the details are fuzzy, Squanto had gone from London to Newfoundland by 1618, “where he met Dermer, a former associate of John Smith. Dermer too Squanto [back] to England to meet Gorges,” who had entered into a “collaboration” with Smith to establish the Plymouth Plantation. “In March 1619 Dermer and Squanto set sail for the Patuxet region with yet another Gorges commission,” spent a period in Virginia, then “returned to southeastern New England in the summer of 1620.” Dermer was then fatally-wounded by a party of Wampanoags, his surviving crew escaped back to Virginia, and Squanto rejoined his people; Salisbury, *Manitou and Providence*, pp. 107, 98, 108.

¹⁷⁷ *Ibid.*, p. 109. At p. 108, Salisbury contends that Squanto was taken captive, apparently “for it was in this condition that he was living when the Plymouth settlers met him in March 1621.” This seems rather unlikely, given that Salisbury himself recounts at p. 107 how Squanto had accompanied Dermer’s expedition in order to “secure his passage home”

[had] manipulated the plague, recalling the devastation that had swept through the region of Plymouth's settlement" beginning in 1616.¹⁷⁸

Squanto also warned his countrymen that the Plymouth colonists "kept the plague buried in the ground, and could send it amongst whom they would,"¹⁷⁹ or, more precisely, that "the plague was buried under the storehouse [in Plymouth], 'which at our pleasure, we could send forth to what place or people we would and destroy them therewith, though we stirred not from home.'"¹⁸⁰ While Squanto's observations were much maligned by the colonists who initially recorded them, and have been routinely disparaged by mainstream historians ever since, they are/always were deserving of serious consideration for at least three reasons:

- 1) Squanto was unquestionably privy to unguarded conversations among the very Englishmen most likely to have known whether the Wampanoags had been deliberately infected in 1616.¹⁸¹
- 2) His description of the colonists' storing the "plague" underground is entirely consistent with the requirements of preserving smallpox matter discussed in Allegation 7.
- 3) There is indication that the colonists may ultimately have done exactly what Squanto warned they would do.

The latter point concerns the origins of the so-called Pequot War in 1633, when the Narragansetts supposedly "murdered" an English trader named John Oldham. In actuality, however, "Oldham was not murdered... [R]ather, he seems to have suffered a state execution by the verdict of a number of the Narragansett sachems," according to historian Francis Jennings.¹⁸²

In the fall of 1633 [Oldham] made the first recorded journey by an Englishman to the upper Connecticut, lodging at Indian villages all the way. [Smallpox] broke out soon afterward and ran rapidly through the tribes. The Narragansetts lost seven hundred of their people in the epidemic, which was believed by their sachem Canonicus to have been "sent" deliberately [via Oldham] by someone among the English.¹⁸³

and, once there, "took his leave in order to search for surviving Patuxets" in 1619. At p. 269n47, moreover, Salisbury observes that the only indication that Squanto might have been a "captive" appears at pp. 243-5 of Morton's *New England Canaan*—Bradford, for example, makes no mention of it—and that Morton "failed to distinguish [between] Squanto and Samoset," another Wampanoag. In other words, Salisbury relied on circumstantial evidence, unconfirmed by the documentary record, to draw what he saw as a reasonable conclusion (qualified in his main text only by the word "apparently"). Were we to adhere to the "standards" of assessment applied by Prof. Radelet to my two sentences, it would be necessary to accuse Prof. Salisbury of "fabrication" in this instance. For what seems likely to be a far more accurate characterization of the status enjoyed by both Squanto and Samoset at the time of the 1621 meeting, see Steel, *Warpaths*, p. 87; relying in part on George F. Willison, *Saints and Strangers: Being the Lives of the Pilgrim Fathers and Their Families* (New York: Reynal and Hitchcock, 1945) pp. 171-7.

¹⁷⁸ Kupperman, *Indians and English*, p. 191; citing Winslow, *Good Newes from New England*, pp. 5-11. Also see Salisbury, *Manitou and Providence*, p. 269n47; Robertson, *Rotting Face*, pp. 106-7.

¹⁷⁹ Axtell, *European and the Indian*, pp. 252-3; quoting Bradford, *Of Plymouth Plantation*, p. 99.

¹⁸⁰ Kupperman, *Indians and English*, p. 191; quoting Winslow, *Good Newes from New England*, pp. 5-11.

¹⁸¹ Even Salisbury concedes that, "Moving in the circles he did, Squanto undoubtedly knew something of the epidemic which ravaged the New England coast in his absence"; Salisbury, *Manitou and Providence*, p. 107.

¹⁸² Francis Jennings, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill: University of North Carolina Press, 1975). p. 208; relying primarily on John Winthrop's 1690 *History of New England from 1630 to 1649*, 2 vols. (Boston: Little, Brown, [2nd ed.] 1853) Vol. 1, pp. 191-2.

¹⁸³ Jennings, *Invasion of America*, p. 208.

Jennings remarks upon certain peculiarities attending the situation before concluding that the “case is unproven.”¹⁸⁴ In response, it should be observed that “the case” certainly appears to have been proven to the *Narragansetts*, whose views on the matter are by rights deserving of being accorded at least as much credence as that extended to those of the possible—or actual—perpetrators. So, too, the statements of Squanto, aptly described by several other scholars in a related research misconduct complaint lodged against the Investigative Committee on May 28, 2007, as conveying “the main *Wampanoag* understanding [about the 1616-20 epidemic] we have available to us [emphasis added].”¹⁸⁵ In substance, both the Narragansett example and that of Squanto, reflect elements of American Indian oral tradition.

At page 12 of its *Report*, the Investigative Committee as a whole claimed that while it had not “attempted to examine every possible work written of the topics in question,” it had in fact “examined the evidence relevant to [my] particular claims.” It further claims at page 12 that, finding itself “compelled...to consider questions of ‘truth,’ ways of knowing (including the oral traditions of native people), and racism,”¹⁸⁶ it had “examined...in some detail,” not only “the relevant primary sources [but] both oral and written traditions” as well. Finally, at page 46, it is asserted that the Committee members “believe all sources have value; we privilege none.”¹⁸⁷

Numerous bases upon which these self-assessments on the part of the Investigative Committee can be challenged or discredited have already been documented in this complaint. None are so blatant—or so offensive—however, as Professor Radelet’s first ignoring, then suppressing, the evidence embodied in Squanto’s statements and warnings. This is not simply a matter of “privileging” eurocentric sources/interpretations over those of native people/tradition, it is instead amounts to an active and outright *denial* of their very existence. Hence, of the several glaring falsifications already at issue in Professor Radelet’s and his coauthors’ representation of “evidence

¹⁸⁴ Ibid. Jennings points out that Oldham “was still in great favor with the Narragansetts shortly after the epidemic’s spread,” making it unlikely that he would have been held personally responsible unless the Indians received additional information. Jennings speculates that “a drunken Englishman”—he doesn’t say who—may have “blurted out a brag or threat,” thereby revealing what had occurred and Oldham’s role in it.

¹⁸⁵ Prof. James M. Craven, Prof. Michael Yellowbird, Prof. Haunani-Kay Trask, Prof. David E. Stannard, Prof. Ruth Hsu, Sharon H. Venne, and Jennifer Harbury, “Research Misconduct Complaint Concerning Investigative Committee Report of May 9, 2006” (submitted to the SCRM, May 28, 2007) p. 4.

¹⁸⁶ Pursuing “questions of ‘truth’” was, of course, not only beyond the scope of the Committee’s mandate, but an activity in which it repeatedly claimed it would not and *did* not engage. Witness the following exchange between my attorney, Mr. David Lane and Prof. McIntosh during the Investigative Committee’s proceedings on Feb. 18, 2006 (*Transcript*, pp. 72, 74):

MR. LANE: ...What I’m concerned about is echoing what I said at the outset, and that is [a recreation of the Scopes Monkey trial. We’re not here to determine what is the truth. We’re here to determine, did Professor Churchill commit fraud or misconduct in coming up with what he came up with? ...

DR. MCINTOSH: Speaking as an historian, I was rather surprised indeed that Mr. Lane used the word “truth,” even in quotation marks. *Truth is not a concept we historians feel is useful* [emphasis added].

After the fact, the Committee again “stressed” that “we were not charged with determining what actually happened in southern New England in 1614-1618 or at Fort Clark, North Dakota, in 1837”; *Report*, p. 12. Plainly, neither Prof. McIntosh’s explicit disavowal of “truth-seeking” objectives, nor the more Committee’s more carefully-worded reiteration in the *Report*, can be reconciled to the Committee’s admission, also at p. 12 of the *Report*, that it did in fact “consider questions of ‘truth.’” It is perhaps instructive that both instances in which the Committee was subsequently censured by the P&T review panel for having “exceeded its charge” in the latter respect appeared in the section of the *Report* written by Prof. McIntosh herself; see, University of Colorado, Faculty Senate Committee on Privilege and Tenure, *Panel Report Regarding Dismissal for Cause of Professor Ward Churchill and the Issue of Selective Enforcement* (Apr. 11, 2007; hereinafter, *P&T Report*) pp. 31, 52, 53.

¹⁸⁷ The falsity of this assertion is underscored by the P&T Review Panel’s finding that, “In deciding that certain references relied upon by Professor Churchill should not be included as legitimate sources, we think the Committee exceeded its charge”; *P&T Report*, p. 31.

relevant to my claim”—evidence they purported to have examined “in some detail”—this is, in my view, the most serious by far.

Allegation 9: Falsification
(re, the question of “native labor”)

At page 37 of the *Report*, having deleted Squanto’s accusations from consideration, Professor Radelet asserts that “evidence that Smith wanted to use Indians as a labor force contradicts Professor Churchill’s contention that [Smith] wanted to see them wiped out,” thereby concluding that I’d “fabricated” even the *possibility* that intentionality played a role in the 1616-20 epidemic. As always, there are several problems with Professor Radelet’s claim. Here, it is important to observe that the single sentence upon which he is forced of necessity to focus—the “contention” at issue appears nowhere else in my entire body of work—reads as follows, verbatim and in its entirety: “There’s some pretty strong circumstantial evidence that Smith introduced smallpox among the *Wampanoags* as a means of clearing the way for the invaders [emphasis added].”¹⁸⁸

Hence, it is readily-observable that Professor Radelet is once again conflating “Wampanoags” with the “Indians of New England” as a whole, thereby drastically distorting what I actually said/wrote.¹⁸⁹ This is of particular significance, since the manipulation allows Professor Radelet to anchor his conclusion upon a quote from Salisbury’s *Manitou and Providence*, observing that Smith “would use military repression to force the Indians to work for their colonial masters.”¹⁹⁰ The quote is accurate, but, stripped as it is of its context, is employed by Professor Radelet in a manner completely misrepresenting what was actually being said.

In the passage quoted, Salisbury was summarizing the relevant contents of Smith’s *Travels and Works*,¹⁹¹ a book Professor Radelet gives no indication of having examined at all (one can only wonder what he and his coauthors might have considered to be the “relevant sources” in this connection, i.e.: which/how many books comprised in the “pile” Professor Radelet testified to the P&T Review Panel he’d perused in the course of his “research” on the topic before him¹⁹²). Had he done so, it would have been readily-apparent that while Smith did indeed plan to use military force to impress “Indians” as slave laborers, there is *no* indication that by this he meant Wampanoags in particular.

On the other hand, Smith was unquestionably well aware from direct observation and experience that even if the Wampanoags were completely eradicated—thus effectively “clearing” the area in which he sought to see the Plymouth Plantation itself established of its indigenous population—there would still be plenty of *other* Indians along the Plantation’s periphery available to meet the colonists’ need for labor. In fact, the creation of exactly such an “Indian-free zone” within the Plantation itself would have been essential, not only in attracting settlers, but as a base or staging area for the sort of military operations Smith described. Such an interpretation is entirely consistent with existing evidence, e.g.: Smith’s earlier-mentioned “sales pitch” to potential

¹⁸⁸ “An American Holocaust?,” p. 54.

¹⁸⁹ See Allegation 3 in this complaint for examples of the same conflation being employed by Prof. Radelet for very similar purposes.

¹⁹⁰ *Report*, p. 37; quoting Salisbury, *Manitou and Providence*, p. 99.

¹⁹¹ At p. 267n27 of *Manitou and Providence*, Salisbury cites Volume 1 of Smith’s *Travels and Works* at pp. 191, 205, 218-9, and 262-4, as well as Volume 2 at pp. 925 and 955-6, as underpinning the paragraph in which the passage quoted by Prof. Radelet appears. In the *Report*, Prof. Radelet offers no hint that this was so.

¹⁹² See Note 73 and attendant text, above. In truth, Prof. Radelet’s annotation includes little to suggest that he actually read much of anything other than *Manitou and Providence*, and even that only partially.

settlers—and investors—indicating that the land and fields destined to become the Plymouth Plantation would be “vacant” when they arrived.¹⁹³

It in fact seems rather clear on the evidence that issuance of a Crown charter authorizing establishment of the Plymouth Plantation was contingent upon the Wampanoags’ elimination. As Salisbury recounts, although it remains unmentioned by Professor Radelet, the necessary patent was not forthcoming until after the arrival of communications from Captain Dermer verified “the utter weakness of the surviving coastal Indians, especially their vulnerability to European microbes and [military] power.”¹⁹⁴ Meanwhile, obviously confident both of the information and its effects, Gorges, who may—or may not—have ended his collaboration with Smith by that point—seems to have *already* dispatched the first boatload of colonists.

After a long delay, the charter was finally sealed on November 3, 1620. Eight days later, the Mayflower, carrying just over a hundred prospective settlers from England, arrived at Cape Cod. Though ostensibly bound for the Hudson River or some other location within the Virginia Company’s patent, their leaders were well aware of Gorges’s pending charter, and probably had his tacit approval to settle within his domain. They also carried a copy of Dermer’s last letter, dated just four months earlier, recommending the depopulated site of Squanto’s Patuxet, “which in Captain [John] Smith’s map is called Plymouth,” as the place to begin English colonization.¹⁹⁵

Once again, it is impossible to avoid the conclusion that Professor Radelet and his coauthors have deliberately manipulated the available evidence to create a false impression. Their action is especially egregious in this instance insofar as it appears to have been intended as a gesture linking their own line of argument to one which they were repeatedly informed has been employed with considerable regularity by neonazi holocaust deniers. Witness the following exchange which occurred during the proceedings on February 18, 2006, after one of the Committee’s expert witnesses, Dr. Michael Trimble, testified in another connections that American Indians would not have been intentionally exterminated because they constituted “a very cheap source of labor.”¹⁹⁶

PROFESSOR WESSON: ...Professor Churchill’s next question I will take the liberty of characterizing as rhetorical. I’m going to read it to you anyway for you to respond if you wish. His question is, you are aware, of course, that it was not in the interest of Germany to exterminate Jewish labor during the 1940s; is that correct?

DR. TRIMBLE: I’m not going to answer that.¹⁹⁷

Although my question was by no means “rhetorical,” I was precluded by the “rules,” which disallowed me from posing questions directly to witnesses at any point, from flowing up.¹⁹⁸ As

¹⁹³ For discussion and references, see Allegation 7.

¹⁹⁴ Salisbury, *Manitou and Providence*, p. 108; citing Bradford, *Of Plymouth Plantation*, pp. 81-3; Samuel Purchas, ed., *Hakluytus Posthumus, or Purchas His Pilgrims*, 20 vols. (Glasgow: James MacLehose and Sons, 1906) Vol. 19, p. 19; Richard Arthur Preston, *Gorges of Plymouth Fort* (Toronto: University of Toronto Press, 1953) pp. 163-4; James Phinney Baxter, ed., *Sir Ferdinand Gorges and His Province of Maine*, 3 vols. (Boston: Prince Society, 1890) Vol. 1, pp. 215-7, 219-22; Vol. 2, pp. 28-30.

¹⁹⁵ Salisbury, *Manitou and Providence*, p. 109; citing Bradford, *Of Plymouth Plantation*, p. 58-60, 81-2; Preston, *Gorges of Plymouth*, pp. 198-9; Charles M. Andrews, *The Colonial Period in American History*, 4 vols. (New Haven: Yale University Press, 1934-38) Vol. 1, pp. 262-4, 279-81; Ruth A. McIntyre, *Debts Hopeful and Desperate: Financing the Plymouth Colony* (Plymouth, MA: Plimouth Plantation, 1963) p. 21.

¹⁹⁶ Timbrook testimony, *Investigative Committee Transcript* (Feb. 18, 2006), p. 12.

¹⁹⁷ *Ibid.*, pp. 50-1. It should be noted that Prof. Wesson was indeed “taking liberties,” because my question was in no sense “rhetorical.” This sort of distortion, which happened repeatedly, accrued from the fact that

¹⁹⁸ Throughout the proceedings, I was prohibited from posing questions directly to witnesses (including my own). Instead, I was required to transmit them by email to Prof. Wesson, who was seated across the room, so that *she* could read them—sometimes in revised form—to witnesses. The obvious awkwardness of this procedure—remarked repeatedly in

was clarified in a written submission, had I been in a position to reframe my query—or, better yet, engage in colloquy with Dr. Trimble—I would have drawn his attention to Christopher Browning’s recent study on nazi policy towards Jewish labor,¹⁹⁹ as well as sources indicating that, far from “needing” the labor of the American Indians he was referring to, U.S. policymaker he determined that they had become “superfluous.”²⁰⁰ In this light, I explained that, whether he intended it or not, the “correspondence” between Dr. Trimble’s line of argument and that of several notorious Holocaust deniers “is quite clear.”

Consider, for instance, the summary by Colin Wilson of the premise advanced by Richard Harwood (Richard Verrall), author of the influential neonazi tract titled *Did Six Million Really Die?* ([Richmond, Surrey: Historical Review Press, 1974]): “What Harwood says, briefly, is that Hitler had no reason to murder Jews when he needed them for forced labor” (quoted in Deborah Lipstadt, *Denying the Holocaust*, p. 119). Identical “reasoning” is utilized by David Hoggan, a Harvard PhD who anonymously authored *The Myth of the Six Million* ([Tacoma Park, MD: Noon tide Press, 1978]). It also figures quite prominently in the work of other deniers, perhaps most prominently that of the American university professors Harry Elmer Barnes and Austin J. App.²⁰¹

Given these circumstances, that Professor Radelet would have opted to insert a sentence holding that my suspicions about John Smith are “contradicted” by his supposed desire to “use them as labor,” and that his coauthors would have approved his action in doing so, illuminates the mentality guiding their “investigation” of my work in a manner, and to a degree, I would not previously have believed possible.²⁰² Even were the argument not essentially extraneous to the main thrust of Professor Radelet’s fabrication of evidence against me, it would be grotesque. That he and his collaborators were compelled to engage in the sort of manipulation of the record in order to advance this gratuitous—and hideously racist—insult is simply unspeakable. Suffice it to say that unless the Committee’s misconduct is in this instance addressed in the most forceful possible manner, it will have effectively placed the University of Colorado, under whose imprimatur their material appears, on the same moral/intellectual footing as Verrall, Hoggan, Barnes, and App.

the record, not only by me but by several witnesses,—greatly impaired my ability to conduct effective witness examinations. Among other things, the fact that I am to all intents and purposes a “one-finger typist” made the reframing of questions and/or the posing of follow-up questions all but impossible. Prof. Wesson explained, also repeatedly, that the procedure was “required by the rules” (albeit I was never shown a copy), and that the Committee was as “uncomfortable” with the situation as I. There is no mention of the problem—to say nothing of my multiple objections—in the Committee’s *Report*, however.

¹⁹⁹ See my “Unfinished Responses,” p. 11. The reference is to Christopher R. Browning, *Nazi Policy, Jewish Workers, German Killers* (Cambridge, UK: Cambridge University Press, 2000).

²⁰⁰ “Unfinished Responses,” p. 11, referencing/quoting Robertson, *Rotting Face*, pp. 247, 286; Barton H. Barbour, *Fort Union and the Upper Missouri Fur Trade* (Norman: University of Oklahoma Press, 2001) p. 25.

²⁰¹ “Unfinished Responses,” p.11. In addition to the materials cited, see Austin J. App, *The Six Million Swindle: Black-mailing the German People for Hard Marks with Fabricated Corpses* (Tacoma Park, MD: St. Boniface Press, 1973) and Harry Elmer Barnes, *The Barnes Trilogy: Three Revisionist Booklets by Harry Elmer Barnes, Historian, Sociologist, Criminologist, Economist* (Torrance, CA: Institute for Historical Review, 1979).

²⁰² My impression in this regard was subsequently confirmed, however, by Professor Radelet’s having been recorded during the 2006 graduation celebration of the UCB Dept. of Sociology, of which he serves a chair, engaging in the “joke” of polling the crowd on how many of those assembled would be “in favor of gassing Ward Churchill?” Plainly, the level of “humor” involved is roughly equivalent to the neonazi query about how many Jews can be fit into a Volkswagen. Although there were to my knowledge several complaints, no adverse action appears to have been taken by the University, a matter suggesting that such behavior is not viewed as besmirching the institution’s reputation.